Answer to RC 1 :

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

Review of the manuscript cp-2021-119 "Influence of the choice of insolation forcing on the results of a conceptual glacial cycle model" by Gaëlle Leloup and Didier Paillard

The motivation for the research is well articulated. Indeed, the insolation amplitudes under different configurations of the orbital forcing may vary significantly enough to potentially influence our understanding of the climate history. To demonstrate the sensitivity of modeling results to the orbital forcing configuration, the authors offer a number of numerical experiments with a specific dynamical model.

We thank the reviewer for his positive appreciation of our objectives. Still, we want to stress that we are not investigating "different configurations of the orbital forcing" but different choices for the definition of "summer insolation". While the orbital forcing is well-prescribed by celestial mechanics, many different definitions of "summer" have been put forward in the context of the astronomical theory of Quaternary climates. These different choices are usually difficult to justify in the context of conceptual models and our objective is to investigate the robustness of our threshold-based model against these choices.

Unfortunately, the experimental set-up is not comprehensive enough and therefore the results are not conclusive.

The authors are a bit overoptimistic when they introduce their model as the one having only 5 governing parameters. In fact, in the presented set-up, the model has 13 governing parameters, specifically: I_0, V_0, τ_i, τ_g, τ_d , I_{41}, I_{23}, I_{22}, I_{19}, T_{41}, T_{23}, T_{22}, T_{19}

Here T_{41}, T_{23}, T_{22}, T_{19} are orbital periods (an index shows a numerical value in kyr) and I_{41}, I_{23}, I_{22}, I_{19} are corresponding insolation amplitudes. Only timescales and orbital periods have dimension of time, other parameters are adimensional.

If we select, let say, the obliquity period as a parameter with independent dimension, then, according to π-theorem, the amplitude of the 100-kyr system response V_{100} will be a function of 12 adimensional similarity parameters:

$V_{100} = \Phi(I_0, V_0, T_{41}/\tau_i, T_{41}/\tau_g, T_{41}/\tau_d, I_{41}, I_{23}, I_{22}, I_{19}, T_{41}/T_{23}, T_{41}/T_{22}, T_{41}/T_{19})$ (R1)

It would be physically reasonable to assume T_{41}/T_{23}, T_{41}/T_{22}, T_{41}/T_{19}, and I_0 to be constant and, using generalized π-theorem, to

migrate to 8 similarity parameters:

$V_{100} = \Phi(V_0, T_{41}/\tau_i, T_{41}/\tau_g, T_{41}/\tau_d, I_{41}, I_{23}, I_{22}, I_{19})$ (R2)

We thank the reviewer for suggesting the use of dimensional analysis and the π - theorem in our study. However, we disagree on the amount of parameters considered. Indeed, the orbital periods are fixed by celestial mechanics (Laskar et al, 2011), and therefore the orbital periods T_{41}, T_{23}, T_{22}, T_{19} cannot be considered as model parameters.

In the same manner, the corresponding insolation amplitudes I_{41}, I_{23}, I_{22}, I_{19}, depend on how we define the input summer forcing. For a given input forcing, these amplitudes are fixed. Furthemore, the input forcing (like the summer solstice for example), is not a linear sum of the different obliquity and precession components, and cannot be fully represented by four numbers like I_{41}, I_{23}, I_{22}, I_{19}.

We could have chosen for this study multiple possible input forcings with multiple combinations of amplitude of the obliquity or precession periods. However, in this manuscript, we choose to study the model's behavior for specifically four insolation forcings that have been used previously in the litterature as "the Milankovitch forcing" : the summer solstice insolation, the caloric season and the Integrated Summer Insolation (with the use of two different thresholds). Many other choices of "summer insolation" are certainly possible and are not restricted to a 2-parameter (precession, obliquity) or a 4-parameter (I_{41}, I_{23}, I_{22}, I_{19}) or a 8-parameter (I_{41}, I_{23}, I_{22}, I_{19}, ϕ_{41}, ϕ_{23}, ϕ_{22}, ϕ_{19}) space if ϕ is the phase. The key parameters involved in the definition of "summer insolation" are more likely to be the latitude (see response to RC4), the length of "summer" (caloric = 6 months, solstice = 1 day, above threshold = insolation dependent, …): they are involving some implicit modeling assumptions on the link between astronomy and the dynamics of ice-sheets.

The choice of the input insolation forcing is therefore not a parameter adjustment, but a modeling choice. In answer to a comment of RC2, we have proposed additional discussion of these modeling choices, with the following additions l. 26.

"In contrast, Milankovitch popularized the idea that the decisive element for glaciation was the presence of cold summers (Berger, 2021), due to reduced summer insolation, at latitudes typical of Northern Hemisphere ice sheets, 65° N. 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 mass balance (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."

The authors demonstrated that period-doubling bifurcation caused by rising deglaciation threshold V_0 is not very sensitive to the choice of I_{41}, I_{23}, I_{22}, I_{19}. Unfortunately, they stopped here, and, by avoiding variations of T_{41}/τ_i,T_{41}/τ_g, T_{41}/τ_d similarity parameters, they left readers with the impression that changing deglaciation threshold V_0 is the only option to reproduce the middle-Pleistocene transition.

In the manuscript, we do not aim at studying all the parameter evolutions that can lead to the MPT (change of a dominated 41 kyr record to a dominated 100 kyr record).
Our purpose is to study the evolution of the deglaciation threshold V_0 while other parameters are kept constant, and to see if it is possible to reproduce the geological record by varying solely this parameter.
We decided to focus on this parameter, as it has been identified as crucial by other previous studies (Parrenin and Paillard, 2003; Tzedakis et al 2017).
However, we cannot and do not exclude the fact that the MPT might be obtained by varying other model parameters. The study of all possible parameter modifications leading to the MPT is however out of the scope of this manuscript.
We fully agree that this point was not clearly stated in the first version of the manuscript. This was also noted by RC4 and we therefore propose significant modifications of the manuscript (this is the same answer as to the first general comment of RC4).

We propose the following modification of the abstract (l. 7) :

"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. We show that some features are robust. Specifically, to be able to reproduce the frequency shift over the Mid Pleistocene Transition, while having all other model parameters fixed, the deglaciation threshold needs to increase over time, independently of the summer insolation used as input."

We propose the following modification of the end of the introduction (l.61) :

"In particular, we are able to reproduce a switch from 41 kyr oscillations before the MPT to 100 kyr cycles afterwards in agreement with the record for all insolation forcings, by varying a single parameter, the deglaciation threshold V_0, and keeping all the other model parameters constant."

We propose the following modification of the conclusion (l.291)

"More specifically, 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 and keeping the other model parameters constant."

l.169, we propose to replace the sentence by "We suggest to replace the sentence by : "In order to study the evolution of the optimal deglaciation threshold V_0 over the Quaternary, it was divided into five 500 kyr periods."

l.274, we suggest to add an additional sentence :

"To model future natural evolutions of the climate system, possible evolutions of the V_0 threshold should be considered. However, we do not exclude the fact that variations of other parameters, that were kept constant in this study, could vary in the future. For instance, different I_0 thresholds have to be considered."

As Verbitsky et al (2018) and Verbitsky and Crucifix (2020, 2021) have demonstrated, the space of possibilities to produce a period-doubling bifurcation similar to the middle-Pleistocene transition is much wider and (if we continue to speak in terms of the model being reviewed) T_{41}/τ_i, T_{41}/τ_g, T_{41}/τ_d similarity parameters definitely have their roles in the drama. The authors' reasoning to keep τ_i, τ_g, τ_d constant because "these parameters gave correct behaviour in previous studies" is surprising – actually, their past experience with τ_i, τ_g, τ_d speaks about importance of these parameters for the system dynamics and therefore strongly advocates for them to be in the center of the study. Formally, making τ_i, τ_g, τ_d constant is equivalent to claiming complete similarity in parameters T_{41}/τ_i, T_{41}/τ_g, T_{41}/τ_d and excluding them from the solution (R2). Since T_{41}/τ_i, T_{41}/τ_g, T_{41}/τ_d reference values are of the same order of magnitude as V_0, there is absolutely no physical justification for such decision. Indeed, let us imagine that T_{41}/τ_i similarity parameter is changing in such way that it becomes a dominant similarity parameter relative to T_{41}/τ_g, T_{41}/τ_d. It means that equations (1d) and (1g) become identical, system (1) becomes linear and independent of V_0, and a period-doubling bifurcation is impossible:

$V_{100} = \Phi(T_{41}/\tau_i,\ I_{41}, I_{23}, I_{22}, I_{19})$ (R3)

Thus T_{41}/τ_i similarity parameter can control what the authors call the middle-Pleistocene transition, and the period-doubling bifurcation can be produced by slow change of T_{41}/τ_i similarity parameter from its relatively high values to its relatively low values under constant threshold V_0. The amplitude of the system response V_{100} will evolve from solution (R3) to solution (R2). Obviously, slow changes of T_{41}/τ_g, T_{41}/τ_d may also produce a period-doubling bifurcation.

In our model, the MPT is not linked to a period-doubling mechanism, but to a frequency- or phase-locking to various astronomical periodicities. Still, we thank the reviewer for mentioning the studies of Verbitsky et al (2018) and Verbitsky and Crucifix (2020, 2021), and we suggest to add a reference to the Verbitsky et al (2018) study in the introduction, l.39.

The new sentence reads :

"The 100 kyr cycles have been proposed to be linked to either eccentricity driven variations of precession (Raymo, 1997; Lisiecki, 2020), obliquity (Huybers and Wunsch, 2005; Liu et al, 2008), or both (Huybers, 2011; Parrenin and Paillard, 2012), to internal oscillations phase locked to the astronomical forcing (Saltzman et al., 1984; Paillard, 1998; Gildor and Tziperman, 2000; Tziperman et al., 2006), to internal oscillations independent of the astronomical forcing (Saltzman and Sutera, 1987; Toggweiler, 2008) or to period doubling bifurcation (Verbitsky et al 2018).

It would be certainly valuable to study the model's behavior to changes of all parameters. But as mentioned above, even the complete list of "parameters" involved in the very definition of summer is subject to lengthy discussions. In any case, this is not the focus of our manuscript.

Furthermore, the system (1) has one more "hidden" parameter, i.e., "1" in the glaciation equation. In fact, it is terrestrial ice mass influx that was tacitly set to be constant and equal

1. Recognition of this parameter is important for the exactly same reasons we outlined above for τ_i, τ_g, τ_d , and, indeed, it is yet another potential source of bifurcation.
We refer the reviewer to the answer to his comment RC3 for a discussion of this question.

We do not know what specific bifurcation (or their mix) we observe in the historical records and therefore the following question needs to be answered: How sensitive are all bifurcations, which system (1) may reveal, to our choice of insolation forcing?
Without answering this question the study is incomplete and inconclusive.

Our goal is not to study the answer of this kind of model to all possible parameter changes. Our purpose is to study the model's behavior under different definitions of the "summer insolation forcing", and to examine its influence on the evolution of the deglaciation threshold V_0, while having other parameters fixed. To our knowledge, it has never been done in the past to force this kind of conceptual model with different kind of insolation forcings, and to compare the induced changes in the results.

Minor comments:
1. In system (1) the glaciation equation is marked as (d) and the deglaciation equation is marked as (g). All references in the text to (g) and (d) states are therefore incorrect.
Indeed, the (d) and (g) labels have been inverted in equation (1). This will be corrected in the next version of the manuscript.

2. Line 81 "there is no contribution from the obliquity..." It is incorrect, since the obliquity is definitely present in all forcing spectra
Indeed, our formulation is confusing. Compared to the work of Parrenin and Paillard (2003), there is no explicit "obliquity term" in the equations. For instance, in Parrenin and Paillard (2003), the glaciation equation reads : $dv/dt = - I_{tr} / \tau_i - O / \tau_O + 1/ \tau_g$, with O being the obliquity. In our model, the "obliquity term" O / τ_O has been deleted. However, as the reviewer stated, there are contributions from the obliquity in the input insolation forcing, as is shown by the spectral analysis of Figure 1 of the manuscript.
We propose to remove the sentence l. 80 to avoid this confusion.

3. Line 93 "The importance of orbital forcing alone seems able to start a glaciation..." "The importance" cannot start anything. The phrase needs to be re-formulated.
We propose to rephrase the sentence and to invert the order of the paragraph for clarity. This gives :

"A critical point is to define the criteria for the switch between the glaciation and deglaciation states. To enter the deglaciation state, both ice volume and insolation seem to play a role (Raymo, 1997; Parrenin and Paillard, 2003, 2012), as terminations occur after considerable build-up of ice sheet over the last million year. To represent the role of both ice volume and insolation in the triggering of deglaciations, the condition to switch from (g) to (d) state uses a linear combination of ice volume and insolation. The deglaciation is triggered when the combination crosses a defined threshold V_0 : the deglaciation threshold. As in the work of Parrenin and Paillard (2003), this allows transitions to occur with moderate insolation when the ice volume is large enough and reciprocally. On the contrary, glacial inceptions seem to depend on orbital forcing alone (Khodri et al, 2001; Ganopolski and Calov, 2011). Therefore, the condition to switch from the deglaciation state to the glaciation state is based on insolation only : it is possible to enter deglaciation when the insolation becomes low enough."

4. Line 94 "Therefore, the condition to switch from the deglaciation state to the glaciation state is based on insolation only: it is possible to enter deglaciation when the insolation becomes low enough". You mean "glaciation" here.
Indeed, this will be corrected in the next version of the manuscript.

5. Line 245 "frow"
Indeed, this will be corrected in the next version of the manuscript.

6. Line 273 "To model future natural evolutions of the climate system, one would need to take into account for
possible evolutions of the V_0 threshold." English should be revisited.
We suggest rephrasing by : "To model future natural evolutions of the climate system, possible evolutions of the V_0 threshold should be considered."

References :
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