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Dear Editor Prof. Rui A P Perdigão,

Thank you very much for reviewing our manuscript entitled "Synchronization phenomena observed in glacial-interglacial cycles simulated in an Earth system model of intermediate complexity". We herewith resubmit a new version of our manuscript, which has been revised following the referees' comments and the comments from Dr. Ganopolski. You'll find a list of the main corrections and our point-by-point responses to the comments below. We consider that these changes will substantially improve the quality and clarity of our manuscript.

Major changes in the revised manuscript

- With an improved Figure 6, the physical mechanism of the ~250-kyr-scale self-sustained oscillations is explained in detail (response to reviewer 2 and Dr. Ganopolski).

- Supplementary Figure S9 is added to show that the self-sustained oscillations arise even without glaciogenic dust feedback if the carbon cycle feedback is active (response to Dr. Ganopolski)

- The difference between two versions of CLIMBER-2 (W19 and GB17) is described in discussion and in the supplementary material (response to Dr. Ganopolski).

Point-by-point reply to the reviewers' comments

In order to improve the readability of our replies we applied a color/type coding to discriminate our replies from the referee's comments.

Color/type coding:
Comments by the reviewers and public comment.
Reply from the authors.
New sentences in the revised manuscript.

Reply to Referee 1

Page 1, lines 12-13:
I think it's better to briefly describe the influence of obliquity than to name-drop 'vibration-enhanced synchronization' in the abstract.

We have briefly described the influence of the obliquity in the revised manuscript as follows. **The latter synchronization occurs with the help of the 41-kyr obliquity forcing, which enables some terminations and glaciations to occur robustly at their right timing. We term this phenomenon as vibration-enhanced synchronization because of its similarity to the noise-enhanced synchronization known in nonlinear science.**

Introduction:
The 100-kyr problem is described in some detail, but the 41-kyr problem is not mentioned at all. I find this odd, as the study is essentially about both problems.

In the revised manuscript, we have mentioned the 41-kyr problem in the same paragraph with the 100-kyr problem: **Another mystery is the dominant 41-kyr periodicity before the MPT that matches the period of obliquity cycles (Raymo and Nisancioglu 2003). Several mechanisms are proposed for the strong 41-kyr power (Raymo and Nisancioglu 2003, Raymo et al. 2006, Huybers 2006), while recent results reveal influences of precession cycles in the temporal patterns of pre-MPT glacial cycles (Liautaud et al. 2020, Barker et al. 2022, Watanabe et al. 2023).**

Page 3, lines 48-49:
It would, if (and only if) it is in fact linked to eccentricity. If it is for example related to 2/3 obliquity cycles, or 4/5 precession cycles, it wouldn't have to involve damping of the longer eccentricity period.

We agree. The frequency locking to ~100-kyr cycles (2~3 obliquity or 4~5 precession cycles) implies the lack of 400-kyr power. However, if it is not frequency locking (for example, if it is linear response), the prominence of 100-kyr power does not necessarily imply the lack of 400-kyr power. In our study, we indeed elucidate that the simulated phenomenon is synchronization (frequency locking). We add the following sentence into the text: **Lockings of self-sustained oscillations to one over 2-to-3 obliquity cycles (Huybers and Wunsch 2005) or to one over 4-to-5 precession cycles (Ridgwell et al. 1999) are examples of subharmonic synchronization leading to approximately 100-kyr glacial cycles.**

Page 3, lines 62-63:
Similar time scale, and amplitude as well?

Yes, amplitude as well. However, the amplitude of oscillations often increases in real-world phenomena as well as in the present simulations because the forcing is often not small. This has been mentioned in the introduction and discussion.

Page 4, line 91:
I think you can remove this sentence, as a proper scientific discussion naturally involves giving caveats.

Thank you. We have removed the sentence.

Page 5, lines 121-122: Remove the brackets.

We have removed the brackets.

Page 5, lines 124-125:
'the CLIMBER-2 is simulated' The grammar is incorrect.

Thank you. We correct the sentence as follows: **In each, CLIMBER-2 runs for a fixed astronomical configuration or under a hypothetical astronomical forcing.**

Page 5, lines 126-127:
Not sure if this information is really needed in a non-technical paper.

We have removed this sentence.

Page 6, lines 155-156:
It is further evidence that changes in the internal dynamics of the Earth system are necessary to explain the MPT in CLIMBER-2.

Thank you for suggesting a proper sentence, restricting the conclusion in CLIMBER-2. We have adopted it.

Page 6, line 157: 'is' should be 'are'

Corrected.

Fig. S7: Add a cyan line to panel B.

We have added the cyan line in the revised supplementary material.

Page 9, line 172:
'Among others' Can this be changed to 'chiefly' (or a similar word)?

Yes, but we have changed the whole sentence as follows: **Especially the glaciogenic dust feedback and the carbon cycle feedback are crucial**. Thus the meaning of "chiefly" is included.

Page 9, lines 190-191:
What is different about a termination? Why is glaciogenic dust deposition sustained during the deglaciation when it wasn't in the interstadials before?

We have explained as follows: **While the glaciogenic dust emission is low during a glaciation period, a high glaciogenic dust emission continues throughout the deglaciation since the terrestrial sediments, which are eroded and transported to the margins of the ice sheets by basal ice sliding if the ice sheet base is at melting point, are exposed to the air when the ice sheets retreat.**

Page 10, line 196: A spurious imbalance?

It is not spurious. In the revised manuscript, we have explained it as follows. **It reflects long transient dynamics, where the carbon fluxes are still slightly imbalanced. We suppose that the system will eventually achieve a more regular limit cycle behavior without a drift. However, it takes at least more than one million years. Thus, we consider that the oscillatory behavior with the subtle drift is an essential character underlying the modeled ice age cycles.**

Page 10, line 206: 'oblquity'. Typo.

Corrected.

Page 12, line 225:
'two sets of sensitivity experiments'. Add 'additional'

Added.

Figs. 7 and 10:
I found myself drawing lines at x=1.0 (true conditions). Perhaps these can be included in the figures?

Thank you for this suggestion. However, since the figures are already busy, we would like to keep these figures as they are.

Page 14, lines 264-269:
I must admit this part is lost on me. I wonder if it is really necessary to compare this mechanism to others, without any further explanation or discussion.

Thank you for pointing out this point. In the revised manuscript, we have moved the corresponding lines to the second paragraph of Summary and Discussions and have added further explanations.

Figure 11:
My compliments on this figure, it summarizes the paper perfectly.

Thank you very much!

Summary and discussion:
In general, I prefer separate discussion and summary sections. The latter can be quite short, just a paragraph. This works better when I just want to check the conclusions of a paper again.

Thank you for this advice. We once considered if separating the summary and discussion fits to this manuscript. However, in the end, the authors preferred the present structure of the discussion section. Actually, discussion sections often begin with a short summary of the research.

Page 18, line 285:
'suitable'. Perhaps change to 'small', 'specific', or 'limited'.

Thank you. We changed it to 'limited'.

Page 19, line 301:
What in particular is improved in CLIMBER-X, that makes this model more reliable?

CLIMBER-X is improved from CLIMBER-2 in several respects: (1) Resolution. The atmosphere component of CLIMBER-X is a statistical-dynamical model similar to that of CLIMBER-2 but has a substantially higher resolution (5°x5°). The 3-D frictional-geostrophic balance model GOLDSTEIN is employed in CLIMBER-X (Willeit et al. GMD 2022) instead of the 3-basin zonally averaged model in CLIMBER-2. (2) Most parameterizations are improved with available high-quality data. (3) Improved and more detailed carbon cycle processes on land and in the ocean, where the state-of-the-art HAMOCC6 model is employed (Willeit et al. GMD 2023). Mainly those three aspects make CLIMBER-X better to simulate the present and past climate fields and the historical evolution and distribution of carbon contents (Willeit et al. GMD 2022, 2023).

Page 19, lines 303-313:
IcIES-MIROC is climatically forced using a matrix interpolation method with pre-run climate simulations. That's an important difference to CLIMBER-2, which is a fully coupled model. This difference should be mentioned, as it could (in part) explain the difference in results.

Thank you for this comment. We have described that **IcIES-MIROC is a 3-dimensional thermomechanical ice sheet model with parameterized climate feedback obtained from pre-run snap-shot GCM experiments.**

Page 19, lines 312-313:
This is true for the post-MPT period mostly.

Yes, we write "simulated pre-MPT glacial cycles" for accuracy.

Page 19, line 319:
'Introduction'. Should be 'the introduction'.

Corrected.

Page 19, line 320-322:
What kind of model do Le Treut and Ghil (1983) use? Please briefly explain.

They used a simple climate--ice-sheet--bedrock model, which exhibits self-sustained oscillations with period of 5-10 kyr. However, we have removed the corresponding sentence because, in the revision stage, we have found that it is not appropriate to cite their paper for the purpose of explaining why synchronization and resonance are subtle in some cases.

Page 19, lines 326-329:
Reading this, I can't help but wondering what happens if the astronomical forcing as a whole (so obliquity and eccentricity/precession combined) is decreased. Maybe as an idea for a next study, as in principle this article describes enough experiments as it is.

Thank you for this question. It is answered by the experiments corresponding to the diagonal line in Fig. 7. So far we can only assume that the synchronization to the astronomical forcing is lost at some point of forcing amplitude as it decreases. As suggested, we will consider this as an idea for a follow-up study.

Figure B1:
Perhaps, you could include a phase wheel of obliquity and precession during terminations, like Figure 4 (top panels) in Watanabe et al. (2023).

Thank you for this suggestion. The phase wheel can provide more detailed/quantitative information for the timings of terminations. However, we believe that Figure B1 is enough for showing that the timings of terminations are constrained more tightly by precession peaks rather than obliquity peaks. So we would like to keep Figure B1 as it is.

Reply to Referee 2

** Introduction, the results sections 4.1 and 4.2, conclusions, and more: the authors need to clarify what's new. The synchronization of ice ages by Milankovitch forcing has been repeatedly discussed in the literature, including the effects of summer insolation vs. obliquity or precession, the effects of noise, the dynamics before and after the MPT, etc. There is no question that there are many new and very valuable results here. Yet, when providing a result consistent with previous results, it would be helpful to note this; if it differs from previous results, explain the reason for the difference.*

In the revised manuscript, we have clarified what's new.

In the introduction and discussion, we write as follows: **In this study we report self-sustained oscillations and their synchronization to the astronomical forcing in glacial cycles simulated in the Earth system model of intermediate complexity (EMIC) CLIMBER-2 with a fully interactive carbon cycle, specifically in a version by Willeit et al. 2019. The finding of self-sustained oscillations at the time scales of glacial cycles is not new in simple models but new in comprehensive EMICs.**

Regarding the novelty of the vibration-enhanced synchronization, we mention in Section 4.2 and Summary as follows: **While the enhancement of the ~100-kyr power by the 41-kyr obliquity forcing is consistent with previous modelling**

studies (Ganopolski and Calov 2011, Abe-Ouchi et al. 2013), we have further shown that ~100-kyr cycles become dominant only for a limited amplitude range of obliquity variations (Fig. 11b).

** As an example of the last issue: the first lines of the conclusions write: "We ... have explained the rhythms of simulated glacial cycles from the perspective of the synchronization principle": I think this was explained multiple times before. What was done here is to demonstrate this issue with a more detailed model and to perform an analysis of the model results that definitely adds to our understanding.*

Since the line is the first sentence of the summary, the same sentence in the previous section is somehow repeated. We have modified the sentence so as to tell that our finding of self-sustained oscillations is new in a comprehensive climate model and also that our conclusion is derived from a couple of forced and unforced experiments: **We have reported self-sustained oscillations and their synchronization to the astronomical forcing, for the first time, in glacial cycles simulated in the comprehensive Earth system model of intermediate complexity CLIMBER-2 (Willeit et al. 2019). Based on the results of forced and unforced experiments, we have explained the rhythms of simulated glacial cycles from the perspective of the synchronization principle.**

** The authors should show all model results in terms of equivalent sea level rather than delta18O. We have a good idea of what the amplitude of ice ages was in terms of sea level, while the isotopic signal is a complex and uncertain mix of temperature and ice volume that is difficult to decipher. The model delta18O curves could be shown in the appendix/supplementary if the authors feel strongly that the model does an excellent job producing the processes involved and that the model proxy record, therefore, contains valuable information.*

We have shown the simulated sequence of glacial cycles in delta18O when we need to compare it with the delta18O record. Otherwise (or when the information of sea level is crucial), we have shown the sea level instead of delta18O. We do so because there is no sea level reconstruction with a wide consensus before the Middle-Pleistocene transition (MPT). We fully agree with the uncertainty in interpreting delta18O. But because of the same reason, sea level records are also uncertain especially in the older period. Thus we believe that our current choice of the variables is reasonable.

** line 41: the phase locking/synchronization between insolation and ice volume was discussed by Tziperman et al. (2006) and Crucifix (2013) in much simpler models than those used here, but exploring the same issues.*

Thank you for pointing out these references. We have cited both Tziperman et al. (2006) and Crucifix (2013).

** lines 292-292: nice analysis. I am not sure the oscillations pre-MPT are self-sustained, but the authors are making an interesting case for this. The alternative is oscillations driven by obliquity (more accurately, by integrated insolation with a low threshold that filters out precession, see Huybers paper on integrated insolation) with some role for nonlinearity that can be seen by the asymmetry in the oscillations and noted by some of the papers cited here already. Verbitsky, Crucifix, and Volobuev (2018) also discuss the mechanism of the mid-Pleistocene transition and the role of Milankovitch forcing.*

Thank you. We have extended the discussions about the MPT including these references.

** line 50: the need for brevity is understood, but the mention of the different mechanisms here seems a bit superficial; what, very briefly, are the dynamics of the mechanisms in each of these papers?*

We agree that a few sentences starting from line 50 appear superficial, although the different mechanisms have been described in the following two sections. Thus, we have modified the structure of the corresponding sections and have described the different mechanisms more clearly.

How confident are we whether the oscillations produced in each of these papers represent internal oscillations or not?

These papers are clear in that their models produce internal oscillations in the absence of forcing.

** 73: mode-> model*

Thank you. Corrected.

** lines 205-210: Why would it be eccentricity and not precession times 4 or 5; or obliquity time 2 or 3? While eccentricity clearly modulates precession, it has such small power in insolation that it typically does not matter (hence the "Milankovitch paradox"). This issue has also been explored previously using simpler models that might help put things in perspective here rather than relying on the general (Pikovsky et al., 2003) reference alone.*

The simulated glacial cycles under realistic forcing are synchronized with 4 or 5 climatic precession cycles, but at the same time, we can say that the glacial cycles are synchronized with eccentricity cycles because the amplitude of climatic precession is the eccentricity. The precise timings of terminations are tightly coupled to precession peaks rather than obliquity peaks (Fig. B1). On the other hand, the ~100-kyr cycles become strongest only if the obliquity forcing exists. In terms of synchronization, the ~100-kyr cycles are roughly 1:1 synchronization to the ~100-kyr eccentricity cycles, but at the same time, they are 1:4 or 1:5 synchronization to the precession cycles, which is achieved by the help of the obliquity changes. We have made this point clearer in the revised manuscript.

** Page 11 and many other places: the difference between 107 kyr and 95 kyr is so small, given observational uncertainty, that it is not clear that there is justification for explaining a presumed 107 kyr signal in terms of a 95 kyr forcing (Rial paper). It seems worth mentioning this issue.*

Thank you for this note of caution. We have modified the text as follows. **For some realizations, a noticeable peak appears between 124 kyr and 95 kyr (Fig. S2). It might be linked with the 107-kyr peak that arises as a higher-order combination tone of 95-kyr and 405-kyr eccentricity periodicities ($1/107 \approx 1/95 - 1/(2 \times 405)$) (Rial 1999 and Appendix A), but it should be noted that the 107-kyr peak is still not well-established since it is so close to 95-kyr and 124-kyr peaks.**

** Figure 6: I agree with the public comment question asking what model component leads to a time scale of 250 kyr here. Perhaps plotting additional model diagnostics might reveal this.*

Thank you for this comment. We have improved the explanation about the time scale in the last paragraph of Section 3.2. The time scale of ~250 kyr is decomposed into two parts. One is the joint period of glaciation and deglaciation, which is 80-to-90 kyr. The other is the glacial period, whose duration depends on the atmospheric CO_2 concentration. It can be longer especially when the atmospheric CO_2 concentration is below 220 ppm. Thus, the duration of glacials depends on how fast the atmospheric CO_2 increases on average during glacial times due to an imbalance between volcanic CO_2 outgassing and CO_2 consumption by silicate weathering, as well as through carbonate pump feedbacks. We have extended the explanation in the last three paragraphs in Section 3.2.

** Section 4.1: I admittedly felt there might be too much material here. The authors may want to attempt to decide what's important and reduce the number of figures. When every statement is followed by a reference to 3 or 4 figures (e.g., Figs S9, 7a, and 7b), this reader was a bit lost in the detail :-)*

We agree that there is a lot of content in Section 4.1. However, this is needed in the discussion covering both pre-MPT and post-MPT periods. Therefore, instead of reducing the number of figures, we improved the readability of the text, avoiding a reference to three figures in a sentence

** Around Line 250: how would you reconcile this with the Huybers and Wunsch results on the synchronization with obliquity?*

In our CLIMBER-2 simulations, the obliquity forcing alone could not constrain the sequence of glacial-interglacial cycles (Fig. 8a). Thus, our result is different from the obliquity-pacing hypothesis by Huybers and Wunsch (2005). On the other hand, Huybers (2011) proposes the combined obliquity and precession pacing. The CLIMBER-2 model requires both obliquity and precession pacing in order to exhibit the ~100-kyr cycles. Therefore, it is more consistent with Huybers' conclusion in 2011. Both are cited in this article.

** Bottom of page 14: the new "vibration" terminology was mentioned in the abstract very prominently, and the authors finally get to it at this point and discuss it very briefly. I did not exactly understand what the message is and what the authors attempted to explain. The explanation was very brief, and I am not convinced that this justifies a new terminology. Also, what is the chaotic equivalent hinted at, and why is it relevant here?*

The vibration-enhanced synchronization implies that the synchronization to a forcing (~100-kyr eccentricity cycles here) is realized by the help of another, comparatively faster forcing (here obliquity variations). We introduced the term 'vibration-enhanced synchronization' because it is analogous to the noise-enhanced synchronization (Zhou et al. 2003). The noise-enhanced synchronization is a synchronization to a periodic forcing that is only achieved when the ambient noise has a suitable amplitude. In the revised manuscript, we have explained those concepts in more detail and why a new terminology is introduced (see the second paragraph of the discussion).

Reply to Dr. Andrey Ganopolski

The authors acknowledged my comments on their work. However, these comments were made on an early version of the manuscript which did not include the important finding of the manuscript published in ESDD, namely, the existence of self-sustained oscillations in the CLIMBER-2 model. This is why I would like to use an opportunity to provide additional comments on this aspect of the manuscript by Mitsui et al.

Thank you for pointing out this. We should have stated that you gave us comments on an early version of the manuscript. We again acknowledge your new comments on the present ESDD manuscript.

The manuscript is based on the version of the CLIMBER-2 model which was used in Willeit et al. (2019) (hereafter W19), and which is very similar to the version used in Ganopolski and Brovkin (GB17) but with somewhat different values of several model parameters. Similar to W19, the GB17 version of CLIMBER-2 simulates self-sustained oscillations with the constant orbital forcing and CO_2 *for regolith covering all continents (as in REG simulation in Ganopolski and Calov, 2011), although, in a* *rather narrow range of CO_2 concentrations (220-240 ppm). However, the GB17 model does not simulate any appreciable (more than several meters in sea level equivalent) self-sustained oscillations for the present regolith cover neither with constant CO_2 for the entire range of CO_2 concentration from 180 to 300 ppm nor with the interactive CO_2 for a wide range of orbital parameters. In principle, such a difference between similar model versions is not very surprising – CLIMBER-2 is a strongly nonlinear model, and even small changes in model parameter values can cause the appetence or disappearance of some dynamical regimes. Moreover, the time-dependent regolith map used in W19 is slightly different at time 0K from that was used in GB17.*

Thank you for the valuable information: The GB17 version of CLIMBER-2 is not self-oscillatory for the present regolith conditions, but it can be self-oscillatory for particular conditions (the regolith covering all the continents and 220-240-ppm CO_2). We have included these points citing your public comment. Also in Supplementary material, we have mentioned the notable differences between W19 and GB17:

(i) W19 includes an interactive dust cycle following Bauer and Ganopolski (2010 and 2014), while the dust deposition fields in GB17 are interpolated between pre-industrial and last glacial maximum fields, using sea level as weighting factor,

(ii) W19 includes the deep permafrost model of Willeit and Ganopolski (2015), which explicitly considers the effect of a coupled heat conducting bedrock on ice sheets basal conditions, accounting also for the latent heat of phase changes of water in the sediments,

(iii) as already mentioned above the present-day regolith mask differs slightly between W19 and GB17.

In particular (i) and (ii) introduce additional feedbacks in the model that may have some impacts on the simulation of internal oscillations. In order to investigate the impact of the above three model differences, we ran the W19 model without the interactive dust model, the deep permafrost model and the regolith mask in W19 but with those components in GB17 (see Figure 1 below). However, we find that those differences are not crucial for having the realizations of self-sustained oscillations in W19, while the interactive dust cycle component increases the frequency of the oscillations in part (Figure 1a-1c).

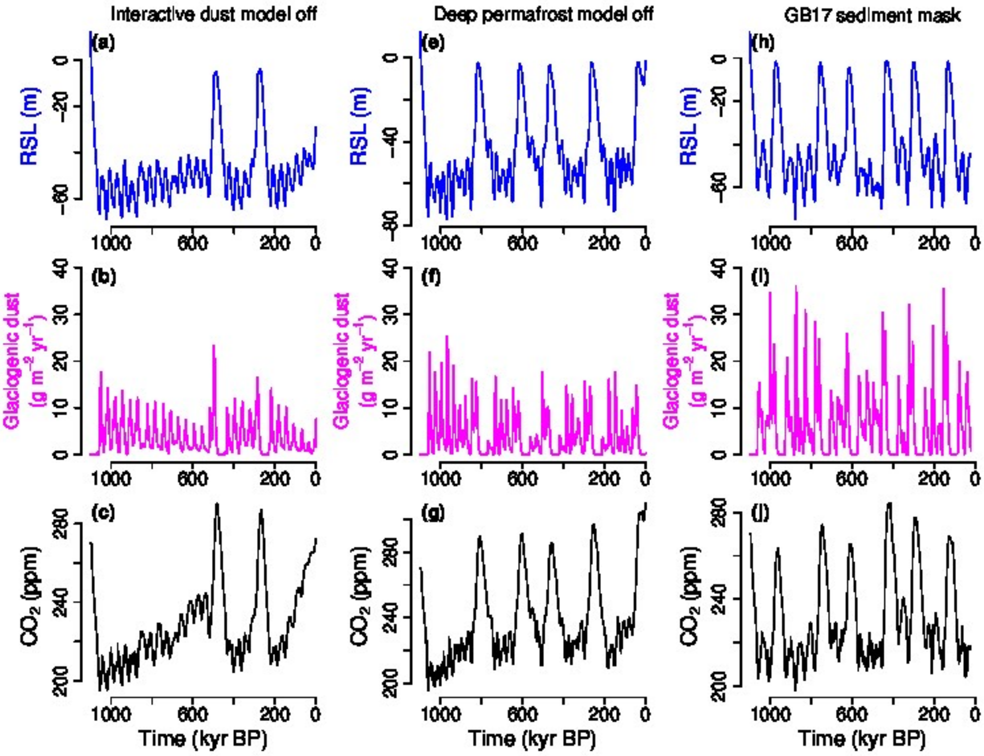


Figure 1: Dependence of post-MPT simulation results on model components that are different between W19 and GB17 versions of CLIMBER-2: (a-c) The interactive dust component in W19 is switched off in the same model. (e-g) The deep permafrost model in W19 is switched off in the same model. (h-j) The sediment mask in GB17 is used in W19. All of the three components are not critical for the existence of self-sustained oscillations, while the lack of the interactive dust cycle component in W19 partly reduces the frequency of self-sustained oscillations.

Thus, the cause of different dynamics of W19 and GB17 is probably in small differences in parameters. As Dr. Ganopolski says, very different dynamics can arise due to small differences in a highly nonlinear system, like bifurcation phenomena. We consider that CLIMBER-2 has potential to exhibit self-sustained oscillations, but it could be either self-oscillatory or non-self-oscillatory depending on the parameters or implementations.

Two questions arise in this regard: (i) What are the mechanisms of long self-sustained oscillations seen in the W19 (but not in GB17) model, and (ii) whether these oscillations with a typical periodicity of several hundred kiloyears arising under constant orbital parameters (Fig. 2c) are related to the strongly asymmetric glacial cycles with the periodicity close 100 kyr simulated in CLIMBER-2 under the influence of real orbital forcing (Fig. 2d)?

1. The appearance of very long glacial cycles (in the case of the orbital parameters corresponding to 21 ka, the periodicity reaches 500 kyr) is puzzling since none of the climate components of CLIMBER-2 has such a long time scale. The only suspect is the negative silicate weathering feedback with just the right time scale. The parameters of the carbon cycle model in CLIMBER-2 are selected in such a *way that the average during glacial cycle weathering compensates for volcanic outgassing. However, under interglacial (warm) climate conditions, weathering exceeds volcanic outgassing and CO_2 drifts down, while under glacial (cold) climate conditions, weathering is smaller than volcanic outgassing and CO_2 rises slowly, which, of course, is opposite to what is observed during real glacial cycles. A combination of several strong positive feedbacks with the slow negative weathering feedback, in principle, can give rise (but not in the GB17 version) to the oscillations with periodicities order of several hundred thousand years. Fig. 6 in Mitsui et al. provides some support for this hypothesis.*

Thank you very much for this comment. Actually, the very long glacial cycles simulated under present and LGM orbital configurations (Fig. S6 and S7) are generated via CO2 feedback. As you point out, the silicate weathering absorbing the atmospheric CO2 reduces during glacials and contrarily enhances during interglacials. Also the carbonate (alkalinity) pump changes in the direction releasing CO2 during glacials. These feedbacks change the atmospheric CO2 gradually. When the atmospheric CO2 reaches critical levels, the rapid termination or the glacial is initiated. In the revised manuscript, we have described this mechanism for the self-sustained oscillations with the time scale of ~250 kyr (new Fig. 6) in detail. The carbon cycle feedback is qualitatively the same between ~250-kyr self-sustained oscillations (Fig. 6) and those with much longer period and amplitude in Figs. S6 and S7.

2. Whatever the mechanism of such long self-sustained oscillations, more important is whether the existence of these oscillations is relevant for the 100-kyr glacial cycles simulated in CLIMBER-2 under realistic orbital forcing. I do not believe that this is the case.

We consider that the self-sustained oscillations are indeed relevant for the 100-kyr glacial cycles simulated under realistic astronomical forcing. We fully agree that self-sustained oscillations are not mandatory for generating 41-kyr or ~100-kyr cycles. Indeed, in the manuscript, we have not claimed that this is mandatory. Instead, we have proposed that if self-sustained oscillations exist, the dominant frequency of glacial cycles under the forcing is related to the period of the self-sustained oscillations. As shown in Figs 2 and S4, the frequency of the simulated glacial cycles under the presence of realistic orbital forcing is related to the internal time scale of self-sustained oscillations. We have mentioned this point in the discussion.

First, the shape and typical periodicity of these self-sustained oscillations are very different from the forced 100-kyr cycles.

We have not claimed that the underlying self-sustained oscillations explain all the aspects of ~100-kyr cycles. We agree that the underlying self-sustained oscillations are different from the actual glacial cycles. On the other hand, we can identify common aspects in the self-sustained oscillations and the actual glacial cycles:

- Shape: The self-sustained oscillations are already asymmetric. Glaciations are slightly slower than deglaciations (Figs 4 and 6).

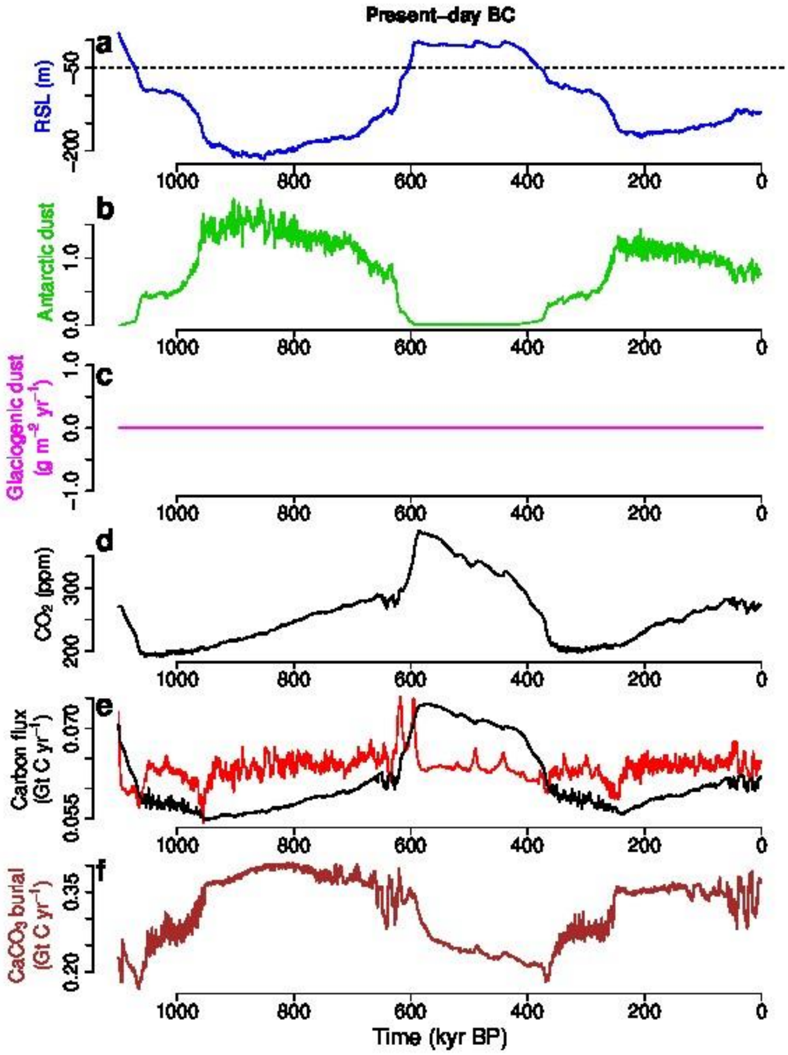
- Periodicity: The mean periodicity of the post-MPT self-sustained oscillations (~250 kyr) is not very different from ~100-kyr. If we focus solely on glaciation and deglaciation phases ignoring quasi-stable glacial intervals, the joint period is closer to ~100 kyr. The orbital forcing may help deglaciations shortening the quasi-stable glacial periods.

Thus we consider that the underlying self-sustained oscillations can be a backbone, allowing asymmetric ~40-to-100-kyr glacial cycles when the system is forced.

Second, the GB17 version does not possess such oscillations but simulates strong 100 kyr periodicity both with constant (sufficiently low) CO_2 concentrations and with interactive CO_2 (Ganopolski and Calov, 2011; GB17). Moreover, other models, which, unlike CLIMBER-2, do not include glaciogenic dust feedback and thus unlikely to possess similar self-sustained oscillations, also simulate 100-kyr cycles (e.g. Berger and Loutre, 2010; Abe-Ouchi et al. 2013). Thus, although the existence of self-sustained oscillations within a certain range of external boundary conditions may amplify nonlinear system response to orbital forcing at some frequencies, they are not essential for reproducing the main features of the late Quaternary glacial cycles.

You say that self-sustained oscillations are not essential for actual glacial cycles, citing three ice age models without self-sustained oscillations: LLN-2D (Berger & Loutre, 2010), IcIES-MIROC (Abe-Ouchi et al. 2013) and GB17. However we consider that the existence of three good non-oscillatory models does not falsify any other model. In the former two models (LLN-2D and IcIES), the atmospheric CO_2 concentration has been prescribed when simulating realistic glacial cycles. Hence we cannot exclude the possibility of self-sustained oscillations in these two models if a carbon cycle model is interactively coupled.

Indeed the CLIMBER-2 model is unique because of its strong glaciogenic dust feedback. However the W19 version of CLIMBER-2 exhibits self-sustained oscillations even without glaciogenic feedback if the carbon cycle feedback is active (new Fig. S9; also below). Thus the carbon cycle feedback alone can be a potential source of oscillatory instability.



New Fig. S9: Simulated self-sustained oscillations **without glaciogenic dust feedback** for the present background condition (regolith cover and volcanic outgassing rate) and fixed orbital configuration (e=0 and $\varepsilon = 23.34°$): (a) Relative sea level (RSL). The horizontal dashed line indicates the RSL of -50 m, below which the dust-borne iron fertilization of the Southern Ocean is enhanced in the model. The mean periodicity is about 250 kyr. (b) Antarctic dust deposition in relative units as a proxy for the iron flux over the Southern Ocean. (c) Glaciogenic dust deposition rate. The mean value at (100°E, 45°N) and (100°E, 55°N). (d) Atmospheric CO_2 concentration. (e) Carbon fluxes: the variable volcanic outgassing rate (red) and the consumption of atmospheric CO_2 due to terrestrial weathering of silicate. (g) $CaCO_3$ burial in the deep ocean and on the ocean shelf. It shows that the carbonate (alkalinity) pump is strengthened in the direction releasing CO_2 during glacials on average as well as during termination.

Nevertheless, we fully agree that self-sustained oscillations are not mandatory for generating 41-kyr or ~100-kyr cycles. Actually, in the manuscript, we have not claimed that it is mandatory. Instead, we have proposed that if self-sustained oscillations exist, the dominant frequency of glacial cycles under the forcing is related to the period of the self-sustained oscillations. We have made this point clearer in the revised manuscript.

Regardless of the existence of self-sustained oscillations, we suppose that the dominant frequency of glacial cycles under the forcing can be determined in relation with the internal time scale. The internal time scale is apparent when the self-sustained oscillations are presented, but it is not obvious in non-self-oscillatory systems. Developing a unified explanation for self-oscillatory and non-self-oscillatory systems is left for future work.

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