Reply to Referee 2

First of all, thank you very much for reviewing our manuscript in detail and giving us very valuable feedback. In what follows, we respond to your comments and questions, point by point, and propose several changes to the manuscript in accordance. We think that these changes will substantially improve the quality and clarity of our manuscript.

In order to improve the readability of our replies we applied a color/type coding to discriminate our replies from the referee's comments. We have attached our replies as a pdf document since color coding is not available in the browser based text editor.

Color/type coding:
Comment by the referee.
Reply from the authors.

** 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.*

We will revise the mentioned sections to clarify our new findings that are (i) the self-sustained oscillations in a comprehensive Earth systems model of intermediate complexity (CLIMBER-2) and (ii) their synchronization to the astronomical forcing including the nontrivial effect of obliquity forcing termed the vibration-enhanced synchronization.

** 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.*

Yes, we agree. We will modify the sentence as follows: "With an Earth system model of intermediate complexity, we have demonstrated that the rhythms of simulated glacial cycles can be understood from the perspective of the synchronization principle. Moreover we have performed a detailed sensitivity analysis to reveal respective roles of different orbital elements and have found a nontrivial effect of obliquity forcing, which we term vibration-enhanced synchronization."

** 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 will mention them there.

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

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 will modify the structure of the corresponding sections and will describe the different mechanisms more clearly.

** 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 are realized only if 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 will make 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 will modify 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 ($1/107 \approx 1/95-1/(2\times405)$) (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 peak."

** 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 will run additional simulations to try to explain the time scale of 250 kyr in more detail, and will try to improve the description of it in the revised manuscript.

** 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 would prefer to improve the readability of the text, avoiding a reference to multiple figures in a sentence, as much as possible.

** 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.

** 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 will explain those concepts in more details and why a new terminology is introduced.