Anonymous Referee #2

I thank the Referee for the useful and constructive comments and suggestions. Below is my response with the original Referee's comments in blue italic.

*1 . **I do not agree with all the views** presented here by the author.*

I suppose this means that the Referee disagrees only with some of my views. This interpretation is supported by the next sentence:

I certainly do agree on the main mechanisms behind the 100-kyr-cyclicity.

This Referee's statement is highly appreciated

But I don't like the so-called "regolith hypothesis"

This is a personal opinion of the Referee. However, I respectfully disagree with the following statement:

now, even its first promoter (Peter Clark) explains why it was not a good idea after all.

While the authors have the right to retract the paper (as far as I know, Clark and Pollard's 1998 paper was not retracted), they cannot retract their own hypothesis if it has been widely discussed for 25 years and has already gained strong support (e.g. Willeit et al., 2019).

Now, about Peter Clark and the fate of his hypothesis. As far as I know, Clark presented his new opinion first at EGU 2021 ("Requiem for the Regolith Hypothesis"). I did not attend his online presentation at EGU, but I attended in person the QUIUGS workshop in Lamont in September 2022 where Clark presented "Requiem" again. My personal impression was that Clark's presentation wasn't met very enthusiastically, and a number of critical questions have been asked. Now, three years after the "Requiem" abstract was submitted to the EGU meeting, the paper has still not been published, and it is impossible to learn based on which methodological advances Clark disproved his former hypothesis. This is why Clark's personal opinion on this issue is of no relevance for my paper.

In the paper, it is clearly presented as a hypothesis, which fills our lack of knowledge on the origin of long-term trends: may be, for completeness, the author could state that this remains a controversial hypothesis.

I believe any hypothesis in some sense is controversial, i.e. it is not generally accepted. After the hypothesis is finally proven and universally accepted – it is no longer "hypothesis"; it is a Law or a Theory. Of course, there are a number of other ideas about the nature of MPT, and in the last paragraph on page 26 I cited five papers presenting alternative views (Chalk et al., 2017; Farmer et al., 2019; Hasenfratz et al., 2019; Ford and Raymo, 2020; Sutter et al., 2019). Since my paper is not a review paper, I think five references are enough. In addition to my paper, there is a 20-page review by Berends et al. (2021), which describes many more ideas about the nature of MPT.

2 . Figure 14 is not referenced in the text. Besides, in my opinion, it is entirely useless and does not help the reader to understand the paper. I would suggest removing it.

Agreed. The figure will be removed.

3 . When discussing the speed of the forcing behind the MPT, ie. the rate of change of the critical ice volume Vc, around lines 815-820: an interesting paper was written on this specific point by Legrain, *Parrenin and Capron (Nature communications Earth & Environment, 4, 2023) using a rather similar "threshold based" model; with the conclusion that a gradual change appears more likely when using random parameters.*

I will cite Legrain et al. in section 4.6 in the sentence (L. 428) "*To reproduce the MPT in P98, the critical ice volume was made time-dependent with a smaller value at the beginning of Quaternary and a larger one toward the present.*"

4. Equation (2) line 300. A minus sign is missing for k=2 (v should decrease during terminations).

Thank you! Indeed, this is a very unfortunate typo, which will be fixed.

5. line 574. I am not convinced at all that the 100-kyr-cycle is a "peculiar regime". Such a 100-kyr periodicity appears in many different contexts and in many pre-Quaternary Earth's paleoclimatic records. For instance, in Pälike et al. (Science 2006) there is a clear 100-kyr cycle in the ^{18}O that might be linked to Antarctic ice-sheet variations. Of course, interpretations are more difficult for these earlier periods, but the Quaternary is certainly much too short a time span to talk about "peculiar" or "ordinary" regimes.

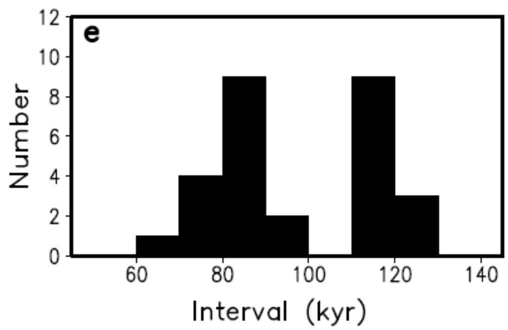
The presence of eccentricity periodicities in the paleoclimate records prior to the Quaternary is not surprising but rather expected: any nonlinear transformation of orbital forcing (and Earth is such a nonlinear transformer) should cause the appearance of ALL eccentricity frequencies. This is precisely what is seen in Pälike et al. (2006) and some other records, and which is very similar to the frequency spectrum of the Imbrie and Imbrie model but not in the real spectra of the late Quaternary (Fig A3). What is unique about the Late Quaternary is the absolute dominance of one eccentricity frequency (100 kyr) and the absence of others. Such a situation requires a very "peculiar" type of non-linearity, and such behaviour is not seen prior to the Late Quaternary. Of course, "peculiar" does not mean that this never happened during the entire Earth's history, but I have never seen anything similar in the earlier paleocrecords.

6 . line 560: « phase locking has a different meaning … ». Of course, "model MiM" and "model3" are different, but in both cases the locking is directly linked to threshold crossing. It is not clear to me why « phase locking… » should have a different meaning.

Fully agree, "*different meaning*" is not the right expression – phase locking is a phase locking. What I meant to say here is that the **mechanisms** of phase looking in MiM model (as well as in different Van der Pol oscillators, etc.) and in Model 3 (also Model 2, Paillard 1998) are different: in the first case, the self-sustained oscillations with a periodicity close to 100 kyr exist without any external forcing and the amplitude modulated extremal forcing under certain conditions can synchronise these internal oscillations with eccentricity cycles. In the second case, the models do not have any internal oscillations, and glacial cycles only arise under the influence of precession-obliquity forcing. Under some conditions, these cycles are synchronised with the amplitude modulation of the precessional cycle. I will modify the manuscript to make this point clear.

7. Figure 8e: there is a single very long "cycle" of about 150-kyr in the histogram. I am wondering which one it is… and how is it possible with this model: a few details on this particular cycle could be helpful.

The reviewer is absolutely right – a single long cycle depicted in the histogram (Fig. 8e) is not present in Fig.8b. This is because the experiments shown in Fig. 8 and 9 began at 3 Ma from initial v=0 conditions, but only the last 2.8 Ma were shown and analysed due to a strong dependence of the model solution from the initial conditions during the first 0.2 Ma. However, because of an algorithmic mistake, when producing a histogram shown in Fig. 8, the entire 3 Ma run was analysed, and at the beginning of this run (prior to 2.8 Ma), there is one 150-kyr long cycle. For consistency with the rest of Fig. 8 and 9, this single long cycle will be excluded from the histogram, and Fig. 8e will now look as shown below:



In addition, to be precise in the description of this experiment, the sentence (L. 400/401) will be modified: “To enhance the resolution of frequency spectrum, the model has been run through the past 3 Ma, of which the last 2.8 were analysed and shown in Fig. 8 and Fig. 9”.

8. Line 350: “Climber-2 has a problem simulating timing of TV while Model 3 does not”… I find this quite interesting! In the author’s opinion, is it pure chance? Or could conceptual models be “in some way” more robust than physical models?

Yes, this is an interesting point which deserves some elaboration. Indeed, during MIS12 to MIS11 transition, Model 3 overperforms CLIMBER-2. This is because the orbital forcing during Termination V is very weak and under such forcing CLIMBER-2 (with interactive CO2) simulates TV too late or even fails to simulate complete deglaciation (Willeit et al., 2019, Fig. 2). Model 3 does not posses such problem because the conditions for triggering glacial termination in the models are: $v > v_c$, $F > 0$, $dF/dt > 0$, where F is the anomaly of orbital forcing. This explains why Model 3 is insensitive to the amplitude of orbital forcing. However, if I change the last condition to $dF/dt > 5$ W/(m^2 kyr), Model 3 results become similar to CLIMBER-2, as it skips Termination V. Since this did not happen in reality, the minimum value of dF/dt required for triggering glacial termination must be rather small. Interestingly, simulations of other glacial terminations of the Late Quaternary are much more robust.

9. some typos:

Figure 12: Qusi-linear ->Quasi-linear

Legend Fig 7: artifitial -> artificial

Thank you! These typos will be fixed