

Dear reviewer,

Thank you for your comments, we here after respond point by point:

*This study investigates the MPT using the conceptual but physically based model PACCO (Physical Adimensional Climate Cryosphere Model). The model incorporates insolation forcing, CO<sub>2</sub>, ice dynamics, and the evolution of regolith layers. Modelling simulations suggest that the progressive removal of regolith slowed ice-sheet flow, allowing larger ice sheets that match the timing and amplitude of the MPT seen in proxy records. Simulations including CO<sub>2</sub> trend but constant sediment layers are not conclusive for simulating MPT. Overall, the findings support the regolith hypothesis as a plausible mechanism for the MPT, but also that hydrological sensitivity may have contributed as well.*

*The approach addresses a crucial question in paleoclimate research: the trigger of the MPT. The choice of model to test the regolith hypothesis, related to the spatial distribution of regolith through time beneath the ice sheet is unconventional, given that the study relies on a 0-D physical model. The study effectively leverages the model flexibility by testing several hypothesis and conducting sensitivity tests. However, the results of the model require more thorough explanation to support the conclusions, in my opinion.*

*The manuscript is generally well written and structured. However, the discussion section lacks sufficient contextualisation with respect to previous studies, does not provide a critical assessment of the authors work, and suffers from a lack of clear structure.*

*Main comments:*

*1. The regolith hypothesis is inherently geographically-based and has already been tested using 2D models (e.g., Willeit et al. 2019). In particular, the spatial evolution of regolith patches beneath the ice sheets is expected to play a critical role in their stability. For this reason, investigating the hypothesis with a spatially adimensional model is, in my view, not an obvious or straightforward approach. At a minimum, a section describing the implications and nuances of this approach choice in the discussion section is necessary. However, the manuscript currently lacks any explicit acknowledgment of model limitations or a critical appraisal of the robustness of the results.*

We understand this concern, but must stress that conceptual models are still useful for a better understanding of the long-term climate evolution, as still reflected in recent literature on the MPT (e.g. Verbitsky et al., 2018, Leloup and Paillard, 2022, Verbitsky and Crucifix, 2023, Koepnick and Tziperman, 2023, Ganopolski, 2024 among others). Of course, an adimensional model will fail in capturing the potential consequences of a spatially-heterogeneous regolith evolution on the overall basal dynamics of the Northern Hemisphere ice sheets. The same applies to the spatial distribution of accumulation and ablation. However, in spite of our adimensional approximation, our results show a successful characterisation of the MPT periodicity change facilitated by the inclusion of the effects of the regolith on basal ice dynamics. Furthermore, all processes are simulated in a physically consistent (not empirical or conceptual) manner. On the other hand, we acknowledge that others

(e.g. Willeit et al., 2019) have already nicely addressed the implication of the regolith in 2D models. But, because of the absence of an explicit module for the interactions between sediments and basal ice dynamics, the assumptions for regolith evolution remain simple even at 2D: a simple progressive shift from a full regolith coverage at 3Ma (with an ad-hoc enhanced sliding factor of 5) to the present-day sediment mask. This study was indeed very inspiring and we are currently working on implementing an explicit basal sediment module in our 3D ice-sheet model, but this is out of the scope of the current manuscript. Nevertheless, we will acknowledge the limitations of our approach inherent from its adimensional character and will accordingly discuss the implications.

*2. The BASE simulation, which serves as the reference for sensitivity tests and comparisons across different hypotheses, does not adequately reproduce the amplitude of the 40 kyr world (Fig. 4f), but only approximately a quarter of its amplitude. As a result, the climate state preceding the MPT is not well simulated in the model. Indeed, the too low amplitude of the climatic 40 ka cycles likely preserve a large quantity of regolith until the start of the MPT. How does this limitation affect the simulation of the MPT under the various hypotheses tested?*

It is true that in the BASE simulation pre-MPT sea-level variations are underestimated compared to other studies or to the  $\delta^{18}\text{O}$  signal. This is the case for the original proxies we used (Bintanja and van der Wal, 2008; Berends et al., 2021b). However, following a suggestion by referee 1 (see RC1 for more details), we have adapted the figure to include a more proxy-based reconstruction (Elderfield et al. 2012). Figure RC1.2 clearly shows an improvement in the comparison.

In addition, compared with the  $\delta^{18}\text{O}$  signal, this underestimation varies from 3-1.1 Ma. It is minor and only present for a few glacial maxima between 3-1.8 Ma, and of the order of a half for 1.8-1.1 Ma. However, this is indeed a limitation of our study (although not necessarily a limitation of our modelling approach). There are two main potential causes for the misrepresentation of the pre-MPT cycles already addressed in the existent literature for the MPT:

1. The 65° boreal summer solstice insolation is known to overestimate the local effects of precession and therefore produces an enhanced response around a 23 kyr periodicity (Leloup and Paillard 2022).
2. The Antarctic ice sheet contributed substantially to sea-level variations in the pre-MPT 40-kyr world. As stated in Raymo et al. (2006), because the Earth's orbital precession is out of phase between hemispheres, precession-related changes in ice volume in each hemisphere would cancel out while the in-phase obliquity-induced changes in Antarctica and in NH ice volume would add up. Of course this cannot be captured by PACCO since it only simulates the Northern Hemisphere ice sheets.

Elucidating the exact cause of the obliquity vs precession pre-MPT sea-level amplitude issue is not directly addressed in our paper. However, underestimating the pre-MPT amplitude is indeed a limitation of our study and we will accordingly acknowledge this in the new version of the manuscript by expanding the discussion section and reflecting this discussion.

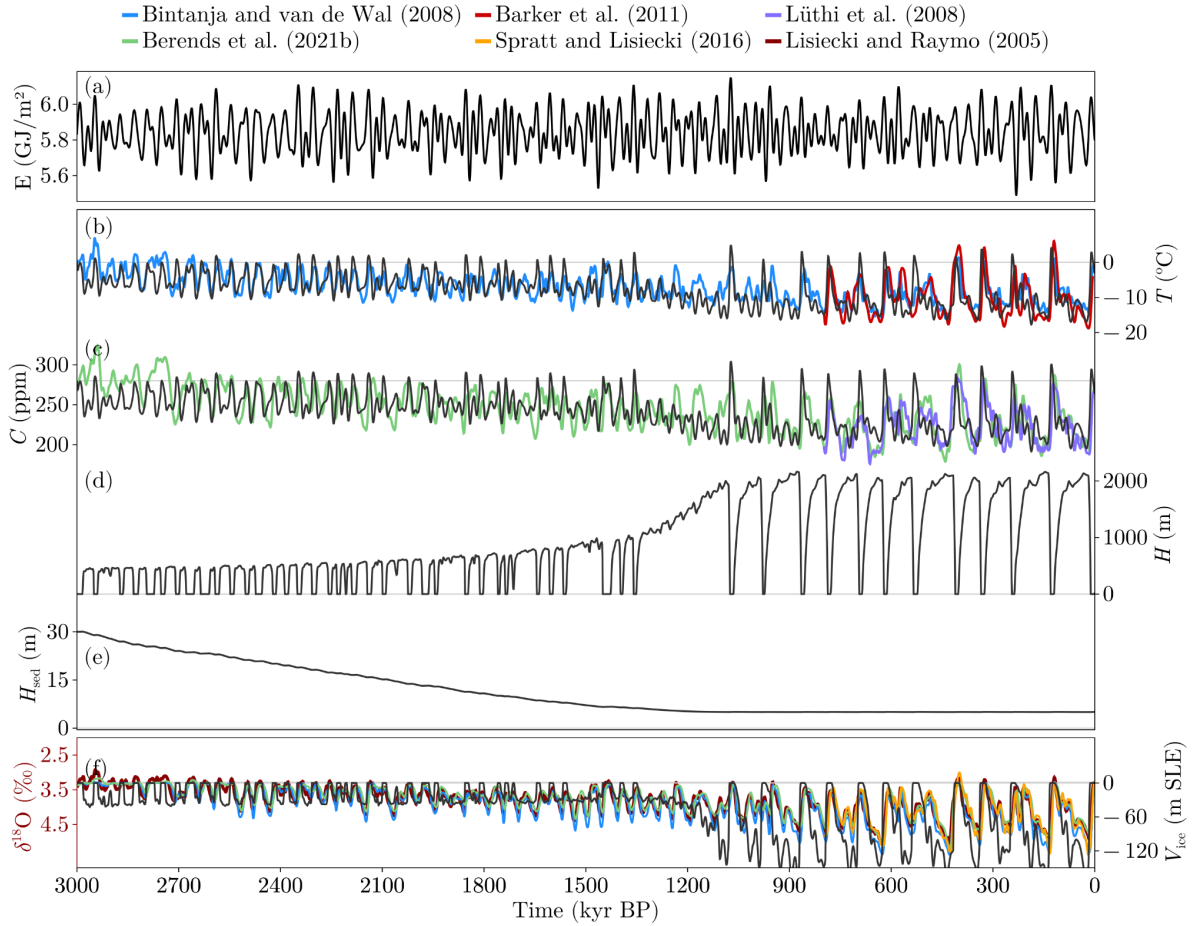
On the other hand, this limitation does not affect the post-MPT simulation in any manner or the other hypotheses tested here. First, the experiments described in section 3.5 are carried out by

unplugging the interactive regolith dynamics and second, as shown in section 3.4, when our model captures better the 40-kyr world it also simulates a very good post-MPT period.

*A related question is the choice of insolation forcing. Using an alternative insolation metric (CST or ISI instead of SSI) allows the model to better capture the 40 kyr amplitude, yet in doing so the MPT itself is simulated much earlier than in the paleoclimate records, which is likely due to the larger quantity of regolith removed during the 40 ka cycles. The choice of insolation metric in the BASE model (SSI) versus alternatives such as CSI or ISI is not clearly justified and likely has a larger impact on the study results than the authors suggest.*

First, SSI was chosen for historical reasons and for preserving Milankovitch's initial hypothesis (as for example, in Ganopolski, 2024's Generalized Milankovitch Theory). SSI is indeed usually employed in conceptual modeling (Paillard, 1998; Parrenin and Paillard, 2003; Leloup and Paillard, 2022; Ganopolski, 2024) and it is cheaper to compute than CSI and ISI.

Second, the sediment module was calibrated against paleorecords using the SSI forcing. The regolith-related parameters could be slightly re-tuned for CSI and the MPT would be simulated at the right timing (see figure RC2.1 below). However, we decided to keep the same regolith parameters as in BASE when exploring other insolation metrics for clarity. In this way, the effects of varying the different insolation forcings are visible on the periodicity-change of the system under the exact same set of parameters as BASE and thus only reflect the effects of insolation. Nevertheless, we agree that we should include a sentence to justify the choice of SSI and will do so in the new version of the manuscript.



**RC2.1.** BASE experiment using CSI forcing (BASE-CSI). Note that we only changed  $f_p$  to  $1e-7$  (-29% of the original value in CSI experiment) and  $f_v$  to  $1.15e-5$  (-62% of the original value in CSI experiment). Note that, in light of the Referee's comment, we will also include this figure in Appendix B.

*3. This study is in line with a broader set of modeling efforts aimed at simulating the MPT over the past decade. Several of these studies, however, have reached conclusions that differ from those presented here. I strongly encourage the authors to expand the discussion by explicitly comparing their results with previous work. A concrete example, though not the only relevant one, is the recent publication by Scherrenberg et al. (2025) in this journal. It would be valuable to discuss whether the differences arise from the type of model employed, the assumptions underlying the hypotheses, or the specific formulation of the model.*

The main difference with Scherrenberg et al. (2025) is that we only force the model using insolation. However Scherrenberg et al (2025)'s forcing index includes the CO<sub>2</sub> signal. In our opinion this actually forces the orbital-scale response during the Pleistocene in their model. In our case, the orbital-scale response emerges from the model physics. This agrees with studies with models more comprehensive than ours suggesting that CO<sub>2</sub> variations are not critical to produce glacial-interglacial cycles (e.g. Abe-Ouchi et al 2013; Willeit et al 2019). Finally, our conclusions do not differ much from those of recent studies. For example, Verbitsky et al. (2018) stated that the MPT is the product of a change in the balance of feedbacks in the system, Willeit et al. (2019) showed in a 3D model that regolith can produce the MPT and Ganopolski (2025) summarized the

main processes that govern GIV which include the change in dynamic regime and also the importance of the ice-sheet size.

The new version of the manuscript will include an expanded discussion to compare with previous results. We appreciate the suggestion.

*4. I am quite surprised by the absence of effect of a decreasing trend in CO<sub>2</sub>, with concentrations starting as high as 700 (!) ppm down to the late Pleistocene values, on the triggering of the MPT. In another hand, the model is able to switch from the 40 to 100 kyr world only by changing hydrological component (i.e. accumulation sensitivity to temperature). Both of these results are quite different from the rest of the literature and would deserve further discussion and comparison with other studies in the discussion section.*

There is a simulated effect of starting at such high CO<sub>2</sub> concentrations in our model (see the related minor comment below). However, the CO<sub>2</sub> trend is not enough to trigger MPT in our case, and it plays a lesser role on the ice evolution than for example in Scherrenberg et al. (2015) or in Willeit et al. (2019). In Scherrenberg et al (2025), a strong influence of a decreasing CO<sub>2</sub> trend on sea-level amplitude is expected because, by construction, the reconstructed CO<sub>2</sub> trend is used to build the climate index utilized to force the ice-sheet model. As for the comparison to Willeit et al. (2019) we both have an active (not imposed) carbon cycle, thus the reason for the lower influence of the decreasing CO<sub>2</sub> in our case is likely our adimensional assumption: It is conceivable that under high CO<sub>2</sub> atmospheric concentrations a global model such as CLIMBER-2 will inhibit glacial inception, even under cold orbits, in continental areas where ablation remains high, so that those glacial maxima will show a relatively low amplitude. In our case, however, due to the lack of spatial dimensions, if those same cold orbits allow for inception, the ice sheet will grow according to the mean conditions without reflecting any spatial heterogeneity. We will acknowledge this fact in the new version of the manuscript and expand the discussion accordingly with particular emphasis on the comparison with previous studies.

On the other hand, concerning section 3.5 (hydrological component) we will also expand the discussion on the potential implications of such new findings. It is worth noting here that the search for MPT hypothesis beyond the regoliths and the decreasing CO<sub>2</sub> was motivated by the new deconvolution of  $\delta^{18}\text{O}$  done in Clark et al. (2025). Even though sea-level is not explicitly reconstructed in that study, by qualitatively exploring their new temperature reconstruction, one might infer that the associated pre-MPT ice volume amplitude has been classically underestimated. If so, the regolith hypothesis might have to face a conundrum: How could the pre-MPT ice-sheet maxima be as big as the post-MPT in a 40-kyrs world? Section 3.5 might shed light on such a question if a new sea-level reconstruction motivating it is finally presented.

Therefore, we will again expand the discussion by including this latter context in the new version of the manuscript.

*Minor comments:*

*Title: Consider using “Exploring” instead of “Understanding” to reflect a less ambitious and more accurate scope.*

We agree and will change the title accordingly.

*Abstract: In my opinion, too much space is given to the introduction, with too little devoted to summarizing the study methods and results. The long sentence that carries all results (line 9-11) should be split into at least two shorter sentences. The last sentence is overly descriptive and does not capture the main conclusion.*

We thank you for your nice suggestion and we will modify the abstract accordingly.

*Line 21: The citation of Chalk 2017 is not the most appropriate reference to introduce the concept of the MPT, go for a more historical one.*

We agree. We will use Shackleton (1987), Lisiecki and Raymo (2005) and Clark et al. (2006) as references to the MPT.

*Line 23: This sentence should be moved up, as the described feature is an inherent part of the MPT itself, not an additional aspect.*

We agree and modify the manuscript accordingly.

*Line 31: References are needed to support the existence of the regolith. Currently, all cited works relate only to the second part of the sentence.*

We agree. The new references will be:

- Setterholm and Moorey (1995)
- Clark et al. (1999)
- Goss and Rooney (2023)

*Line 24: The paragraph is quite dense. it would be better to start a new paragraph line 31 to improve clarity.*

We thank you for the suggestion and we will implement it in the new version of the manuscript.

*Line 50: The sentence is quite reductive of the work done, as the authors also test the CO<sub>2</sub> decrease hypothesis.*

We agree and we will change it accordingly.

*Line 51 and following: I would avoid describing the paper section by section. The current structure is standard and the descriptions here are not helpful because too vague.*

Agreed. We will delete those lines.

*Line 114: It should be mentioned that while there is a change in amplitude and frequency, the model does not reproduce the amplitude of 40 kyr cycles. It seems the BASE model transitions from a quasi-stable climate directly to 100 kyr cycles, which is not equivalent to reproducing the MPT. This is*

*nuanced by the fact that the 41 kyr cycles are better captured using other insolation metrics. However, why were these other insolation not applied across all simulations, instead of SSI?*

Please see above our previous responses concerning the choice of insolation forcing. Applying other insolation forcings (e.g. CSI) to all sections does not alter the main findings of the study and we believe it would deviate reader's attention from the well-structured and compartmentalized outline of the current progressive experimental setup. See also figure RC2.1.

*Line 115 and following: At this stage, it may be premature to draw such a conclusion. In my view, it is the constant sediment simulation that provides stronger evidence for that.*

We agree. We will change those lines accordingly.

*Figure 8: How is defined the  $t_{MPT}$  value ? I guess it is an arbitrary choice, but it needs to be justified in the text.*

$t_{MPT}$  is defined in Equation 7 and justified in text as: "since we empirically observed a threshold in the oscillatory regime around that value of sediment thickness".

*Figure 9: It would be interesting to do the same plot but with sea level value (as in fig. 4f) to see how it vary compared to the sea level curves.*

We plotted  $H$  since it is one of the main prognostic variables of the model, and we believe that because the manuscript is already quite lengthy and has 15 figures (plus the the former figure of Appendix B and the new figures RC2.1 and RC.1.3 to be added in the Appendix as well), adding new plots is not desirable.

*Line 200: Interesting observation. However, the authors note that with CSI and ISIn, 40 kyr cycles are larger and thus remove sediments earlier. This relates to a key criticism of the regolith hypothesis: much of the regolith would already be removed during the initial 40 kyr cycles. Could the good match between modeled regolith removal and MPT timing be due to the BASE experiment's failure to produce realistic 40 kyr cycles compared to data?*

We think this is not the case. The good match of BASE with the proxies is intrinsic to the change in dynamic regime, but it is not related by any means with the periodicities before the MPT. The CSI and ISIn experiments remove more sediments because we kept  $f_v$  and  $f_p$  as in BASE. This is important and the reason why we carried out the SEDIM experiment. Our formulation of  $H_{sed}$  evolution is simple and thus, the election of  $f_v$  and  $f_p$  is key to the correct timing of the MPT. Therefore, our good match with BASE is not due to SSI forcing but due to the fact that the change in the dynamic behavior of the ice sheet is produced at the right time. Please see also our response to main comment #2 and figure RC2.1.

*Line 205-207: I disagree with this conclusion. The change in frequency and amplitude occurs very early compared to the "real" timing of the MPT.*

The conclusion is that the change in dynamic regime due to the removal of regolith is the trigger of the MPT. We were not saying that the timing was good, but the mechanism is. See also our answer to



the referee's main comment #2 where we show a CSI simulation with tuned regolith parameters allowing not only the right pre-MPT frequency but also a good MPT timing.

*Line 224: The absence of an impact on the trend in frequency and amplitude is quite surprising, especially in light of previous modeling studies on the MPT (e.g. Scherrenberg et al., 2025, CP; Willeit et al., 2019; Science Advances).*

The influence of the decreasing CO<sub>2</sub> is indeed limited when comparing to other studies (see main comment #3) but it does exist (see new figure RC1.3): The runs with an initial CO<sub>2</sub> above 600 ppm show a tendency to inhibit many glacial maxima between 2 and 1.5 Ma. Note, for example in the red curve of figure 14, the presence of only a few short (precession-induced) and low-amplitude glacials during that period. For the second part of the runs (1 - 0 Ma), it can be seen that higher CO<sub>2</sub> translates into slightly thicker ice sheets (because of the enhanced accumulation associated with a warmer atmosphere), with little frequency impact. This effect is progressively attenuated from the initial CO<sub>2</sub> range of approximately 500 - 250 ppm until the end of the simulations.

*Line 234: This section and the results here are quite surprising, and to my knowledge, the first modelling study that proposes the hydrological cycle as a trigger of the MPT.*

Thank you for this comment. We will try to stress out the novelty of the section and will expand the discussion accordingly (see main comment #4).

*Line 244: The sentence here, will describing accurately the results of this study, sounds very surprising. See main comment 4 for more details.*

(see our response to main comment #4)

*Line 284 and after. The paragraph should be reworked, as is it poorly written: e.g. "Of course, it is difficult to come to a final conclusion without proxies with a more accurate time resolution covering the Early Pleistocene."*

We have rephrased it: "Having a paleorecord with more accurate time resolution covering the Early Pleistocene would help to shed light on the mechanisms explored in modelling studies".

*Typos:*

*Line 8: "sediment layers of sediments above the continents"*

*Ligne 105 : "beyon the regoliths"*

*Ligne 225 : allows*

*Ligne 255 : removes*

We appreciate your careful observation of these typos and we will correct them in the new version of the article.

*References:*



Willeit, M., Ganopolski, A., Calov, R., & Brovkin, V. (2019). Mid-Pleistocene transition in glacial cycles explained by declining CO<sub>2</sub> and regolith removal. *Science Advances*, 5(4), eaav7337.

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Sincerely,  
Sergio Pérez-Montero et al.

## References

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