Summary of changes related to round 2 of comments.

Note about the purpose of this document: This is a summary of the changes implemented in the new version of the manuscript. Please note that we have included some additions to the previous version of the manuscript in **orange** and fragments already included in **green**.

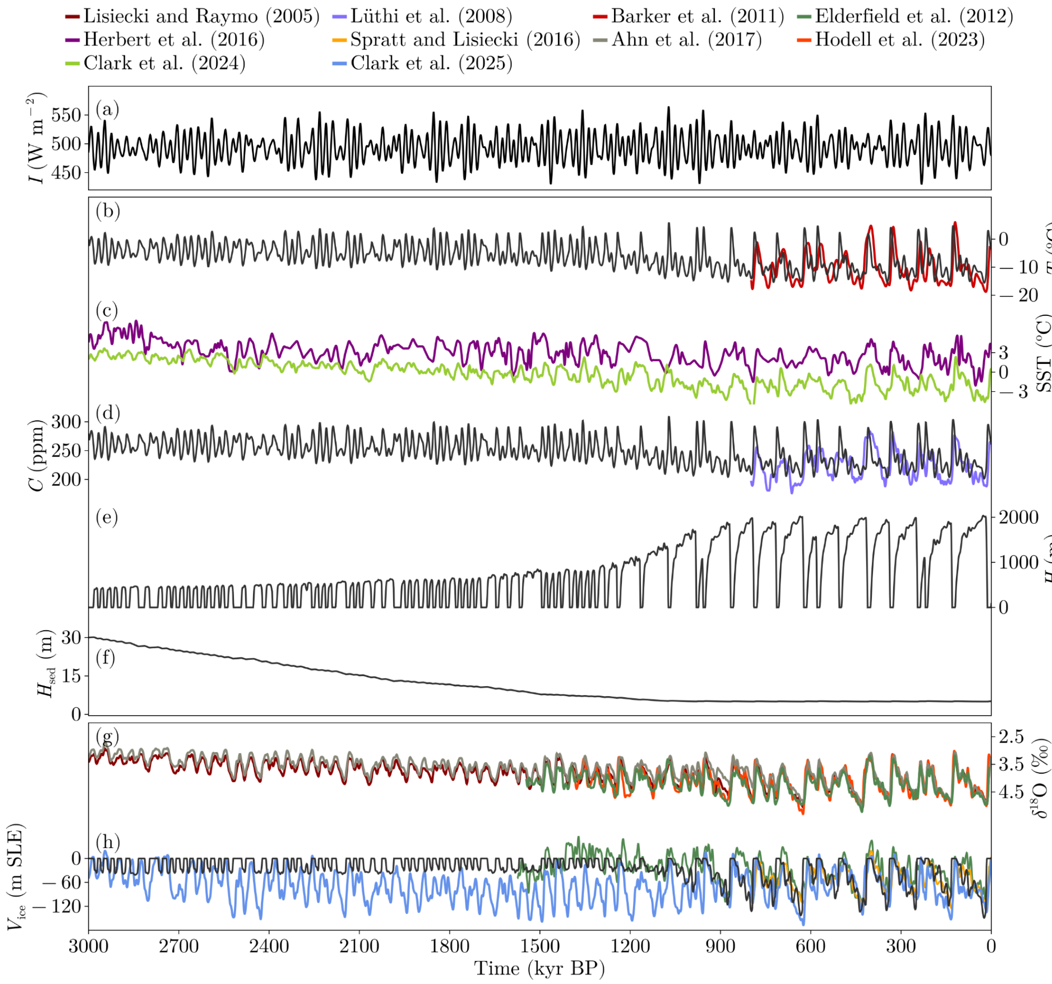
Note about typos: We found some typographical errors in the manuscript and corrected them without altering the meaning of the text. These errors are highlighted in the attached document, with the differences between the previous and the current versions.

Changes related with comments from reviewer 2:

1. We have added some sentences to Section 3.1.
2. We have reorganized the discussion section as suggested by the reviewer.
3. We added a sentence about the uncertainty in the pre-MPT amplitude right after discussing our results related with the insolation metrics:
4. We included a more detailed comparison with Scherrenberg et al. (2025) results.

Changes related with comments from editor after reviewer 1:

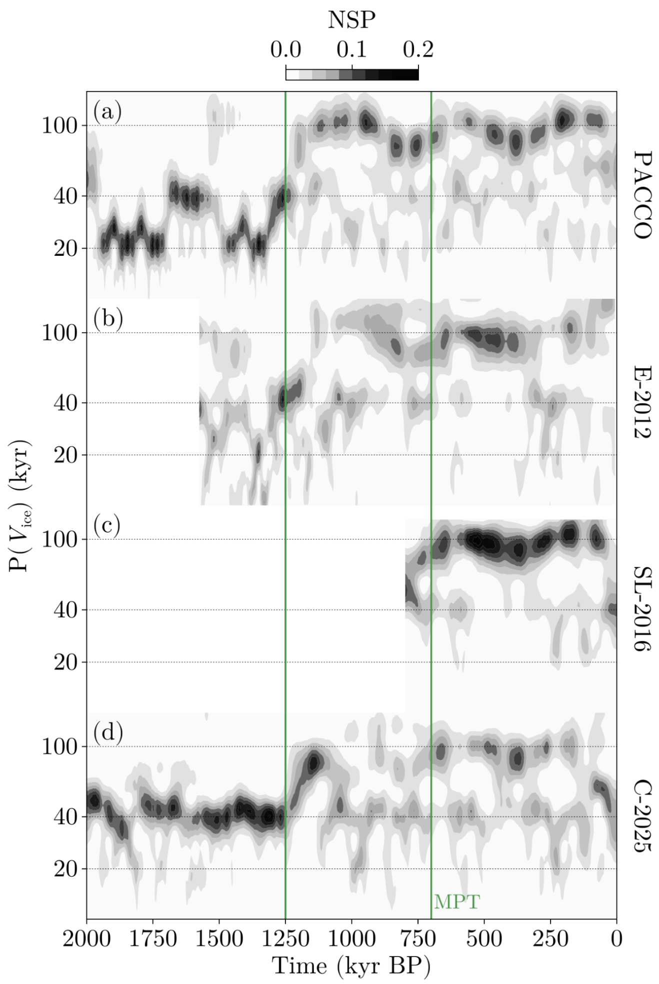
1. All references to Clark et al. (2025, d18O decomposition) have been modified to Clark et al. (2025, sea level reconstruction).
2. We have modified Figure 4 according to change #1.



(modified) Figure 4. Results of BASE experiment: (a) Boreal SSI, (b) regional air temperature anomaly with respect to Tref in comparison with Barker et al. (2011), (c) global-mean sea-surface temperature (SST) from Herbert et al. (2016) and Clark et al. (2024). (d) CO2 concentration compared with Lüthi et al. (2008), (e) ice thickness, (f) sediments thickness, (g) δ18O from

Lisiecki and Raymo (2005), Elderfield et al. (2012), Ahn et al. (2017) and Hodell et al. (2023), and (h) ice volume time series in comparison with Elderfield et al. (2012), Spratt and Lisiecki (2016) and Clark et al. (2025). Note that black curves all represent results simulated with PACCO.

3. We have modified Figure 7 according to change #1.



(modified) Figure 7. NSP of ice volume Vice at each period (P) as a function of time from (a) BASE experiment, (b) Bintanja and Van de Wal (2008, BW-2008), (c) Spratt and Lisiecki (2016, SL-2016) and (d) Berends et al. (2021b, B-2021). Vertical green lines correspond to the interval associated with the occurrence of the MPT (Clark et al., 2006)

4. We have updated the discussion related with the findings of Clark et al. (2025, sea level reconstruction):

3.1 BASE experiment:

As can be seen in Fig. 4, the GIV in BASE is generally in good agreement with proxies, especially the sea-level evolution over the last 800 kyr. Before the MPT, the ice volume presents a slightly lower amplitude than the one suggested by proxies. This is due to the geometric assumption of a circular ice domain that underlies the computation of spatially averaged quantities (Eq. A8), which does not capture the spatial heterogeneity of the regolith-cleared areas. Note that Clark et al. (2025) curve is the first ice volume reconstruction that shows a constant amplitude of ice volume throughout the entire Pleistocene. This particular finding will be discussed in Section 4.

Discussion section:

However, as shown in Fig. 15, a trend in snowfall is capable of producing a change of periodicity along the Pleistocene. In this case, we play directly with the mass balance using snowfall sensitivity to temperature as another forcing. Where does this extra forcing come from? We cannot identify the physical mechanism underlying the change in snowfall sensitivity to temperature because of the simplicity of its implementation. However, one could hypothesize possible mechanisms that could imply such a change: In a warmer climate, the hydrological cycle is amplified, thus being more sensitive to changes in short-scale fluctuations of temperature. Then, if air temperature (governed in these timescales by solar radiation) changes, the mass balance of the ice sheet can be affected more strongly. However, in a colder climate, the hydrological cycle is weakened and less sensitive to orbital fluctuations. Thus, we hypothesize here that considering the sea-level curve from Clark et al. (2025), a possible solution would be to take into account the effect of a progressive change in hydrological sensitivity through the MPT.

…

Lastly, we see the necessity to point out that the recent study by Clark et al. (2025) ruled out the regolith hypothesis proposed by Clark and Pollard (1998). This study claims that the relative importance of temperature and ice volume in the signal of δ18O could have changed over the Pleistocene, thus eliminating the direct consequence of the regolith hypothesis, which is a change in ice volume during the MPT (Fig. 4h). However, in our view, their conclusions remain to be tested in relation with evidence from other reconstructions of the Pleistocene (e.g. ice rafted detritus and other marine sediments, Raymo et al., 1989; Clark et al., 2006; Hodell et al., 2008; Sosdian and Rosenthal, 2009; Rohling et al., 2014; Hodell and Channell, 2016) and, lastly, when comparing both the Pleistocene SSTs (Clark et al., 2024) and the ice volume reconstruction (Clark et al., 2025) an intriguing pattern emerges: pre-MPT and post-MPT glacial maxima appear to have similar sea-level amplitudes while global pre-MPT SSTs are significantly higher than the post-MPT ones. How to build glacial pre-MPT Earth's ice sheets as big as the post-MPT ones while pre-MPT global SSTs remain closer to interglacial values than to glacial ones represents a novel real challenge to climate models and our understanding of climate.

Response to Anonymous reviewer, Climate of the Past, Jan 01, 2026

I thank the authors for providing these revisions. However, my main concerns from the first round remain largely unchanged. I am supportive of conceptual or physically adimensional models as complementary tools to more complex ones, but for the regolith hypothesis, I remain unconvinced of this approach. However, this point was not raised by the other reviewer or by community comments, and the author modelling framework involves substantial work and interesting simulations. Beyond this initial reserve, the most problematic ones are on the discussion part of the manuscript, that clearly still lack structure, contextualization, and limitation acknowledgments:

- *The contextualization with previous studies, which was absent in the initial version of the manuscript, is now reduced to a brief paragraph beginning with "These conclusions match the results both of previous conceptual and comprehensive studies." This clearly lacks nuance. Much of the discussion provided in the response to reviewers is interesting but has not been incorporated into the manuscript.*
- *The discussion section remains insufficiently structured; I would recommend introducing subsections and reorganizing it accordingly.*
- *The limitation related to not reproducing the amplitude of glacial cycles, and its consequences for the timing of the MPT under the regolith hypothesis, is still not discussed in the manuscript.*

Dear reviewer,

Thank you for your comments. Below, we respond in detail to each point. Please note that we have included some additions to the previous version of the manuscript in **orange** and fragments already included from the previous review round in **green**.

Response to Main comments discussion:

1. *The point raised in the new paragraph of the discussion regarding limitations "Thus, it does not take into account that certain regions are cleared of sediments and, therefore, some ice flows on hard bedrock" is entirely valid. I fully agree with this limitation, which is, in my opinion, crucial when investigating regolith evolution and the interactions between the ice sheet and the underlying bedrock. In my opinion, the regolith hypothesis is strongly related to its spatial distribution component, contrary to other MPT hypothesis. Naturally, there is nothing more the authors can do at this stage beyond acknowledging this limitation. On a more minor note, I would recommend moving this limitation to the beginning of the discussion section, immediately after presenting the assumptions regarding the regolith. In its current placement, it disrupts the logical flow of the argument. More broadly, I find the discussion still disorganized, even though it contains valuable content.*

We thank you for your suggestion and we will restructure the discussion section as you suggest:

4.1 Role of the regolith layer and limitations in our approach
4.2 Regolith layer evolution equation
4.4 Carbon cycle
4.5 Insolation metrics
4.6 Role of trends in PACCO's climate

4.7 A constant ice volume amplitude during the Pleistocene?

We have also added some sentences to Section 3.1:

As can be seen in Fig. 4, the GIV in BASE is generally in good agreement with proxies, especially the sea-level evolution over the last 800 kyr. Before the MPT, the ice volume presents a slightly lower amplitude than the one suggested by proxies. This is due to the geometric assumption of a circular ice domain that underlies the computation of spatially averaged quantities (Eq. A8), which does not capture the spatial heterogeneity of the regolith-cleared areas.

2. *I still have concerns regarding the limitation of modelling low-amplitude pre-MPT cycles. I slightly disagree with the authors statement that the comparison with the Elderfield reconstruction provides a better match. From a broader perspective, their model continues to underestimate the amplitude of glacial–interglacial cycles, which in turn artificially constrains the system. What would be the state of the regolith at 1.2 Myr if it had experienced climatic cycles with at least twice the amplitude over the preceding 1.4 Myr? I believe this question and the associated limitation remain unaddressed in the discussion section. The authors claim they do, but I cannot find the specific section of the discussion where they mention it, as they do not cite a specific passage. While the impact of insolation on ice volume is indeed discussed, the consequences of reduced 40-kyr cycle amplitudes on regolith preservation prior to the MPT are not.*

We believe Figure RC1.2 shows the improvement we mentioned in the previous response. However, we understand the point about the uncertainty in the state at 1.2 Myr depending on the pre-MPT amplitude. Nonetheless, the amplitude of the pre-MPT cycles does not have any imprint on the post-MPT world in our simulations, neither with respect to sea-level amplitude nor for their frequencies. This is due to the fact that, once regoliths are depleted enough for the basal sliding to be significantly reduced, the system locks at ~100 kyr periodicities regardless of its pre-MPT amplitude. That being said, it is true that a higher-than-BASE pre-MPT amplitude of the cycles would imply (for the exact same set of parameters) a slightly enhanced regolith removal and thus a slightly earlier MPT. In such a case, a small recalibration of the parameters in sediment dynamics would be required to simulate the MPT at the precise timing. We have included this acknowledgment in the discussion section as it follows:

We have also seen that the selection of the particular metric for insolation forcing facilitates the pre-MPT world to oscillate at 40 kyr. Thus, one could see this as a simple forcing choice problem, but it could also hide the role of the Antarctic ice sheet, the pole antisymmetry of precession cycles or the latitudinal insolation gradient (Raymo and Nisancioglu, 2003; Raymo et al., 2006). Still, metric selection or uncertainty about the pre-MPT world does not have any imprint on the simulated post-MPT world, neither with respect to sea-level amplitude nor for their frequencies. This is due to the fact that, once regoliths are depleted enough for the basal sliding to be significantly reduced, the system locks at 100 kyr periodicities regardless of its pre-MPT amplitude. However, the ice volume amplitude of the pre-MPT cycles does slightly influence the timing of the transition. Variations in the pre-MPT amplitudes or frequencies with respect to BASE require a

small recalibration of the parameters in sediment dynamics in order to simulate the MPT at the precise timing.

Finally, we would like to note that the uncertainty related with the precise timing of the simulated MPT is already expressed in section 3.2, where we explore the sensitivity of the model to the parameters of the sediment layer equation. Note that we modified one sentence to make it clearer:

Nonetheless, we must also highlight the implicit limitations of our approach. Since we do not take into account spatial heterogeneity in sediment distribution, some magnitudes may be overestimated (such as total ice-sheet velocity) or underestimated (such as ice volume). In the latter case, on one hand, Eq. A8 relies on a simplistic geometric assumption. Thus, it does not take into account that certain regions are cleared of sediment and, therefore some ice grows on hard bedrock. Moving more slowly with greater ice volumes. On the other hand, the election of the insolation forcing metric may have and effect on the pre-MPT world, either because of the asymmetry of precession between high latitudes in both hemispheres (Raymo et al., 2006) or because the SSI produces an enhanced response around 20 kyr periodicity preventing the ice sheet to further expand (or gain mass) during longer obliquity-paced cycles (Leloup and Paillard, 2022). However, elucidating the exact cause of the obliquity vs precession pre-MPT ice-volume amplitude issue is out of the scope of this paper. To resolve this, a three-dimensional ice-sheet model should be used in conjunction with a fully operational sediment flux module; however, to our knowledge, no such approach exists to date. This fact leaves room for possible future work.

and also right after

The phase space of the sediment layer equation has been tested for a wide range of the parameters involved in the SEDIM experiment. The imbalance between the quarrying (fv) and denudation (fp) fractions defines the rate at which sediments are removed beneath the ice sheets. Therefore, it determines the timing of the change of dynamic regime. There is no reason for this relationship to be linear and we do not exclude the existence of better representations of the sediment quarrying and generation. However, Eqs. (4) and (5) are enough to show that a gradual, interactive change in the dynamic regime can, in principle, alter the response of the climate system to insolation forcing along the Pleistocene, in agreement with Legrain et al. (2023).

3. *Again, the discussion added in response to my earlier comment remains very limited and does not acknowledge the nuance required to properly contextualize the study. For example, in your response to the reviewer, you discuss the discrepancies between your results and those of Scherrenberg et al. Why is this comparison not included in the manuscript itself? Presenting it would help readers understand why your study arrives at opposite conclusions. Furthermore, the authors claim: "These conclusions match the results both of previous conceptual and comprehensive studies." This is not accurate. The literature does not uniformly support your findings, and this is perfectly acceptable. Scherrenberg et al. reach different conclusions; Legrain et al. propose a gradual CO_2 decline as trigger of the MPT; Willeit et al. require both regolith evolution and CO_2 changes to model the transition, and so on… In my opinion, it is important to present the full range of limitations and divergent results. Doing so would greatly benefit the reader by clarifying why numerous MPT studies*

over recent years disagree (due to differences in modelling assumptions, frameworks, experiment design, etc.). Including this fuller perspective would significantly strengthen the discussion section, even if some earlier studies do not align with your conclusion, which is, again, perfectly acceptable.

We are thankful to the reviewer for their comment. We will modify our comparison with previous studies as follows:

These conclusions agree with previous conceptual efforts such as Verbitsky et al. (2018), in which the authors conclude that the simulated MPT is the product of a change in the balance of feedbacks in the system. In a complementary study, Ganopolski (2024) summarized the relevant processes affecting the MPT and included a change in the dynamic regime of the ice sheets and their size as a critical threshold. Willeit et al. (2019) also showed that the regolith removal could have changed the dynamic properties of the Northern Hemisphere ice sheets, propitiating a periodicity regime shift in their Earth System model. Recent studies have also come to different conclusions. For example, Scherrenberg et al. (2025) concluded that CO2 variations were vital to producing glacial-interglacial variability. However, in our view, their climate forcing index, based on both insolation and CO2 proxies, is what induces such a variability in their approach. In contrast, PACCO only includes one boundary condition, which is the insolation.

4. *The response to main comment 4 begins with "see the related minor comment below." However, when looking for the minor comment in question (that is not specified by the authors), the probable one starts with "see main comment #3." This makes it difficult to follow the authors' response and reasoning. Please provide clearer guidance in your responses, as this would make the review process more efficient and less time-consuming. Regarding CO_2: Once again, the discussion presented in the response to the reviewers is thoughtful and relevant, yet none of it has been integrated into the manuscript. The response ends with "we will acknowledge this fact in the new version of the manuscript and expand the discussion." However, in the revised version, I am unable to find any section where this point is actually addressed. Regarding the hydrological cycle: The same issue arises. There is no new content in Section 3.5 of the updated manuscript, only a discussion provided in the response to reviewers. From what I have seen in the tracked-changes version of the manuscript, none of this valuable material has been incorporated into the manuscript itself.*

First, we apologize for the messy response. We have carefully revised the manuscript, and we do not find the need for including these particular discussions in section 3.5 since the Discussion section already includes them, and we believe this is the correct place for them to be.

Sincerely,
Sergio Pérez-Montero et al.

Comments from the previous round with changes added to the manuscript:

Dear reviewer,

Thank you for your comments, we hereafter 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_2, 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 CO2 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.

Nonetheless, we must also highlight the implicit limitations of our approach. Since we do not take into account spatial heterogeneity in sediment distribution, some magnitudes may be overestimated (such as total ice-sheet velocity) or underestimated (such as ice volume). In the latter case, on one hand, Eq. A8 relies on a simplistic geometric assumption. Thus, it does not take into account that certain regions are cleared of sediment and, therefore some ice grows on hard bedrock. Moving more slowly with greater ice volumes.

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 $\delta^{18}O$ 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 $\delta^{18}O$ 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-inphase 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, there is the chosen insolation forcing metric, either because of the asymmetry of precession across the globe (Raymo et al., 2006) or because the SSI produces an enhanced response around 20 kyr periodicity that does not allow the ice to grow bigger (Leloup and Paillard, 2022). However, elucidating the exact cause of the obliquity vs precession pre-MPT ice-volume amplitude issue is out of the scope of this paper. To resolve this, a three-dimensional ice-sheet model should be used in conjunction with a fully operational sediment flux module; however, to our knowledge, no such approach exists to date. This fact leaves room for possible future work.

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.

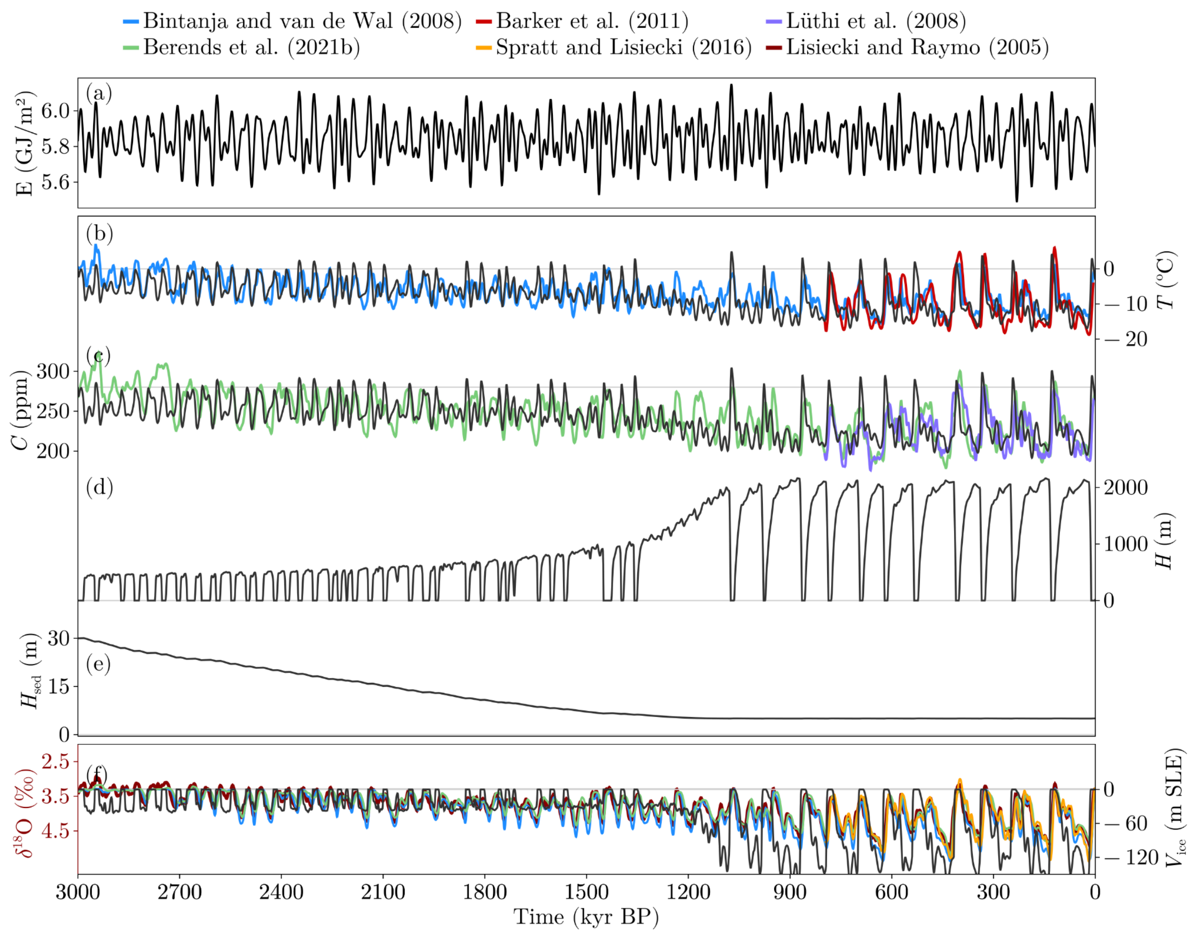
A related question is the choice of insolation forcing. Using an alternative insolation metric (CST or ISI instead of SSI) allows the model to better capture the 40 kyr amplitude, yet in doing so the MPT itself is simulated much earlier than in the paleoclimate records, which is likely due to the larger 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.

Therefore, we conclude that there is an improvement in the pre-MPT world in terms of the simulated periodicity, but not in terms of the amplitude when using CSI or ISI metrics. Thus, for historical reasons (Paillard, 1998; Parrenin and Paillard, 2003; Leloup and Paillard, 2022; Ganopolski, 2024), to reduce the computational cost and to allow for a more direct comparison of the role of different mechanisms (Raymo and Nisancioglu, 2003; Raymo et al., 2006) we continue in the following sections to use SSI as the model forcing. This improvement does not alter our main finding on the important role of regolith quarrying to trigger the MPT, in contrast with decreasing trends in temperature and CO2 , which occur as consequences of changes in the cryosphere in this

model.



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 CO2 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 CO2 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.

These conclusions match the results both of previous conceptual and comprehensive studies. For instance, Verbitsky et al. (2018) simulated the MPT as the product of a change in the balance of feedbacks in the system. Willeit et al. (2019) used regolith removal to change the regime of a three-dimensional climate system. Ganopolski (2024) summarized the relevant processes affecting the MPT and included a change in the dynamic regime of the ice sheets and their size as a critical threshold.

4. I am quite surprised by the absence of effect of a decreasing trend in CO2, 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 CO2 concentrations in our model (see the related minor comment below). However, the CO2 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 CO2 trend on sea-level amplitude is expected because, by construction, the reconstructed CO2 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 CO2 in our case is likely our adimensional assumption: It is conceivable that under high CO2 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 CO2 was motivated by the new deconvolution of $\delta^{18}O$ 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.

Lastly, we see the necessity to point out that a recent study by Clark et al. (2025) ruled out the regolith hypothesis proposed in Clark and Pollard (1998). This study claims that the relative importance of temperature and ice volume in the signal of δ18O could have changed over the Pleistocene, thus eliminating the direct consequence of the regolith hypothesis, which is a change in ice volume during the MPT. However, in our view, their conclusions remain to be tested in relation with evidence from other reconstructions of the Pleistocene (e.g. ice rafted detritus and other marine sediments, Raymo et al., 1989; Clark et al., 2006; Hodell et al., 2008; Sosdian and Rosenthal, 2009; Rohling et al., 2014; Hodell and Channell, 2016).

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.

Proxy records indicate that, around 1250-700 kyr before present (BP), the characteristic climate variability underwent a transition from small-amplitude cycles of 40 kyr to large-amplitude cycles of 100 kyr cycles

Line 31: References are need 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.

The paragraph in question was divided into two.

Line 50: The sentence is quite reductive of the work done, as the authors also test the CO_2 decrease hypothesis.

We agree and we will change it accordingly.

Thus, we will first focus on the regolith hypothesis. To this end, PACCO is extended to represent sediment layer dynamics. Next, we will analyze how the insolation metric used affects our results. Finally, we will explore the role of trends in the carbon cycle and the hydrological cycle in order to reproduce the MPT.

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.

We deleted 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 Tmpt 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 CO2 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 CO2 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 CO2 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 CO2 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.

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Sincerely,
Sergio Pérez-Montero et al.

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Clark, P. U., Alley, R. B., & Pollard, D. (1999). Northern Hemisphere ice-sheet influences on global climate change. *Science*, *286*(5442), 1104-1111.

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