Response to L. Lisiecki, RC1 (https://doi.org/10.5194/egusphere-2025-2467-RC1) Climate of the Past, July 18, 2025

Dear Lorraine Lisiecki,

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

This paper uses a recently published simple model, the Physical Adimensional Climate Cryosphere Model (Perez-Montero et al., 2025), to investigate several hypotheses about the Mid-Pleistocene Transition (MPT). They find that removal of North American regolith caused by repeated glaciations throughout the Pleistocene (i.e., the regolith hypothesis) is capable of reproducing the increase in ice volume amplitudes and switch to longer ~100-kyr cycle lengths in the mid-Pleistocene. The model also simulates increases in glacial-interglacial amplitudes (i.e., decreasing glacial minima) for CO2 and regional surface temperatures as a consequence of ice volume changes, which the manuscript argues demonstrates that a change in CO2 forcing is not necessary to drive these changes. The manuscript also includes many sensitivity tests to explore how the model responds to different parameter choices, insolation forcing, different constant CO2 levels, and other potential climate system changes in order to investigate mechanisms that are and are not associated with transitions in the model.

This manuscript is well written and thoroughly explores the dynamics of the model associated with MPT-like glacial cycle changes. The work is novel, well-executed and likely to be of interest to many paleoclimate researchers. However, there are a couple issues that should be addressed before its publication in Climate of the Past.

Major concerns

For comparing the model results to paleoclimate observations of the MPT, the manuscript (e.g., Figure 4) mainly relies on results from Bintanja & van de Wal (2008) and Berends et al (2021), both of which use forward inverse modeling to infer temperature and ice volume from benthic d18O. This is a concerning limitation of current manuscript because it relies on the accuracy of assumptions in these inverse models. While there are no direct observations of CO2 across and before the MPT, there are direct observations of regional temperatures to which the authors could compare their model results and there are estimates of ice volume that don't rely on the same inverse modeling assumptions. The authors should compare their model results to some of these more observationally based estimates, such as

NH extra-tropical SST records:

Clark, P. U., Shakun, J. D., Rosenthal, Y., Köhler, P., and Bartlein, P. J.: Global and regional temperature change over the last 4.5 million years, Science, 383, 884–890, 2024.

McClymont et al. , Earth Sci. Rev. 123, 173–193 (2013).

Lawrence et al, North Atlantic climate evolution through the Plio-Pleistocene climate transitions, EPSL, 300, 329–342, 2010.

Ice volume estimates:

Elderfield, et al (2012) Evolution of ocean temperature and ice volume through the mid-Pleistocene climate transition, Science, 337, 704–709, https://doi.org/10.1126/science.1221294.

Rohling et al (2014) Sea-level and deep-sea-temperature variability over the past 5.3 million years, Nature, 508, 477–482.

Clark et al (2025), Mean ocean temperature change and decomposition of the benthic d18O record over the past 4.5 million years Clim. Past, 21, 973–1000, https://doi.org/10.5194/cp-21-973-2025

In describing these comparisons, it would also be appropriate for the manuscript to clarify that regional SST records would be expected to systematically differ from temperature over the ice sheet because of different sensitivities to ice sheet height

We thank the reviewer for these constructive comments and have taken them into account. We now show in Fig. RC1.1 some of the proxies suggested compared with the related variables of PACCO. In addition, we attach a new version of Fig. 4 for the BASE experiment (Fig. RC1.2) that includes the comparison with the suggested proxies, where we splitted the last row in order to compare more clearly V_{ice} and $\delta^{18}O$. Concerning the proxies:

- We added Herbert et al. (2016) instead of Lawrence et al. (2010) since their studies are similar and the data for the first one is publicly available but not for the second one (the first author of Lawrence et al (2010) seems to not be in academia anymore).
- Clark et al. (2025) provides a deconvolution of $\delta^{18}O$ in its thermal and sea water components, but not the relative sea level (ice volume), and it is not within our scope or area of expertise to make the conversion required for a direct comparison.
- We added $\delta^{18}O$ from Hodell et al. (2023) that was already included in Figure 1 from the manuscript.

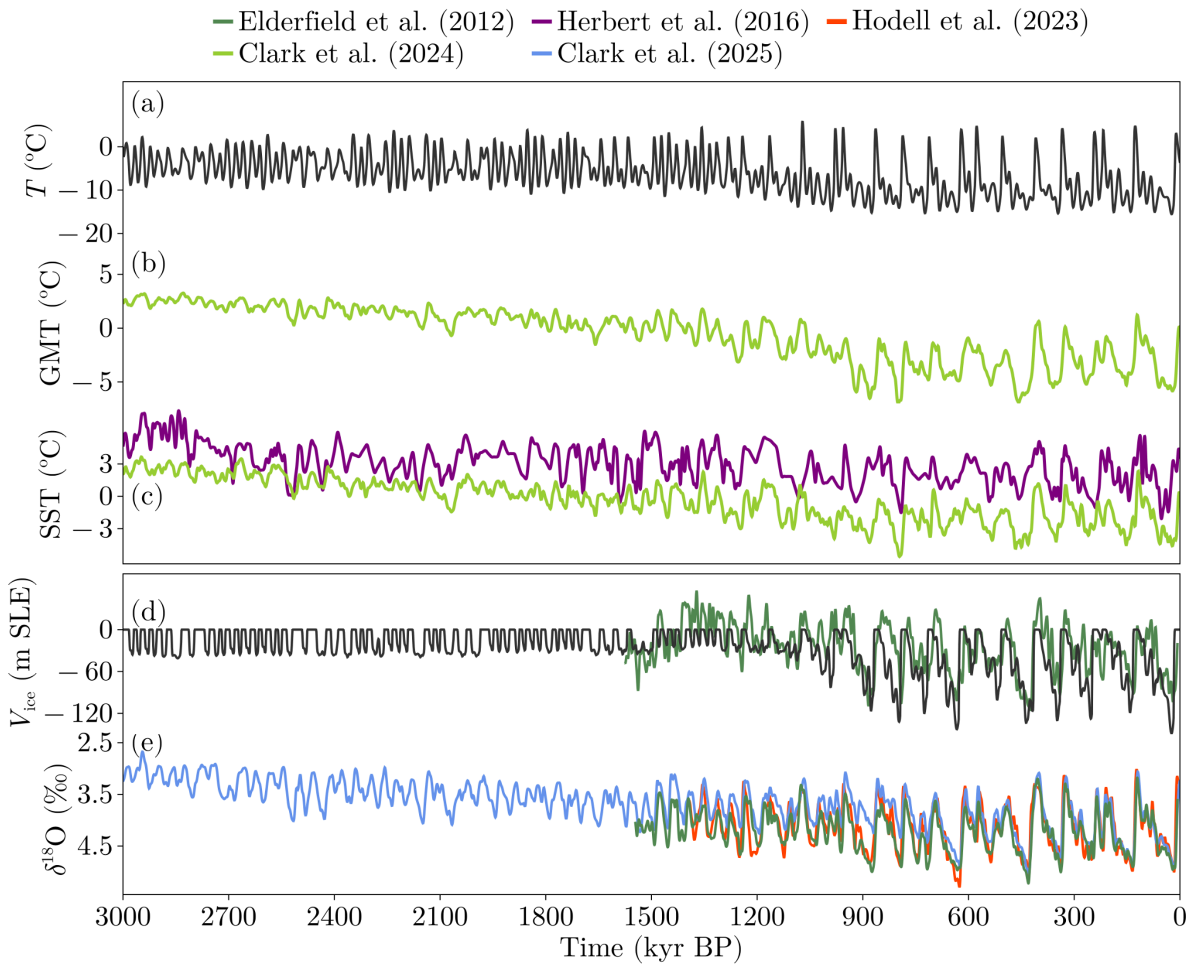

Figure RC1.1. Comparison between the suggested proxy records (coloured lines) and our simulations (black). The new variables included are global mean surface temperature (GMT) and sea surface temperature (SST).

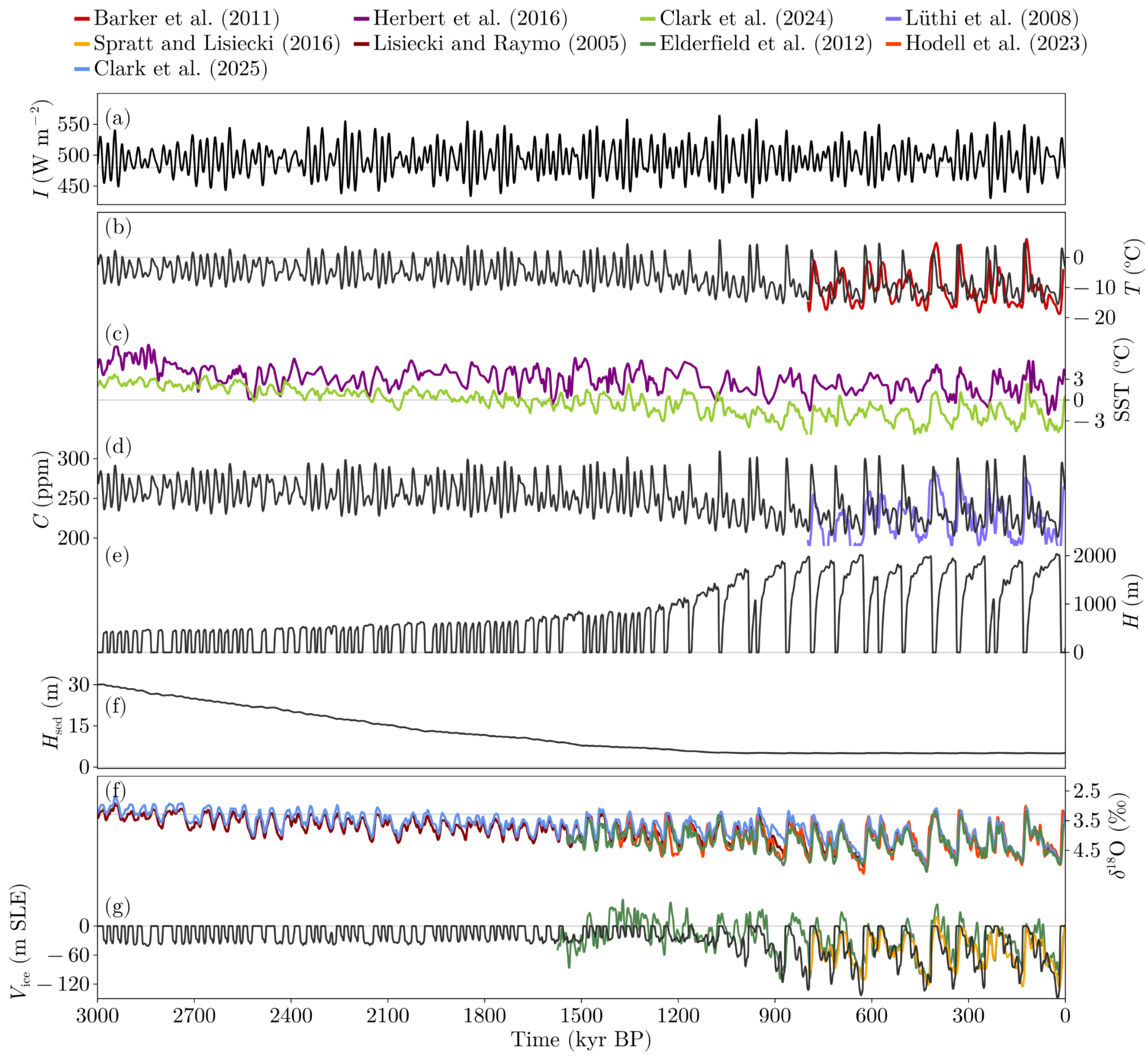

Figure RC1.2. New Figure 4 for BASE experiment.

Because the new pre-MPT ice volume estimates of Clark et al (2025) differ substantially from most other study's reconstructions (and this model's output), I recommend that the authors add some discussion of this new study somewhere in the manuscript.

As far as we know, Clark et al. (2025) have only published the deconvolution of the $\delta^{18}O$ but not the ice-volume estimates, thus we can not compare the sea-level estimates. We agree that the conclusion of their deconvolution is interesting since it could yield pre-MPT and post-MPT ice volume changes of the same amplitude while conserving the 40 and 100 kyr periodicities. This is however controversial since it conflicts with the regolith hypothesis proposed in Clark and Pollard (1998). However, we think that other proxies such as the ice rafted detritus also need to be explained by this new hypothesis. We have therefore included a paragraph on this issue in the discussion section.

More generally, the model seems to produce very small ice volume estimates right up until the MPT (e.g., see 1500-1200 kyr BP in Figure 4f). Do the authors have any comments about this aspect of the *model response? Is the model specifically producing estimates of Laurentide ice volume or all NH ice sheets?*

It is important to note that our ice-volume equation is based on a very basic assumption concerning ice-sheet geometry. Thus, our ice-volume estimates cannot be compared one to one with more sophisticated reconstructions of this magnitude. The reason behind the low values of ice volume before the MPT is related with the geometric assumption that relates ice thickness to the horizontal profile. This relationship helps with ice dynamics but strongly limits how much ice the ice sheet can store. The model represents the ice volume of the Northern Hemisphere as a whole, but neither of these ice sheets were purely symmetrical. Therefore, the comparison must be qualitative rather than quantitative. It is also important to say that in reality, the regoliths were spatially distributed and removed. Thus, some parts could accumulate more ice as the regoliths were removed sooner in some places than in others. Therefore providing bigger ice volumes than our equation. On the other hand, our simulated pre-MPT ice volume is indeed infraestimated compared to proxies. Please see our response to referee #2 (RC2), where we expand on the influences of the summer solstice insolation forcing and the absence of the Antarctic ice sheet on this underestimated ice volume previous to the MPT.

Minor comments

- *Lines 205-207: The sentence at the bottom of page 16 is somewhat difficult to parse. One way to clarify it (I think) would be "This improvement … trigger the MPT, whereas the decreasing trends … occur as consequences of changes in the cryosphere in this model."*

We agree, thanks for the suggestion.

- *Lines 223-236: The authors show model results for different CO2 trends in Figure 14 and on line 228 summarize the results by saying the carbon cycle affects the amplitude of GIV but "has little influence on the periodicities." However, it is difficult to discern the periodicities of glacial cycles in Figure 14c, particularly because results from different model runs are overlain. The manuscript should provide more specific documentation of constant periodicity between the early and late Pleistocene in these simulations, perhaps by plotting NSP for a couple of the TREND-C experiments (e.g., similar to Fig. 15d).*

We agree, thanks for the nice suggestion. You can see in Fig. RC1.3 that the frequencies are very similarly distributed between cases. The only change is related with the inception and the amplitude. Moreover, we think that this figure can be a good addition to the appendix of the paper.

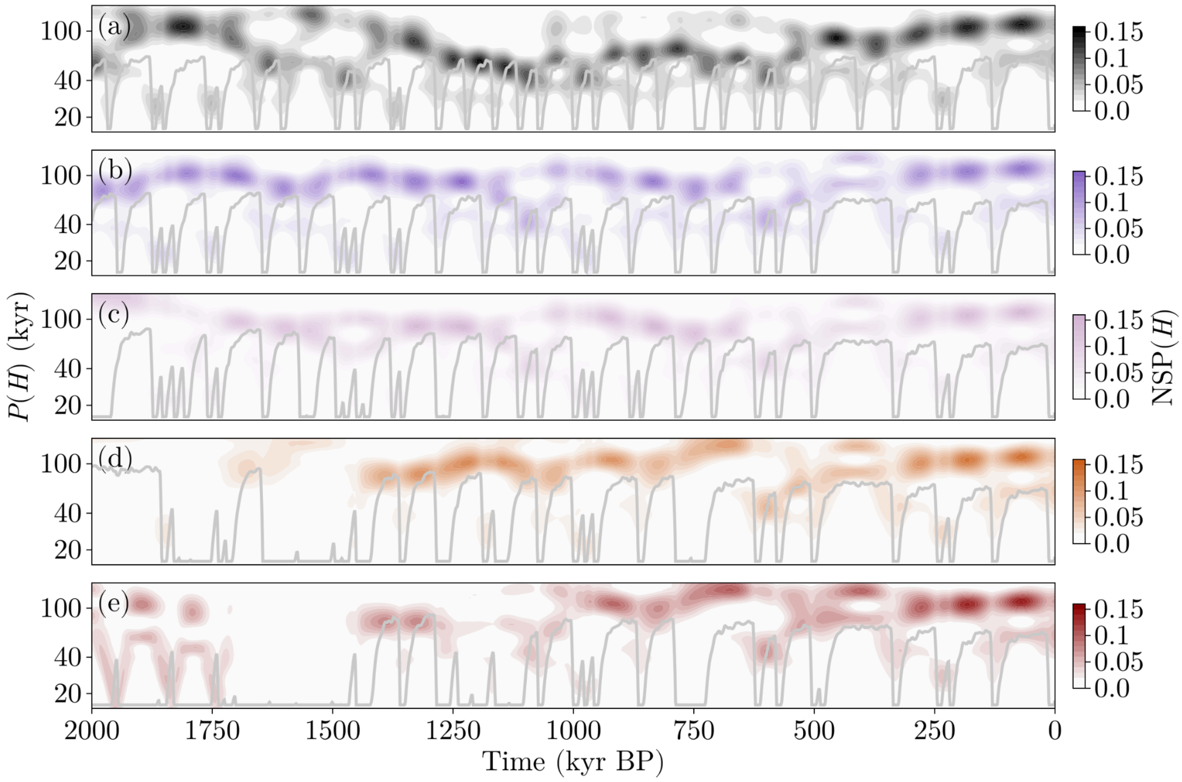

Figure RC1.3. NSP as a function of time for NOSEDIM-low and 4 emblematic cases of the TREND-C experiments. Note that the associated H is plotted in grey as a reference. (a) Interactive carbon cycle and the trend applied in the rows below is (b) -0.05 ppm/kyr, (c) -0.12 ppm/kyr, (d) -0.20 ppm/kyr and (e) -0.23 ppm/kyr.

- *Line 266: Please specify which "two parameters" are referred to here.*

We meant fv and fp; we will clarify this in the new version of the manuscript.

- *Line 289: The sentence "We cannot say which is the physical mechanism behind because of the simplicity of its implementation" is unclear. Perhaps one or more words is missing after "behind."*

We will change this to "We cannot identify the physical mechanism underlying the change in snowfall sensitivity to temperature because of the simplicity of its implementation." in the new version of the manuscript.

- *The citation for Perez-Montero et al. (2025) should be updated to the final, published version instead of the pre-print.*

That is right, the article was published after submitting this manuscript. We will update the citation.

Sincerely,
Sergio Pérez-Montero et al.