Summary of changes according to reviewers comments

Michel Crucifix comments:

1. Modifications on abstract.
2. Reference to Broecker and Van Donk (1970, RoG) to introduction.
3. Better justification on why we do not include eccentricity in the artificial forcing.
4. Sect. 2.6 is modified according to suggestions.

Antonjia Rimac comments:

1. We have looked for additional clarifications about the dependence of periodicities with the sliding parameters as suggested.
2. We have added a new appendix with the absolute periodograms as suggested.

Dear Michel Crucifix,

Thanks for your comment. We hereafter respond to your editorial comments:

I would like to thank the authors for having considered my comments and modified their original submission accordingly. This proved to be a stimulating exchange.
I'm overall very satisfied with the result and encourage publication. Below are a few editorial suggestions (plus one small scientific question about tau_alpha):

- abstract: 'avoid "in terms of". Just wriet: "Most of them capture well the shape..."

We agree and we will change it accordingly in the new version of the paper.

- in the Milankovitch introduction: my understanding is that Broeker and Van Donk, Reviews of Geophysics (1970) were the first to publish clearly emphasise the saw-tooth shaped of the four latest glacial interglacial cycles.

We will add this reference to the introduction.

- section 2.1 : It is not clear that the very small globally averaged insolation anomaly caused by eccentricity is negligible for all aspects of the climate system, but it is indeed reasonably and conventional to ignore it for the mass balance of the ice sheets. Furthermore, eccentircity shows up as the 'beating' of the precession signal and that must be important for the response of the climate system, during and before the Pleistocene. Perhaps just write: "is negligibe in the present context".

You are completely right and we will modify that phrase of the paper.

- In section 2.6 it is written twice that the configuration "tries" something. It is a bit inelegant / inadequate. Perhaps : "this model configuration accounts for the warming at the base" and "this configuration tends to enhance".
- also in this section: "periodicities slower than the forcing" -> "response periods longer than that of the forcing"

We thank you for your suggestions and will include them in the revised version of the manuscript.

- In section 2.7: it is nice to have resolved the prognostic equation for the albedo and replaced it by a diagnostic equation. It is up to the authors, but I would have found instructive to keep the original prognostic equation as a justification of the diagnostic equation.

We also liked the prognostic formulation. However, discussions with A. Ganopolski convinced us that it was more useful to employ just one equation that includes an explicit and directly the timescale associated with the physical process. In the former formulation the particular time scale was hidden in the nonlinear relationship between the prognostic and the diagnostic equation. We think that the interpretation is more straightforward now, while keeping the same physical meaning.

- All the very long nominal tau_alpha time scale (50 kyr) is a little bit puzzling. Is this considered as a realistic duration?

In Willeit et al. (2018), authors assess the effect of different properties of dust such as the size and its origin in the darkening of snow albedo. In that paper, a timescale for the albedo reduction can be derived from Figure 4a. There, a reduction of about 0.1 is produced in approximately 10 kyr, thus, a reduction of 0.5 (from fresh snow, 0.9, to the selected old ice albedo, 0.4) is expected to be in about 50 kyr. In our case, 50 kyr (in Fig 15 of the manuscript it can be seen that around 30 to almost 60 kyr, the agreement is good) is the timescale that allows a good synchronization with the forcing to produce the right glacial-interglacial variability. We need to have in mind that, in our model, this timescale could include the role different mechanisms, for example: dust deposition due to drier climates (Ganopolski and Calov, 2011, Willeit et al., 2018) and/or regolith-based regions (as in Willeit et al., 2019), increase of supraglacial rivers (Pitcher et al., 2019, among others) above the ice sheet due to the increase of insolation, black carbon deposition or even the growing of algae in the surface of the ice (Hotaling et al. 2021, Willeit et al., 2024, among others). We nevertheless agree that it is puzzling in the sense of being highly uncertain and that it potentially includes different physical processes. We hope that future investigation into this matter with more detailed modelling on the dust cycle will shed light on its associated importance and time scale adjustment for glacial cycles.

We hope our answers clarify your concerns.

Best regards,
Sergio Pérez-Montero et al.

References:

Ganopolski, A., & Calov, R. (2011). The role of orbital forcing, carbon dioxide and regolith in 100 kyr glacial cycles. *Climate of the Past*, *7*(4), 1415-1425.

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Willeit, M., & Ganopolski, A. (2018). The importance of snow albedo for ice sheet evolution over the last glacial cycle. *Climate of the Past*, *14*(5), 697-707.

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Willeit, M., Calov, R., Talento, S., Greve, R., Bernales, J., Klemann, V., ... & Ganopolski, A. (2024). Glacial inception through rapid ice area increase driven by albedo and vegetation feedbacks. *Climate of the Past*, *20*(3), 597-623.

Dear Antonija Rimac,

Thanks for your comment. We hereafter respond point by point:

Authors investigated Pleistocene glacial cycles that are known to be driven by the nonlinear response of the climate to solar forcing. Though complex models accurately simulate these cycles they are computationally expensive. Simpler conceptual models lack physical detail, though. The Physical Adimensional Climate Cryosphere mOdel (PACCO) used in this study aims to balance complexity and simplicity by focusing on the interaction between climate and Northern Hemisphere ice sheets. Authors show that PACCO effectively reproduces 100,000-year glacial cycles by incorporating ice-sheet dynamics, thermodynamics, and ice aging. The study reveals that ice aging and delayed isostatic response are key to matching geological records, making PACCOavaluable tool for studying glacial cycles.

Suggestions for the improvements:

Though I see the improvements made to this manuscript some of my comments are still not fully addressed and thus, I cannot recommend this article for a publication just yet. My comment about the energy spectra and peaks that we see at 60 and 100 kyears still stands; it is not a general observation that these peaks are present, but it really depends on the sliding parameter, and it mainly seems that the peaks are present for sliding parameter of 10^-6 m yr^-1 Pa^-1, the question is why?

The 60 and 100 kyr peaks are present because they are a combination of precession (20 kyr) and obliquity (40 kyr). $C_s = 10^{-6}$ m yr^{-1} Pa^{-2} seems to be the threshold between a highly reactive configuration (and a less reactive configuration) to the insolation forcing as a result of having sufficiently high (low) basal sliding which facilitates (delays) deglaciations around 60 kyr. This is why we get a dispersion in the periodogram associated with that particular value of the sliding parameter. We believe we had discussed the dependence of the ice sheet periodicity on the sliding parameter thoroughly enough through the manuscript when analyzing the different PACCO configurations and thus largely responded to your question. For example:

- Line 209: "if the ice sheet is dynamic enough (i.e. high values of the sliding factor C_s), the model resonates to certain multiples of the insolation fundamental periods"
- Line 269: "The sliding strength modulates the amplitude of the ice thickness and thus, the ice-sheet sensitivity to full deglaciation. With this, we produce a more realistic 100 kyr periodicity in H than in RISOS when also using real insolation forcing."
- Line 326: "We saw that two regimes are produced: one produces deglaciations but the other one remains in a state with no glacial terminations. Thus a value above C_s=10^{-6} m yr^{-1} Pa^{-2} is needed to allow GIV."
- Line 393: "On one hand, ice-sheet velocity determines the responsiveness of the ice sheet to the insolation forcing. The strength of sliding affects the level of ice-sheet sliding and thus facilitates its synchronization with the forcing via ice discharge. Therefore, the periodicity of the system response varies with sliding"

Additionally, we are confident that the description of the basal sliding velocity was clear enough to understand its dependence on the sliding parameter:

- Lines 141-147: “The basal velocity field is assumed to follow a Weertman-like sliding law (Cuffey and Paterson, 2010; Pattyn, 2010; Pollard and DeConto, 2012): … In the spatially adimensional simplification, this becomes: … where C_s is a model parameter that represents the “raw” sliding coefficient derived from Pollard and DeConto (2012) and f_{str} is a model parameter that represents the fraction of ice streams in the ice sheet.”

Nevertheless, thanks to your concern, we will carefully inspect the text to seek places of further clarification on the dependence of basal sliding on the system periodicities.

Also, what happens above 100 kyr? The energy spectra are extremely dispersed, so what does that mean? Wouldn't we expect that at one point the energy goes to zero as we mainly see with the higher values of the sliding parameter? Additionally, I think that the authors put themselves in a trap by submitting Figure 1 in their rebuttal as I have an additional question now in connection to that figure. Here, it is not explained why there is such a big change between energy spectra of the ice thickness between different experiments.

On one hand, by definition, the energy ($E \propto H^2$) and power ($P \propto H^2/f$, f is the frequency) are quadratic with the ice thickness in the Fourier transform. Thus, it is expected that in the absolute periodograms in the Figure 1 of the response we get a quadratic dependence on the ice thickness and that is precisely that big change. On the other hand, the aim of this work is to study the main physical processes that allow the Earth System to produce Glacial-Interglacial Variability (GIV), thus we focus on those particular periods (0-200 kyr to allow for a wide range including the uncertainty associated to the simplicity of the model), and most importantly, the relative weight of the peaks in the system response to insolation. Time series and their respective periodograms are shown together for a reason, and it is for a direct comparison between time evolution and its frequency-space distribution. Taking a careful look at those runs that present energy above 0 for periods bigger than 100 kyr it can be seen that those simulations do not deglaciate or deglaciate less than the rest. The dispersion is associated with the nature of applying a Fourier transform to a finite time series: if the series do not oscillate enough, the periodogram shows a certain energy at the period of the entire length of the series. In fact, regarding your comment about energy tending to zero, it can be seen that the higher the sliding parameter, it is most likely that the energy above 40-60 kyr goes to zero (reddish colors of Figs. 3, 5, 7, 9, 11, A1 and A2 in the manuscript). However, we reiterate that the particular values of the spectral power are not relevant for the purpose of this work, but rather the relative importance of the peaks.

I think that this Figure 1 is now a great asset to the manuscript and it should be kept as it gives a great overview of the performed experiments and changes the complexity of the model brings to the results, still it needs to be made in a systematic way.

We understand that you see the asset of that particular figure to the manuscript and we could add it to an appendix of the paper. However we do not see the relevance of replacing it to our normalized spectra since as we said in the previous response, we are confident that the particular values of the spectral power (and its units) are not relevant. What is informative is the relative weight of each Milankovitch band and its multiples. Therefore, we believe that normalized spectra are more appropriated to illustrate the information relevant for the study. This is a practice widely common in the literature devoted to glacial cycles: e.g. Paillard (1998), Clark et al. (2006), Ganopolski and Calov (2011), Willeit et al. (2019).

Also, many of the figures need replotting as they are not done in a systematic way (looking at the axes, colours, etc.)

We disagree: All the experiment figures are built in the same way (Figs. 3, 5, 7, 9, 11, A1 and A2 in the manuscript) with the same range of the explored sliding parameter and colors. The vertical axis of the time series was scaled if needed because of the particularities of each configuration. The only two experimental cases where we change the template are Figs. 8, 13, 15, 16 and 17 since these plots do not aim to show the dependence on the sliding parameter. If the reviewer has any specific suggestion on what to replot and according to what, we will be happy to consider it.

We hope our answers clarify your concerns.

Best regards,
Sergio Pérez-Montero et al.

<u>References</u>

Clark, P. U., Archer, D., Pollard, D., Blum, J. D., Rial, J. A., Brovkin, V., ... & Roy, M. (2006). The middle Pleistocene transition: characteristics, mechanisms, and implications for long-term changes in atmospheric pCO2. *Quaternary Science Reviews*, *25*(23-24), 3150-3184.

Ganopolski, A., & Calov, R. (2011). The role of orbital forcing, carbon dioxide and regolith in 100 kyr glacial cycles. *Climate of the Past*, *7*(4), 1415-1425.

Paillard, D. (1998). The timing of Pleistocene glaciations from a simple multiple-state climate model. *Nature*, *391*(6665), 378-381.

Willeit, M., Ganopolski, A., Calov, R., & Brovkin, V. (2019). Mid-Pleistocene transition in glacial cycles explained by declining CO2 and regolith removal. *Science Advances*, *5*(4), eaav7337.