Response to Anonymous referee comment (RC2)

Dear Anonymous referee,

Thank you very much for your comments. 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 PACCO a valuable tool for studying glacial cycles.

Major comments on the paper:

I have to say that I like how the paper was written. It goes from a simple model, where linear insolation forcing is introduced to the one-dimensional model as described by SIA equations. I do not agree with RC1/CC1's description that it even might have 2 dimensions, as here H is not a dimension but merely a product of equations. I only miss here the motivation for all constants and order of complexity they introduced to the model. For example, in table 2, Length is 1000 km based on Oerlemans et al., 2008. But why is simply that taken and not given a reasoning? The same goes for many equations that are not fully motivated. Please note that I am a big fan of simple conceptual models as I believe that we can learn more from simple yet fast models compared to expensive and long-time running Stokes models.

We thank you for your comment. The motivation behind the gradual increase in complexity is to obtain glacial cycles with good amplitude and timing with respect to the proxies. For that, we analyzed each step in order to fully understand in which way each process affects the variability of the system. We understand that this intention might not be as clear as we thought and we will elaborate more on the justifications in order to make it more clear. For that, we have extended the justification of the following equations:

- Equation (1) is a mass conservation equation that takes into account mass inputs and outputs, similar to the one employed in Benn et al. (2019).
- Equations (2) and (3) are combined into one equation with explicit definition of each component. We also added a new equation (based on the comments from CC1, CC2, CC3 and RC1) that explains the typical horizontal scale of an ice sheet and the approach followed to obtain the parameters employed.
- Equations (7) and (8) are now related with the basal stress in a different manner. In the new version of the manuscript we have an equation for the driving stress and another one for the basal stress. The reason is basically that the ice profile associated with those dynamic regimes is different and two equations are needed if we are to follow the approach suggested by M. Verbitsky in CC1. After presenting the equation we explain that the ice profile of a region dominated by basal dynamics is mostly horizontal and, thus, a constant slope in the basal stress is assumed.
- Equations (9) and (10) are justified now explaining that they represent how the ice deforms under its own weight.
- Equation (16) is motivated by the effect of temperature on the amount of allowed moisture in the atmosphere: more temperature, more moisture.

- Equation (21) is now justified by explaining the interaction between the ice and the bedrock. If the ice sheet grows, the bedrock sinks. However, this is not instantaneous but takes a certain amount of time given by the relaxation time, which is a parameter of the model.
- Equations (29-34) have been simplified and explained in detail:
  - We introduced the notion of Péclet number, its relation with Hb and H as suggested by CC2.
  - We have included the advective heat contribution and explained its formulation.
  - We have added a phrase that summarizes the purpose of the entire THERM configuration, that is basically "to mimic the warming at the base of the ice sheet and to slow down its effect in order to represent how this mechanism is propagated through the entire ice sheet."
- Equation (38) and (39) have been simplified to one diagnostic equation that clarifies how albedo ages in time. This change was made thanks to a personal communication from A. Ganopolski.

Then, from section 2.2 onwards, authors speak of periodicities at time scales of 60-80-100-120 kyears. I must disagree as, e.g., Figure 5 shows pronounced peeks around 20 and 40 kyears for all values of the sliding parameter, and around 60 kyears for lower values of the sliding parameter. Above that, there is no significant peak which might suggest that what we see is just a model artifact or an aliasing error. And this is additionally supported with the figure 5c. The same holds for figure 7 (ISOS simulation). When we go to figure 9 (RISOS simulation), the authors statement does not hold at all, because the peaks really depend on the sliding parameter and cannot be generalized. So here, for me, the result of AGING simulation is the only result that can claim that there is a periodicity between 80-120 kyears (the correct time really depends on the sliding parameter).

We agree in the sense that it is true that the more remarkable peaks are found at 20 and 40 kyr. However, when we mention multiples of those periodicities we refer to the appearance of peaks around those multiples, but not its dominance over 20 and 40 kyr. In this way, we tried to show how adding different mechanisms favors the appearance of longer periods. This can be seen when inspecting the results of the different experiments in order of increasing complexity. We understand that our intention was not as clear as we thought and we will clarify this issue in the new version of the paper.

Another thing that should be changed in the following manuscript is explanations and captions of all figures. For example: we can see a change in dominant peaks which is logical as the authors introduce the model complexity. However, it is not explained why the energy of the peaks for the same sliding factor changes between the runs (see for example the difference in peaks at 60 kyears between Figure 5 and Figure 7. Also, here the vertical axes would really be beneficial to make a good comparison.

We did not add them in the first place since we think the comparison must be made within each configuration and between different values of the sliding parameter, as the model changes between configurations. Nonetheless we include a figure below with the absolute PSD (power spectral density) of every experiment (Fig. 1). The comparison between configurations should only be made in the appearance or not of periodicities longer than the ones provided by the forcing, always having in mind the evolution of the variables in time. This comparison allows us to illustrate how, as you pointed out above, adding complexity increases the nonlinear response, which is basically the aim of the paper structure.

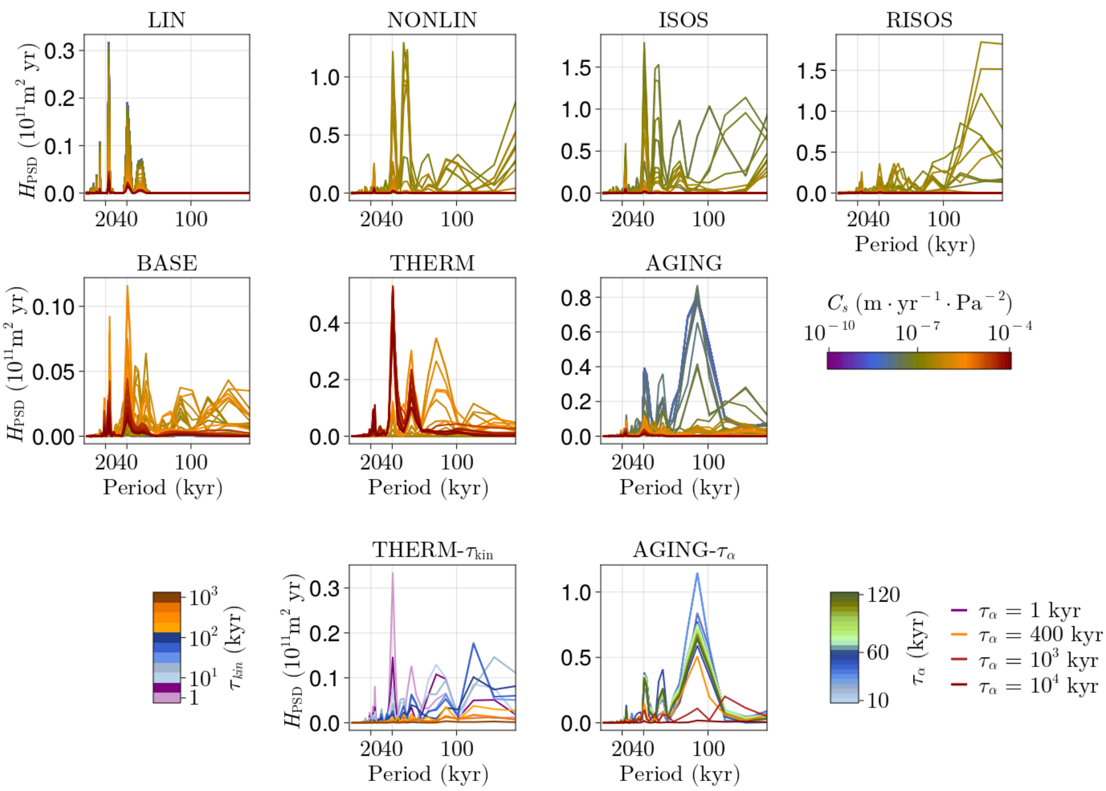

Figure 1. Power spectral density of ice thickness (H_{PSD}) for each experiment performed. Note that the vertical axes are different for each panel and that the colors change according to each experiment. Note also that these are the results after the comments following the open discussion and both referees.

Additionally, the authors did explain why we see differences in periodicity between the sliding parameters, and why their statements are "true" for the lower values of the sliding parameter. There to say, for which glacier could we use these results to possibly predict the future evolution?

Our model was conceived to reproduce the interaction between Northern Hemisphere ice sheets and climate. Different modeling studies (e.g. Clark and Pollard, 1998; Willeit et al., 2019; among others) rely on the regolith hypothesis. This hypothesis basically indicates that a gradual reduction in the capability of the ice sheets to slide during the Pleistocene made them slow and less dynamic, leading to larger ice sheets and a nonlinear impact on late Pleistocene glacial cycle frequency. In our study we find this to be true for AGING configuration. When the sliding capacity is not very high, our model is capable of synchronizing more with the paleoclimatic proxies. To answer your last question, PACCO could simulate the evolution of the Northern Hemisphere in the long-term future.

Thus, to conclude: I think the paper is a valuable contribution to understand the glacier cycles and ice-sheet dynamics. However, I would not consider it for publication just yet. 1. The model and its equations need to be explained and introduced better as as CC1 says: "it looks like a verbalized computer code". 2. The figures are not "self-standing" and need a better explanation of all lines, better comparison between experiments, and y-axes would be beneficial. 3. Better discussion is necessary as it is not obvious how this model can be used and what is a future of it.

Thank you for your final comments which are very useful to better formulate the message of the manuscript. To this end we will:

1. Explain and justify more deeply every equation in the paper.
2. Describe better the figures and add the vertical axes to periodograms to allow a better comparison between experiments.
3. Add a deeper explanation of the possible applications of the model such as the research about the Middle Pleistocene Transition or the Anthropogenic future.

Sincerely,

Sergio Pérez-Montero et al.