Response to Michel Crucifix referee comment ([RC1](#))

Dear Michel Crucifix,

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

Review of « a simple physical model for glacial cycles » by Sergio Pérez-Montero.

    1. General impressions

    The contribution of the authors is a very welcome addition to the literature of models of glacial-interglacial cycles. The model that they introduce, called PACCO, features 0-d ice sheet dynamics along with a simple linear predictor of CO_2 concentration. It is argued that it is as an adequate device for "testing the different hypotheses and isolating them from each other".

    1.1 Mechanism attribution

    How much we learn about physics and balance of mechanisms in a simple model is an interesting question. I would suggest the following argument. Consider a model with thousands of lines of codes, with the representation of highly detailed mechanisms based on physical principles and parameterisations. Suppose then that this model successfully reproduces one or several glacial cycles. For sure, such achievement required some tuning (the Saltzman 1990 argument), but one might argue that, in such a big model, the relative importance of different components or mechanisms in the resulting ice age dynamics is fairly robust. It does not mean that it is faithful to the truth, but that it is hard to change this balance by tuning. Now consider a low-order model with, say, 10 state variables such as PACCO. One might suspect that different choices of parameterisations or even different assumptions may have a large impact on the relative role of different components. This is both the strength and the weakness of the low-order models. A strength, because it offers opportunities of experimentation and addresses sensitivities not apparent in the bigger model; a weakness, because as different assumptions might converge to similar mathematical expressions, interpretation is not as straightforward as it may look. This modelling ambiguity was I would argue perhaps most severe in Saltzman - Maash models (e.g. Saltzman and Maash 1990, hereafter SM90), where only one out of the three differential equations was non-linear (the one for the carbon cycle); the carbon cycle was therefore by design given the responsibility of causing the limit cycle.

For sure, PACCO has more physically explicit mechanisms than SM90, but I suspect caution must still be exercised when attributing a "role" to a component --- I will make a case below that perhaps the respective roles of isostasy, basal melt and snow/ice aging might be sensitive to questionable assumptions (through comments 1.2 and 1.3).

Before going further, I would suggest the authors to have a look and perhaps position themselves with respect to two articles which they did not cite, yet I suspect that have been somehow influential --- because some of the comments I am making here have already been made there, and also because of snow aging was already the attention of a controversy at the time:

    -     Gallée et al 1992 introduced what they called a "sectorially" averaged climate model, with different parameterisations affecting the heat balance of the ice sheet including snow aging (based on previous works by L.D.D Harvey --- also relevant for your section 2.7)

    -     Tarasov and Peltier 1997 reported a simulation of the latest termination with an energy balance model coupled to an ice sheet model. They commented the Gallée et al. 1992 paper as well as other, similar efforts, and specifically, they made the following point: "Although the addition of each of these plausible mechanisms [they quoted here the list of mechanisms] to the reduced model has led to improvements in their

We understand your argument and we mostly agree. In a simplified model like PACCO, isolating the relevant processes and illustrating their relative importance is subjected to the potential lack of accuracy inherent to reducing the problem to a few differential equations without explicitly solving the spatial dimensions. To illustrate this, one could think of a deglaciation of the Northern Hemisphere ice sheets: it is likely that the ice darkening process would be highly relevant for the southern ablation borders of the ice sheets, whereas the effects of a basal thermally driven acceleration of ice streams would be more relevant for inner parts of the ice sheets, both of them finally contributing to the nonlinearities at the core of deglaciations. This spatially heterogeneous phenomenon would not be captured by a model with reduced spatial dimensions as PACCO, in which solely the "mean" state of the ice sheet is simulated. Therefore we agree that conclusions on the relative importance of the mechanisms have to be drawn with caution and we have expanded on this in the new version of the manuscript.

That being said, we have built PACCO by including all the suspected relevant processes for glacial cycles in the simplest, yet keeping a physically-based approach. By doing so, we believe that we have illustrated in a novel way how progressively including different mechanisms translates into the gradual emergence of periodicity peaks of the climate-cryosphere response that are not present in the insolation forcing alone. However, we were perhaps not explicit enough on how well VCV18 already captured the timing and shape of glacial cycles and we have better acknowledged this in the new version of the manuscript. Indeed, PACCO's THERM experiments also perform satisfactorily (see former figure 13), and have now been considerably improved thanks to the implementation of the suggestions made by Mikhail Verbitsky (see figure CC2.1 in our response to CC1 and CC2 comments). Nonetheless, as described in the manuscript, the inclusion of a progressive decrease in surface albedo resulting from ice aging facilitates in our model a good calibration of the glacial cycles rhythmicity shown in proxies.

Following your suggestion on mechanism attribution, we have now added the references to Gallée et al, 1992 and Tarasov and Peltier 1997.

We agree. Indeed, in our simulations the elevation feedback (NONLIN set of experiments) as well as isostasy (ISOS) favor the emergence of the asymmetry, which is considerably enhanced when considering the effects of the thermal basal state on ice sliding (THERM) by means of a delayed feedback. The characteristic time for the base to become temperate, thus enhancing sliding, depends on the vertical diffusion within the ice column, the geothermal heat flow and the vertical advection. This is better captured with the improved version of THERM thanks to M. Verbitsky's suggestions (see details in CC2). Using this approach the base gets tempered relatively fast as the analytical study of Moreno-Parada et al. (2024) shows. The delayed feedback associated with the basal thermal state is now largely explored in terms of the (dynamic and dependant on accumulation rate) Péclet number and the relaxation time of ice-stream activation when the base becomes temperate. Thus, even though the thermodynamics is now more robust our conclusions remain similar. The new version of the manuscript will reflect these improvements. Please see also the responses to comments CC1 and CC2 and also figure CC2.1

As you say, fixing L in the ice-sheet dynamic equations overestimates the driving stress during the second half of the cycle and thus changes ice velocity. Thus, as M. Verbitsky suggested, we introduced an ice profile of the form $L = c * H^2$. We have therefore changed the formulation of deformational velocities accordingly. Please see AC1, AC2 and AC3 for more details. The new version of the manuscript includes these corrections.

We acknowledge all your comments. We have now modulated and nuanced our discussion and conclusions with respect to attribution to specific mechanisms when preparing the new version of the manuscript.

Editorial Comments

1. Introduction

- Some comments about the introduction have already been made by Mikhail Verbitsky. Just adding here that Milankovitch did not "postulate" that glacial - interglacial variability is caused by changes in the insolation. Milankovitch can be seen, as summed up by Berger (2021), as the father of (paleo-)climate modelling. He reasoned on the physics of the radiative balance and effectively computed the variations in temperature to obtain an "effective snow line" (expressed as a latitude) caused by the quasi-periodic changes in insolation. The "postulate" about the astronomical origin of ice ages has to be found earlier (specifically, speaking of the attribution of ice ages to summer insolation , J.J. Murphy 1876. See reviews by Berger 1988 and Berger 2021.)

Thank you, we have corrected this in the new version of the paper.

- The phrase "conceptual model" is widely used, but it may be a bit ambiguous. Perhaps use the distinction between "phenomenological" (describing the observations in a way consistent with the theory but not derived from it --- Paillard 1998 would be phenomenological, for example) and "theoretical" models? In that sense PACCO is indeed a theoretical model with some empirical parameterisations; Verbitsky et al. 2018 also present their model as a theoretical construction. There is no sharp line between phenomenological and theoretical, but I suspect this distinction would help for the presentation of PACCO.

Thank you for the suggestion, we have made the distinction between phenomenological and theoretical where appropriate.

2a. Model description

- Would it be possible to have all parameters listed in one table by alphabetical order? It was at places a bit painful to find the definition and value of the different parameters while reading the text (e.g., after equation 16, or to that \dot s and k_{\dot s} are model parameters, but doesn't mention that T_ref is also listed in the parameter table. It took me a while to find T_thr which indeed is also in table 2.

We acknowledge your recommendation and we will remake all tables to include all parameters in alphabetical order in just one table in the next version of the manuscript.

- Synthetic insolation forcing: I found introducing P_e and then setting it to zero confusing. Why not simply explain that one major difference between the synthetic signal and the real one is the amplitude modulation of precession by eccentricity? (Which indeed can be relevant for resonance phenomena)

We agree and have followed your suggestion when preparing the new version of the paper. We initially included it just to show the complete approach and the possibilities of the model, but as you say, it is cleaner and more elegant to eliminate that contribution for the sake of simplicity and clarity.

- line 300 : I did not understand what is meant by the "Dynamic nature of thermodynamic hypothesis".

We intended to say that the thermodynamic hypothesis (i.e. the base becomes temperate and then basal sliding is increased) affects the ice-sheet dynamics in the sense that if you try to produce glacial cycles via this hypothesis, you need a system that reacts highly to changes in the base. If the system is not sensitive enough to these changes, then glacial cycles are not produced in a realistic manner, at least with our formulation. We will rephrase this to make it more clear.

2b. Result analysis

- line 216 : replace "higher periodicity" by longer periods.

Done.

I suspect (but this can be discussed) that the word "resonance" is used a bit abusively here. Resonance would refer to a disproportionate increase in amplitude for certain frequencies of the forcing (and then distinguishing linear from non-linear resonance, non-linear resonance occurring when the matching frequency depends on the amplitude of forcing). It is not phase locking either because the model does not have a self-sustained and oscillation. So I believe the right phrase here is just non-linear response with the output period being a multiple of that of the forcing.

Agreed.We will change that phrase following your suggestion.

- line 220: "on the contrary, even a moderate increase in insolation induces a termination too easily". (If this is indeed what you meant; though it might be argued, high sensitivity to insolation is also what you need for the MIS12-11 transition).

We agree, you need a high sensitivity to insolation to produce MIS11, however, in this particular model configuration (ISOS and RISOS), a high sensitivity is maintained all along the simulation. Thus, the energy in 20 and 40 kyr is very high and PACCO does not produce 100-kyr glacial cycles. It is only after including a more complex climate representation (after the BASE configuration) that sensitivity to insolation evolves dynamically in time so that the model reproduces MIS11.

- Line 370: the sentence is a little bit unclear. Isn't it normal to have high sensitivity to sliding?

In the sentence "one possible mechanism could be related to the rigidity of the substratum on which ice is formed" : please clarify or rephrase: rigidity is not a mechanism.

We understand that "rigidity" was not a good choice to refer to the regolith hypothesis (Clark and Pollard, 1998) and we will be more explicit in the new version of the manuscript.

- Figure 13: For clarity just specify which h_geo was used for the I,k,m,o panels.

We will specify that in the new version of the manuscript.

Sincerely,

Sergio Pérez-Montero et al.

References:

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Berger A. (2021), Milankovitch, the father of paleoclimate modeling, Climate of the Past, (17) 1727-1733 doi:10.5194/cp-17-1727-2021

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<u>References:</u>

Clark, P. U., & Pollard, D. (1998). Origin of the middle Pleistocene transition by ice sheet erosion of regolith. *Paleoceanography*, *13*(1), 1-9.

Moreno-Parada, D., Robinson, A., Montoya, M., & Alvarez-Solas, J. (2024). Analytical solutions for the advective–diffusive ice column in the presence of strain heating. *The Cryosphere*, *18*(9), 4215-4232.