Climate of the Past Discussions

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Comment on cp-2021-119

Andrey Ganopolski (Referee)

Referee comment on "Influence of the choice of insolation forcing on the results of a conceptual glacial cycle model" by Gaëlle Leloup and Didier Paillard, Clim. Past Discuss., https://doi.org/10.5194/cp-2021-119-RC2, 2021

The manuscript by Leloup and Paillard tests the ability of a simple conceptual model to simulate Quaternary glacial cycles using different metrics of "orbital forcing" which differ by relative contributions of precessional and obliquity components. The authors used only one parameter (critical ice volume) to maximize model performance in term of a novel performance metric proposed by the authors. The main results of this study can be summarized as follows:

i) model performance does not strongly depend on whether "orbital forcing" is dominated by precession or precession is essentially absent.

ii) the transition from short to long glacial cycles in all cases can be achieved by an increase of the critical ice volume by factor 2-2.5.

While the latter is not surprising since 100/40=2.5, the first result requires more serious discussion (see below).

General comments

- Since the review of Mikhail Verbitsky was already available at the time when I was requested to write my own, it is natural that I read his review before writing. And I must respectfully disagree with Mikhail in respect of the number of model parameters used in this study. Under "model parameters" (at least in our field) we understand parameters that can be used for model tuning. Gravity acceleration, Plank constant or Milankovitch' frequencies are not such parameters. This is why, formally, the Leloup

and Paillard model (which is a simplified version of Parennin and Paillard mode) has only 5 parameters. In fact, the authors did not use four of them for model tuning since they used values for these parameters from a different model (Parennin and Paillard, 2003). Thus, the only parameter which the authors used for model tuning is the critical ice volume. Whether it is good on not is another issue.

- When authors discuss the current state of our understanding of Quaternary climate variability, they are too pessimistic. The authors repeat twice (in abstract and introduction) that "the nature and physics of the [link between insolation and the glacial - interglacial cycles] remain unclear", and "the Mid Pleistocene transition ... remain mostly unexplained". Such statements would be, probably, appropriate in 1998 but not in 2021. Of course, some questions remain and, likely, will remain for some time but the major issues are already clear.
- Of course, it is up to the authors to decide which model to employ, which parameters use to tune the model and which criteria use to select the optimal parameters set. However, as the result of authors' choice, the model performance for all four "orbital forcings" for post-MPT glacial cycles are essentially the same. The authors claim that "we are able to represent the Mid Pleistocene Transition and the switch from a 41 kyr dominated record to a 100 kyr dominated record, by raising the deglaciation threshold (L. 291)." However, Fig. 4 clearly shows that this is not the case, since for three of four "orbital forcings" obliquity continues to dominate after MPT. Only for the solstice insolation, this is not the case, but then the model instead of sharp 100 kyr cyclicity simulates something which looks more like a red noise. Thus, as far as spectral properties of simulated glacial cycles are concerned, none of the model realisations is really successful. Whether this is a result of model formulation, fixing of four of five model parameters, or criteria for optimization - is not clear to me but has to be discussed in the paper.
- As I already mentioned above, if the authors are convinced that model results are equally realistic irrespectively of whether "orbital forcing" contains a strong precessional component (solstice insolation) or almost none (ISI), then they should conclude that precession plays no role in Quaternary glacial cycles and, thus, 100 kyr cyclicity has nothing to do with eccentricity. Do the authors agree with such a statement? Please comment.
- I do not understand what is shown in fig 1. Obviously, the figure heading (Normalized summer solstice insolation) is not applicable to the entire figure. More important is that the upper panel does NOT show summer solstice insolation. What it shows - I do not know.

Specific comments

L. 24. Milankovitch not just "popularized" this idea (which was not his own idea) but made it the key element of his ice age theory.

L. 26. "This also raises the question of what period should be defined as summer". It should be made clear that the question of how to define "summer insolation" is relevant only for conceptual models, like one used in this study. Climate models and ESMs compute insolation at each time step for each grid-cell and do not need such prescriptions.

L. 28. This choice leads to very different forcings.

L. 41. The authors should make it very clear that they only consider here conceptual models of glacial cycles.

L. 81, 93 and 94. I fully agree with the comments by Mikhail Verbitsky

Eq. 2. Please change V to v.

Last par, p. 7. When discussing pre-MPT model performance, it is important to realise that for this period of time, LR04 stack was tuned to obliquity. This is why it is not surprising that it contains nothing apart from obliquity

L. 231. Which “data”? What “best guess” means?

L. 300. Talento and Ganopolski is now published