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Climate of the Past
Discussions

Interactive comment on "Pre-LGM Northern Hemisphere paleo-ice sheet topography" *by* J. Kleman et al.

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The study of Kleman et al. aims to provide a new and more accurate reconstruction of the Northern Hemisphere ice sheets previous to the LGM during last glacial. It focuses on stages 5b, 4 and 2 for which some geological constrains about the position of the ice margins exist.

The goal of the paper is undeniably of interest for readers of CP, I find the approach interesting and I believe this is potentially a good contribution. It appears, however, that there is a large amount of information missing or the need for further explanation.

More specifically, I see two main aspects of the manuscript that need considerable improvement : 1. Modelling strategy and reproducibility 2. Discussion and caveats

about the limitations of the current approach

MODELLING: The description of the model and model setup (section 3) is not detailed enough to ensure the reproducibility of this study. I have nothing against the approach of a radially symmetrical temperature and mass balance fields to a given "climatic" pole, but these fields are not described at all. Could you show them ? (at least for the initial state of the simulations). If you do not consider it necessary, could you give the gradients of temperature and accumulation increase with the radius?

The same applies for the ice-sheet modelling:

Page 2566, line 21, reads "A flow enhancement [...]". Does this enhancement factor refer to deformation-based ice flow or basal sliding? Additionally, is this "tuning" done here? If yes, how?

The following sentence (lines 21-23) states "Areas of basal sliding can be specified [. . .] or internally determined [...]". What do you do here?

P. 2567. Line 2 (after describing the "free-running mode") : "[...] it can also be constrained [...]" Again, what is your choice here?

P. 2567, L. 18: "A parameter we call [...]" How did you manage the "Weertman" parameter? I can only guess that is probably set to 0 everywhere, or at least at Hudson Strait, because no ice valley can be distinguished there in figure 5 at MIS2. Is this the case? If yes, why?

Line 25 : "[...] cooling events [. . .] suspected to not reflect ..." What are they reflecting then? Please provide references.

Line 28. How was your Vostok-GRIP record spliced? Looking at figure 3a, I have the impression that deglaciation part is GRIP and the Eemian-LGM part is Vostok. Is this the case? If yes, are you aware that (concerning the stages you are focusing on in the paper) you are forcing the Northern Hemisphere ice sheets with a signal that reflects the timing of Antarctica ? Temperature changes registered in Antarctica behave in antiphase and in a more "integrative" way than those of Greenland and other proxies of the Northern Hemisphere (see Barker et al, 2007 and 2011). I understand that the reason behind this choice is related to the dating problem and to the two cooling events, but the remedy seems much worse than the illness. Even more if it appears relatively easy to fix (see the choice of Stokes et al, 2012 to this respect). Furthermore, you offer a whole (and pretty sophisticated) explanation (P. 2573, L. 7-10) for the weaker response of your ice sheets to stadial to interstadial transitions when comparing with Stokes et al, 2012 (related to calving in pro-glacial lakes), whereas you ignore the basic reason (i.e. there are not D-O events in your forcing, thus it would be quite difficult to find their signature in the ice-sheet response).

Figure 4a (the evolution of the temperature scaling factors) seems completely arbitrary. How did you end-up with that shape? It was based on the a priori knowledge that for the earlier inception you needed to further decrease the temperature to get better ice sheets? And then to increase temperature? Or, on the contrary, is it the result of some (non-described) automatic and algorithmic-based procedure to match the geological constraints. If the former is the case, the procedure seems too weak for being published.

In sections 3 and 3.1 the reader can find several times (for example P. 2568, L. 5), that a given parameter of the model was necessary to adjust to a given value in order to have a good fit with the constraints. But the authors do not give any clue of the related procedure:

Is it somehow automated? Is it based on a Monte-Carlo-type approach of different realizations with random values of the parameters and estimating the fit a posteriori? Is there any evolutive algorithm for calculating the misfits?

In other words: Could the scaling factor or/and the climatic-pole position evolve freely within your procedure?

Did they converge to the shown trajectories thanks to any procedure that minimize

errors? Or is it just determined by eye? (And therefore the parameters set to a given value just by hand).

The only related reference is given in P. 2570, L. 2-3 : "Several hundred iterations were run, before selecting the ones reported here". But :

Iterations of what? Was the "selection" process based on any objective method ?

Furthermore, the reader can not know which parameters were investigated in the procedure for arriving to the reported runs. From figures and text, the reader could deduce that the only two involved parameters are the climatic-pole position and the scaling factor (independent of whether their values were chosen by eye or by any objective procedure), but reading section 3.1 other parameters begin to appear : the "Weertman parameter", basal sliding or not (and its dependence to saturated sediments or specified by geological constraints), enhancement ice-flow factor ...

There is even an explicit reference in this sense to a "calving parameter" (P. 2570, L. 1) which was not at all mentioned before. (Besides, given the fact that the misfits with outlines occur mainly in continental areas, one could wonder how playing with the calving could help there).

As far as the method used for constraining the model results to fit the geological data is not described, the sentence "mutually supportive role" (Page 2558, lines 3-4 and page 2562, line 17) should be avoided. The reader cannot judge if both approaches support each other if it is unknown how they communicate with each other.

DISCUSSION: It is somehow curious that given the repeatedly announced purpose of this paper (provide robust ice sheet reconstructions for atmospheric modelling) no comments are made regarding the underlying atmospheric assumption employed here (i.e. perfect radial temperatures and surface mass balance patterns around a climatic pole).

The only caveats that appear in the text refer to the fact that our knowledge about the exact shape of the pre-LGM ice sheets is poor and therefore it is justified to look only to the first-order patterns. But the reader could wonder whether different physics and/or parameterizations in your ice-modelling approach produce different first-order patterns (which you have not shown).

In this sense, I suggest that the authors include some limitations of the current approach. For example, it seems impossible to me (just by looking at the outlines of MIS4 in figure 2) to find the position of a single climatic pole that (under the procedure described here) fits both the Quebec and the Kewatin sectors. (The curvature of those lines is really different and they point to different centers).

On the other hand, I would try to replace any reference to the role of internal ice dynamics for shaping the reconstructions to the discussion section and also try to be more cautious about it.

For example, in page 2562, lines 10-11, the sentence "We have not tried to capture [. . .] internal dynamics because [. . .] is not critically dependent [...]" is highly arguable. Areas of ice streaming during the build-up phase (even if less active than during deglaciation) will behave largely different than those flowing only by deformation, ultimately defining the capability of the ice sheet to further thickening or expanding more easily to a given direction.

This is potentially true even for your modelling work. If no streaming at all is allowed (again guessing because of lack of information) ice margins will expand during a cooling phase according to the radial forcing fields and to local elevation. On the other hand, accumulation is dependent on the local ice-sheet slope (Model section) . Now, if one considers a heterogeneous pattern of streaming (by considering sediments and basal properties), the surface slope will be different even for two areas of the same initial elevation and distance to the climatic pole, allowing different accumulation and potentially favoring a much less symmetrical pattern of ice expansion than that illustrated in figure 5, which would have important implications even for atmospheric flow.

The absence of any plot concerning the ice velocities or any clear reference to the adopted approach in the text obliges the reader to only speculate about.

These two main points should be explicitly addressed in a revised version of the manuscript.

MINOR COMMENTS:

1. Consider changing the title by : "Pre-LGM Northern Hemisphere ice sheet topography". (The paleo character of the study is implicit in "Pre-LGM"). Or alternatively, by: "Reconstructing Northern Hemisphere ice sheet topography prior to the last glacial maximum" (avoiding the acronym in the title)

2. Page 2559. Lines 18-22. The relevance of new reconstructions was already convincing enough, and I don't see these questions addressed back in the results or the discussion sections. Can this study help in answering them? If yes, how? If no, please suggest further research strategies for that purpose.

3. P. 2560. L. 23. "More recent ice sheet models [...]" You could add here even more recent modelling studies as Alvarez-Solas et al. 2011 and Gregoire et al. 2012

4. Page 2563. Line 28 (and several other places along the paper): MIS 5, MIS4 and MIS2 stages are referred to as stadials. I would try to avoid the use of stadials here. A given stage can potentially contain several stadials and interstadials as is clearly the case during MIS 4.

5. P. 2561. L. 3. "[...] for assessing the validity and credibility of models [...]" If by models you refer here to ice sheet models (as one could deduce from previous sentences), too much ice in Alaska does not affect their validity or credibility (rather those of the provided climate fields). I followed your logic, but please be more precise here.

6. P. 2563, L. 5-7. Consider adding here Winsborrow et al, 2004.

7. P.2574. L. 23-28. These two sentences are arguable: please see main point #2 above.

8. P.2575. L. 1-2: “The model does not well reproduce ice streams in peripheral areas [...]”. Does the model reproduce the ice streams in non-peripheral areas well?

9. Figures 3a and 4a. It is a bit frustrating that colors are not consistent between the two figures and that the scaling factor appears after the core record.

References :

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