Clim. Past Discuss., 9, C1695–C1706, 2013
www.clim-past-discuss.net/9/C1695/2013/

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

J. Kleman et al.

johan.kleman@natgeo.su.se

Received and published: 12 August 2013

Response to comments by reviewer J. J. Alvarez-Solas (The authors comments in bold)

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?

Done. I have included much more discussion of the radial climatology, which is described in detail in Fastook and Prentice 1993.

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 mention of the flow enhancement factor was part of the description of the model. Inclusion of such is common in whole ice sheet models. It was not used for tuning.

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

Again this was description of model capabilities. In this study the model determined areas of basal sliding based on the amount of water produced at the bed.

1. 2567. Line 2 (after describing the "free-running mode") : "[...] it can also be constrained [...]" Again, what is your choice here?

Again, model capability description, used only slightly in this study to exclude Rocky Mountain glaciation that occurs at low resolution when the rugged topography is not well represented. Also mountainous "micro-climate" is probably beyond the scope of our radial climatology.

1. 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?

The "Weertman" is now better described. It also in not used in this exercise except to prevent expansion of the ice sheet beyond the continental shelf break

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

Now rephrased to emphasize that the large magnitude of these events may be a regional phenomenon. The events are certainly global, but the very large magnitude of them in the Greenland records may not be representative for the entire NH. Because of the uncertainty about their nature (at least the 74-kyr event(s) is more or less synchronous with the Toba eruption, but the causal link of the climate event to the Toba eruption is questionable), we prefer the Vostok record for this time interval. Two references are now included.

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 anti-phase 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).

Justification and references now provided.

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.

The procedure is "guided" convergence to a best fit to the available ice sheet margins. The procedure is not "automatic and algorithmic-based". Any metrics that we know of require complete targets that can be numerically described. This is almost impossible when incomplete ***outlines and first-order dome patterns (which are real and useful constraining data) are what is available. Theoretically we could have used numerical techniques for e.g minimizing center of gravity offsets for individual ice sheets or cumulative ice margin offsets (and their variance). But these metric need complete outlines, for all ice masses, which we simply do not have. The only way to solve that would have been to, at the preceding stage, draw complete outlines based on shaky or nonexistent data, a procedure we regarded as unacceptable. Also, the metrics above would have to be calculated on an individual ice sheet basis, which becomes very questionable***

in situations with semi-confluent ice sheet configurations.

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).

See above, and improved and more detailed description in manuscript

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 ?

Careful “hand tuning,” guided evolution to hit the various targets. See improved and more detailed description in manuscript

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 ...

The only tuning parameters were the position of the climatic pole and a scaling factor on the amplitude of the climate proxy. No others of the many parameters available to us (and to any model) were used.

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).

Including the “Weertman,” wrongly called “calving” was a mistake and has been removed.

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.

“Mutually supportive” has been removed.

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).

A key thing is that the climatic pole is not fixed at the north pole, hence zonal asymmetry can be accounted for. There are two main ways in which an ice sheet model can be forced, either through a first-principles climatology of the kind we have used, or through applying fixed (or interpolated between) measured or modeled climate fields. As discussed in the text, we chose the first-principles approach, because it allows for tuning of space and scaling. Tuning (which is necessary to hit geological targets) of fixed climate fields appear to us as a more

dubious technique, as described in the text.

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).

We know, through previous experiments, that fully suppressed calving (which is physically unreasonable) can give different first order growth patterns because ice then freely invades marine areas. From previous experiments (Kleman et al. 2002) we also know that a different temporal climate structure can give different first-order growth patterns. The reason for the latter is that during interstadials ice is quickly cut back to terrestrial areas only. In Kleman et al (2002) a stable intermediately cold climate (unrealistic) created monodome growth, whereas a realistic climate with interstadials provided the geologically documented two-dome growth.

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).

This is wrong. Such a configuration is easy to obtain during the growth phase. Al that are needed are two highland areas approximately the same distance from the climatic pole with lowlands separating them. An ice sheet initiates on the high terrain (a vertical lapse rate of temperature insuring highland growth) and then as they grow and their surfaces at the edges become higher, the ice sheets expand (Weertman described this sort of "instability"). The two growing, roughly circular ice sheets at some point touch, with a resulting re-entrant. as this fills,

one can easily have the right angle shown in the reconstruction. What is key at that point is that the climate must warm to stop growth in that configuration, something that we did with our "guided" convergence method. Incidentally, it is unlikely that any automated, or Monte Carlo method would ever achieve such a fit. See also the above comment.

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.

All this is true. We have tried to keep it as simple as possible.

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.

A velocity plot that shows where streaming occurs is now included

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)

We agree and have changed accordingly.

1. 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.

We are providing the topography of the ice sheets to the climate modellers who presumably can attempt to answer them. The results that follow directly from our study , e.g. configurations, number of domes, migration of center of mass etc, are clearly presented in discussion and conclusions and in themselves of interest to atmospheric circulation modelers. These results, for very poorly known time intervals, provide clear incentives for atmospheric modelers to explore the atmospheric response to these changes. This latter exploration is beyond the scope of this initial study, which however provides data sets necessary for that research.

1. 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

Included, thank you.

1. 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.

Reference to "stadials" has been removed.

1. 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.

Thank you for pointing this out. It was the climate fields I was referring to. Of course the physics in the ice sheet model is tested too, by reconstructing existing (almost steady state) ice sheets, but also by constructing time-dependent events such as the retreat of the northern hemisphere ice sheets.

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

Thank you, we did.

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

We agree and have removed it.

1. 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?

We acknowledge that a model running at roughly 100 km resolution (95 to be precise) cannot well represent features that are tens of km wide and a few hundred km long (a typical "peripheral" ice stream. With this resolution, such features are smeared out, appearing wider and slower than they actually are. They do however pass the same flux. A new figure shows the velocity field with clear areas of accelerated flow where we expect them to be (the Hudson Strait, the St. Lawrence, etc.)

1. 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.

Thank you, we have have re-drafted them with a consistent color scheme.

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Interactive comment on Clim. Past Discuss., 9, 2557, 2013.