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Climate of the Past Discussions

Interactive comment on "Modelling the Early Weichselian Eurasian Ice Sheets: role of ice shelves and influence of ice-dammed lakes" *by* V. Peyaud et al.

L. Tarasoff (Referee)

lev@physics.mun.ca

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This paper examines the climate impact of pro-glacial lakes and the role of ice shelves on Early Weichselian Eurasian ice sheets using a 3D thermo-mechanically coupled ice sheet/stream/shelf model and an interpolation of GCM fields and present day climatologies.

On the ice dynamics side, I have few complaints given the current state of the field. The ice dynamics model has been well validated, though it's still subject to the current set of uncertainties we modellers face: lack of well-constrained calving model/theory, debates concerning the stress transition at the grounding line, and uncertainties with

fast flow processes especially with regards to hydrological controls. The first application of a "proper" ice-shelf model to a Northern hemispheric ice-sheet is noteworthy.

The biggest challenge for paleo ice sheet modelling is the lack of a well constrained climate chronology. In this regard, the authors' choice of climate forcing raises two issues:

1) The extent to which solar forcing dominates climate during glacial conditions especially when represented by the summer solstice value at 65N.

2) The extent to which climate sensitivity to solar forcing changes between interglacial and glacial conditions.

As detailed below, I would like to see more discussion and if possible more analysis to provide some bounds on the uncertainties arising from these assumptions. For various reasons, the ice modelling field has paid insufficient attention to constraining the uncertainties associated with assumptions employed. This needs to start changing across our field. It would also be helpful (maybe in the conclusions), to more completely summarize the main uncertainties in the results concerning both the climate forcing and current limitations in ice modelling (eg calving constraints, especially on a sub-grid scale).

With regards to the results obtained, though the general impacts obtained for ice shelves and pro-glacial lakes were not un-expected, the actual 50% maximum ice volume difference between runs with lake and no-lake climate forcing is noteworthy. Life for modellers of paleo ice-sheets keeps getting more complicated...

In overall assessment of the value of this paper, I would emphasize 3 points: 1) It addresses a time interval for the Eurasian ice sheets that has received little attention in the modelling field. 2) Given the rising concerns of potential instabilities in the West Antarctic ice sheet and associated ice shelves, the numerical investigation of the dynamics of past ice-shelves is timely and worthy of attention. 3) The climatic impact

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of pro-glacial lakes on associated ice-sheets has up to now received insufficient attention. I therefore recommend acceptance of this paper after the following points are addressed. (@= "minor/medium revisions", an in between option is needed)

**********Main issues**********

pg 224 Why is there no temperature dependence in the shelf calving model given that present day Antarctic shelf fronts are constrained by regional isotherms? What impact might this absence have? Please refer to the isotherms used in the modelling.

pg 227-228 I am uncomfortable with using a derived mid-late Holocene climate sensitivity during glacial periods (ie to reconstruct the 95 ka climate from 90 ka fields). There is no basis to assume that glacial sensitivities should be the same given the large difference in surface albedo and surface topography (and ignoring changes in large-scale ocean circulation). Some constraint needs to be provided as to the uncertainties involved with this assumption. The group has archived a number of GCM snap-shots between LGM and present and it would be worthwhile to at least provide sensitivities from those time-slices as well to provide some uncertainty bounds on the actual 90 to 95 ka climate sensitivity. If possible, two additional runs with say 1 sigma extremal bounds on the climate forcing would provide some meaningful error bounds on the results of this investigation.

pg 230-231 I would like to see a few more caveats in the discussion of calving at lacustrine margins. On the one hand, there may be a limited iceberg drainage and melt capacity in the lake. But on the other hand, though the ice in the model grid cell (O(50km)?) is too thick to float, in reality the thickness with decrease sharply as one approaches the margin. Furthermore, the lack of a well-constrained calving model, and the use of a simplified isostatic model adds further uncertainties.

On the topic of calving, uncertainties associated with marine calving should also receive more attention. Again, if at all possible, results from two additional runs with extremal ice calving parameters would provide some meaningful error bounds.

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232, discussion: Given the important points raised concerning changes in seasonal lengths, why wasn't a seasonally integrated measure of Northern summer insolation chosen as the interpolating function? It would be best, if possible, if a model run were carried out to test the impact of an integrated interpolating function.

********** Small points, typos, clarifications...:

pg 223, what resolution is the ice model run at? and line 9 change " lakes " -> " lakes' "

pg 230, ln 25 change "sensitive" to "significant"

ln 26-28. It should be make clear whether or not the pro-glacial lakes were ice-covered in the AGCM simulations.

Figure 1, No red contour visible, contrary to legend. "Thick blue" contour doesn't stand out enough, choose a colour with stronger contrast (or even black).

pg 222, ln 19-21, > At that time, a large ice sheet covered the Barents and Kara >seas from the continental-shelf edge, where it leaned against the >Arctic archipelago, to the south of the present Siberian coast.

This does not make sense (at least to someone not familiar with the term wrt the Eurasian context). The archipelago is, I would assume by definition, seaward to (and therefore north of) the continental coastline.

pg 223, ln 19: insert "lakes"

pg 224, item "v-", the description (or my comprehension) is insufficient. Please elaborate on the innovative Lagrangian calving scheme. Exactly how/when is the threshold imposed (eg is it when the sub-grid interpolated calving front will be <150m within the grid cell by the end of the current time-step?). I understand it to be accounting for sub-grid front thickness, is this correct?

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As well, is calving treated any differently for the lacustrine conditions of pro-glacial lakes? If so, how?

pg 226, ln 18, should also cite observations described in:

Marshall, S.J., M.J. Sharp, D.O. Burgess and F.S. Anslow. Near-surface temperature lapse rate variability on the Prince of Wales Icefield, Ellesmere Island, Nunavut: Implications for regional-scale temperature downscaling. International Journal of Climatology, in press

Also, why were the vertical temperature gradients not directly extracted from the AGCM fields?

ln 27 "trough" -> through. Also, how exactly is refreezing calculated? And why is a sinusoidal variation of temperature assumed when one could just use the fields from the AGCM?

pg 228, ln 25 "floatation is" -> "flotation", "When ice" -> "When an"

pg 229, ln 13 "trough" -> "through"

pg 231, ln 5-6. I can only discern a single inception site in the small figure 6a. It would be helpful to graphical enhance this to ensure all the sites are demarcated (arrows?).

Fig 2: caption (climat -> climate). What does "exp." stand for in the figures? Also, there are too many black contours. Choose a different colour (and thicker line width) for denoting the lake extent. Finally, legend colour bars for precipitation (Top left and right) are missing (given the m/a units and plotted colours, the colour bars shown can only apply to Temperature).

Interactive comment on Clim. Past Discuss., 3, 221, 2007.