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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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Response to comments by reviewer C.R Stokes (The authors comments in bold)

This paper reconstructs the pre-LGM topography of the Northern Hemisphere ice sheets with a particular emphasis on marine isotope stages 5b and 4, and comparison to stage 2. The strength of the manuscript lies in its approach, which uses glacial geological evidence of putative past ice sheet extents to tune a numerical ice sheet model, which then provides reconstructions of the 3-D geometry of the ice sheets. These reconstructions (e.g. of ice sheet thickness) represent important boundary conditions

for atmospheric modelling prior to the Last Glacial Maximum (LGM), which partly motivates this study. In contrast, previous work has tended to only present and discuss geological evidence, or only use numerical modelling that is poorly constrained by geological evidence. A further strength lies in the scope of the manuscript in that it deals with both the North American and Eurasian Ice Sheets, when most other studies are restricted to either North America or Eurasia and sometimes just one ice sheet. The manuscript is also very well written, well-organised, and uses Figures appropriately. Studies of pre-LGM ice sheets are often necessarily speculative and often rather long treatises, and so I expect this concise and accessible manuscript to be a useful and valuable contribution to the relatively small number of studies on pre-LGM ice sheets. That said, there are a small number of relatively minor points that I would recommend are addressed/clarified in a revised manuscript.

1. The Introduction is excellent and provides a very useful context, but it is rather long. I wonder if the authors might consider splitting it into a brief Introduction, followed by a 'Previous Work' section? In relation to this, I think it would be helpful to warn the reader that the current Section 2 is actually moving on to the Methods for reconstructing the spatial extent of the ice sheets and thus might be better entitled "2. Methods", followed by "2.1. Spatial extent of ice sheets based on geological evidence", since it should also be made clear that these 2D extents are all based on glacial geology.

Now restructured according to comment

2. Page 2561, line 12: It's perhaps a nuanced point, but the Stokes et al modelling was constrained by geological data for the final deglaciation, i.e. the model was run from the last interglaciation (122 ka) and those model runs that failed to fit the final deglacial record (e.g. dated ice margin chronology) were eliminated in the ensemble analysis. This should help increase the plausibility of the pre-LGM model runs, but perhaps not to the same degree as the analysis in the present paper.

Rewritten so that geological tuning in Stokes et al. is acknowledged

3. Page 2563, lines 11-14: this is one of the few places where I think the reader may benefit from some additional information/clarification. The authors point out that they "weighed the evidence and decided on what we consider the most likely alternative...". I acknowledge that there is large uncertainty, but can the authors reassure the reader that these decisions were taken as objectively as possible. Were any criteria employed and/or what type of evidence was weighed and judged? Maybe even a short example might help of how disparate sources of evidence were reconciled or ignored. I wouldn't want to see an important study accused of being too speculative/subjective.

A minor rewrite is made. Specific examples are given under 3.1 and 3.2.

4. Section 2.1 "North America". In this review, I was a little surprised to see some important studies not cited. I realise that the Introduction is clear in stating that this section is based primarily on the recent review by Kleman et al. (2010) "and references therein", but I think a little bit of discussion (and at the least a citation) is required for

some of the key studies, e.g. Clark et al. (1993), Boulton and Clark (1990), the clutch of papers in GPQ vol. 41 (2) in 1987, and Clark and Lea (1992) Special Paper.

We acknowledge bad organization of the text. Some of the wanted references are cited earlier, now also in this place.

5. Page 2574, lines 16-18: I was intrigued by this sentence and citation, which would appear to be quite relevant to the Discussion in this paper. I wonder whether the authors might consider adding a few more sentences of explanation, e.g. quite how radically different are these ice sheet configurations; what were they based on; and what might have caused them?

Text now expanded to give key charateristics of these configurations.

6. Page 2575, lines 9-11: I can certainly appreciate the authors' reluctance not to reiterate the long-standing debate over mono-versus –multi-domed ice sheets, but it is important to perhaps briefly inform the uninitiated of which configuration current finds most favour and why.

Text now expanded.

7. Page 2575, lines 17-21: Compared to the rest of the paper, this paragraph is poorly written (with several typos) and seems to be too brief (almost as an afterthought?). However, I think it is making a really important point, given that LGM portioning of global ice volumes is very poorly understood. Thus, I would strongly encourage the authors to expand this paragraph and perhaps incorporate some literature that also estimates Southern Hemisphere contributions, perhaps based on evidence from the

southern-hemisphere and/or numerical modelling.

We appreciate the comment, but find it quite hard to find any reference with well-constrained southern hemisphere contributions.

8. Figure 6: I can see the advantage of placing each panel in chronological order from top to bottom, but this Figure is quite small on a printed page. Of course, most people will read the paper online and zoom in, but I'd encourage the authors to consider enlarging these important panels.

We regard it as important that the reader has access to all panels in one glance for comparisons.

Minor technical points:

2558, lines 1 and 23: delete "here" (it seems unnecessary)

Done

2558, lines 15-19: this sentence is awkward. Suggest re-write.

Sentence rewritten

2559, line 29 and on to following page: this sentence is awkward. Suggest re-write.

Sentence rewritten

2560, line 9: "ice sheet"

Done

2560, line 11: this sentence could do with some supporting citations (e.g. Boulton and Clark, 1990; Hubbard et al., etc?)

Boulton & Clark 1990 now referenced

2560, line 12: I think I know what you mean, but it might be useful to state what you mean by "core areas"

We think core areas are a well established term that requires no further explanation

2560, line 15: delete "available"

Deleted

2560, line 16: delete the first "the"

Deleted

2560, line 26: this sentence needs re-writing.

Now rewritten

2561, lines 7-10: "are also unable to generate a southward extent of the Quebec dome, during ice sheet build-up, that is sufficiently..."

Done

2562, line 23: "...location is important."

Done

2563, lines 4-7: do the authors need to make it clear that these "spatial reconstructions" are all based on glacial geological evidence? Are they?

All except Lambecket et al. are based on geological evidence. Lambeck et al is geologically guided modeling.

2566, lines 1-2: "Eurasian ice sheet, that a British Ice Sheet may have provided, was potentially..."

Done

2569, line 6: delete “here”

Done

2569, lines 21-22: “...was that they achieved a good fit, in ice sheet outline and location, to the available...”

Done

2574, line 24: “were largely...”

Changed

2575, line 7: delete “in the model”

Deleted

2575, line 11: “underestimates”

Corrected

2575, line 24: is “additional” required?

Taken out

2576, lines 4-7: I think it is important to point out here that the MIS 5d ice sheet was actually the first to emerge. Otherwise, it reads as if 5b was the first period of ice sheet growth.

Now pointed out

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