

Interactive comment on “Coupled Northern Hemisphere permafrost-ice sheet evolution over the last glacial cycle” by M. Willeit and A. Ganopolski

Anonymous Referee #2

Received and published: 5 June 2015

Overall I find the study well designed, interesting and suited for publication in Climate of the Past. I find the implications on the simulated ice-sheet extent particularly interesting. I would therefore recommend it for publication provided that the comments below are addressed.

Comments below are stated in order of reading. Major comments are highlighted with a (***) , others are minor comments.

p. 558, line 4-16 (***) : this paragraph is intended as an outlook of the limitation of the previous works. I am not convinced that the major limitation of previous modelling studies for long-term permafrost evolution is due essentially to the climate forcing. My

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impression is more that the main limitations for long term permafrost evolution evaluation is 1) the lack of climate modelling on this timescale, apart from the work of the group of the authors on the last four glacial cycles 2) the lack of coupled models including a permafrost component. The "limitation of previous modelling ... has been the climate forcing" is therefore very much incomplete.

p. 558, line 7 : "A step forward with ..." => "A step forward in ..."

p. 558, line 7-10 (***) : first here but also in many other occurrences below, there is a need to include a discussion of the published paper of Kitover et al. (2015). The drawbacks mentioned by the authors in using MAGST versus MASAT which is of concern for the present paragraph and correctly noted for the cited work of Kitover et al. (2013) has been addressed fully in Kitover et al. (2015).

p. 560 (***) : given the importance of MAGST versus air temperature, I find the split of equations between the main text and the appendix to be not optimal. I would rather have all the descriptions that are likely to be crucial for the permafrost extent and its effect on climate described in the main text. That should include the snow representation. If including all the equations would seem frightening to some reader, I recommend to at least include in the "Model Description" a recap of the main choices and their implications.

p. 561, line 24: Can pore water "feel" ? replace with "is affected by"

p. 563, line 23-25 (***) : if I understand correctly, the solving of the temperature profile from the top of the ice-sheet to the bottom of the ice-sheet is done as a single layer on top of the permafrost / soil layer? Is that correct? In that case it should be stated explicitly. Also, it states the question of the coherence of the temperature profile that is computed in the ice-sheet model. Since the manuscript claims a coupling between permafrost and ice-sheet, it is crucial to detail the level of coupling. As it is done, it gives the impression of a "one-way coupling", that is the permafrost affects the ice-sheet through the heat transfer, but nothing is given to the permafrost module apart

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from the ice-sheet height. You might expect the liquid water content, the temperature at ice base etc. to be part of the two-way coupling.

appendix A-B-C (**): given the high similarity of the approach taken to that of Kitover et al. (2015) a discussion of the coherence and the differences is necessary

p. 566, line 13, should be “observations and model data” or “observation data and model data”

p. 570, line 4: "kilometeres" => "kilometers"

p. 570, line 4 and line 23: "the mode of". I do not understand this expression. Do you mean "mean of"?

p. 570 (**): When the modern-day or LGM modeled extents are compared to either observed or previous studies, they should include some explanation of the discrepancy as due to how the permafrost is defined (i.e. continuous, etc.). They say their modeled extent is different from Vandenberghe et al. (2012) but that is because they also estimated discontinuous extent.

p. 571, line 13, “radically differently” sounds strange to me.

p. 572, Section 3.4. This is a relevant point observed from the authors’ series of experiments but the paper from Osterkamp and Gosink (1991) should be included in the discussion. They discuss convergence time and initialization as well. Important to include since long-term permafrost evolution modeling does not have a lot of literature behind it.

p. 572, line 11: What is figure 15a ? Labels a and b should be explicitly added to the figure.

Overall: I generally do not like to use the term significant unless there is some statistics involved. It is a very subjective term. The authors tend to use this term a lot throughout the manuscript.

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Figure 4 (***) : There is a very obvious bias for Northern Canada that needs to be explained and discussed further. The bias for southern Siberia is mentioned by the authors and discussed, why not the one for Northern Canada?

Overall (***) : the validity of the conclusion presented in the manuscript (on the ice-sheet volume at the LGM) is very dependent on the amount of warm-based ice-sheet simulated during the glacial cycle. Though this aspect is acknowledged in the text, it should be made very clear in the conclusions and in the abstract that this is only the result of ONE particular model. The numbers of other models presented in the introduction indicates a very large range of uncertainty and this range should be reflected in the sea-level estimation that is provided with the manuscript.

References cited: D. C. Kitover, R. van Balen, D. M. Roche, J. Vandenberghe, and H. Renssen *Geosci. Model Dev.*, 8, 1445-1460, 2015, <http://www.geosci-model-dev.net/8/1445/2015/gmd-8-1445-2015.pdf> Osterkamp, T.E. and Gosink, J.P. (1991). Variations in permafrost thickness in response to changes in paleoclimate. *Journal of Geophysical Research* 96: doi: 10.1029/90JB02492. issn: 0148-0227.

Interactive comment on *Clim. Past Discuss.*, 11, 555, 2015.

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