Response to reviewer #2

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 modeling studies for long-term permafrost evolution is due essentially to the climate forcing. My 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.

The section has been rewritten taking into account the reviewers suggestions:
“The main limitation for long term permafrost evolution evaluation is the lack of coupled models including a permafrost component that are capable of performing multi-millennial transient simulations. All modelling studies on the long-term evolution of permafrost have been performed by somehow prescribing surface temperature changes as boundary condition and ignoring the various permafrost feedbacks on climate. Even this simplified offline modelling approach is problematic because of the limited climate modelling available on the glacial cycles time scale. As a workaround, Tarasov and Peltier (2007) used temperature forcing inferred from interpolated LGM and preindustrial climate model simulations to explore the permafrost evolution over the last glacial cycle. A step forward in this respect was done by Kitover et al. (2013), who used surface air temperature from transient simulations with an Earth system model of intermediate complexity (EMIC) to estimate the permafrost thickness evolution during the last 21 ky at selected locations in Eurasia.”

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

Done.

p. 558, line 7-10 (***):
first here but also in many other occurences 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 concerned 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).

The improvements described in the paper of Kitover et al. (2015) have been included in the discussion.

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.

The description of the computation of the MAGST has been moved to the model description section in the main text as suggested by the reviewer.

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

Done.

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 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.

Ice sheet and permafrost module are thermally fully two-way coupled. The ice sheet model is discretized in 20 vertical levels and the ground in 30 vertical levels and the temperature profile at this 20+30 levels in ice sheet and ground below the ice sheet is solved in one step using a fully implicit scheme which can be solved with a tridiagonal matrix algorithm. The boundary conditions are given by the geothermal heat flux at 5 km depth and the surface temperature at the top of the ice sheet.
To be clearer, the sentence has been reformulated as:
“When the surface is overlaid by an ice sheet, the temperature profile in the ice sheet and the ground is solved simultaneously using a tridiagonal matrix algorithm, with the ice sheet surface temperature prescribed as top boundary condition. Ice sheet and ground are therefore fully two-way thermally coupled and the temperature at the ice sheet base is free to evolve in response to changes in ice surface temperature, internal ice sheet dynamics and ground heat flux.”

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

Where appropriate the relation to the approach of Kitover et al. (2013, 2015) has been mentioned in the text.

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

Changed.

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

Corrected.

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

The mode is the value that appears most often in a set of data.

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.

In the revised version of the manuscript we state explicitly that the modelled permafrost extent should be compared to the continuous permafrost area. We also included a citation to the LGM permafrost map of Vandenberghe et al. (2014) where the continuous and discontinuous permafrost extents are explicitly separated.

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

Changed to *'radically different'.*

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.

A discussion of the Osterkamp and Gosink (1991) results has been included as suggested.

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

Done.

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.

The term significant has been substituted by appropriate words where required.

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?

A discussion of the bias over Northern Canada has been added:
"Permafrost thickness is also overestimated in the north of the Canadian Arctic Archipelago (Fig.4).This is mainly a result of the low geothermal heat flux in the Pollack (1993) dataset in this region. Using the Davies (2013) geothermal heat flux, which has higher values over most of Canada (Fig.2), remarkably reduces the tendency of the model to overestimate permafrost thickness over northern Canada (Fig.5)."
Figure 5 has been modified to show the permafrost thickness for the simulations with both geothermal heat flux datasets.
Furthermore, a figure showing the scatter of modeled versus observed MAGT has been added to the paper as new Figure 4 to complement Figure 3 in showing the performance of the model at simulating the MAGT.

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.

In the abstract we know explicitly state that this result is valid for our model only. In the conclusions we added the following sentence to stress the point made by the reviewer: *“However, the effect of permafrost on ice sheet volume is expected to depend on the amount of warm-based ice-sheet simulated during the glacial cycle, which is known to be model dependent. Independent model simulations are therefore required to confirm the robustness of this result.”*

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.