We would like to thank the referee for the valuable comments. Please find below our point-by-point replies to each of the comments.

Reply to Referee #1

This paper describes some interesting model developments, and investigates the important physical processes in permafrost modelling. There are two main points to the paper: 1. The inclusion of new processes in, and verification of, the model. 2. The assessment of model sensitivity, which emphasises the importance of certain processes in influencing the soil thermal dynamics. The main conclusion of the second point is that it is important to include water bodies in models, which is often not done currently. While this point is not necessarily new it adds strength to the argument for their inclusion, which is clearly valuable. Thus the combination of points 1 and 2 merits this research worthy of publication. However, the paper would benefit from substantial revisions.

== Scientific Comments ==

Section 2 - Methods (and Appendix A)

Firstly, regarding the calculation of volumetric water content for each layer of a water body: Since the volumetric water content is reduced by at most 6% by the bottom of the water body, is this calculation worth doing? Is the difference made by including this small volume of soil larger than the uncertainty in the physical parametrizations of water and soil? If not, it may not be worth doing. It would be valuable to give some justification for including this calculation in the model - the most obvious way would be to run the simulation with VWC = 1 for all water layers to show whether it makes a significant difference.

Reply:

Thank you for your suggestion. We have made the comparison that you suggested and present the results in subsection 3.2.3, followed by relevant discussion in subsection 4.3.

Secondly, a comment regarding the setting of a maximum snow depth to represent snowdrift processes: This is a simplistic method that requires further justification to convince me, as a reader, that it's reasonable. Can you find some more observed time-series of snow depth with which to compare your model? Can you explain why you chose to do this instead of scaling the snow 'input' until the modelled depth matched the observations?

Reply:

Unfortunately, continuous snow thickness measurements are very scarse for this remote field site, where snow depth sensors are operated unattended during the harsh winter period. We tried the "scale method" following Zhang et al. (2012) but the simulated snow thicknesses were still too great, so that a site-specific parameterization based on the available measurements became a matter of necessity. Even though this parameterization is very simplistic, it is representative for the basic features of snow accumulation processes at the study site, which are strongly affected by wind drift and micro-relief. Please see the description in subsection 2.4.2

Regarding the sensitivity analysis with changing water depth: In order to include this sensitivity analysis in the paper, more work needs to be done. The results are interesting but would be much more interesting if backed up with further simulations. I suggest that you run with a greater number of different water depths in order to plot curves of the unfrozen soil thickness as a function of water depth, rather than just bar charts.

Reply:

Thank you for your suggestion. We have run further simulations, as suggested, and found some interesting phenomena concerning the response of multi-year mean simulated unfrozen soil thickness to water depth changes within polygon center. We now modified subsection 2.4.3 (of the Methods section), subsection 3.3 (of the Results section), and subsection 4.2 (of the Discussion section) accordingly.

Section 3 - Results

3.2.4 compares modelled temperatures with borehole measurements. Where is the borehole? You found the temperatures at the centre site closest to those of the borehole. If the borehole is at a polygon centre that is a good result! If not, it's not so good. Please include this information.

Reply:

We have added information on the borehole location in subsection 2.1 (Site Description) and updated subsection 3.2.4 (of the Results section)

In subsection 2.1:

A 26.75 m borehole was drilled in 2006, in an area that consists of about 60% polygon centers and about 40% polygon rims, with a negligible areal proportion of ponds (Figure 1).

In subsection 3.2.4:

The centers and rims are typically about 10 m across and horizontal temperature differences due to surface heterogeneities can be assumed to be largely averaged out at depths greater than 10m. The borehole temperatures at depths greater than 10 m therefore represent an average temperature beneath both polygon centers and polygon rims. If the simulated temperatures from the rim and center sites were averaged by 40% and 60%, respectively, then the overall mean simulated temperature at 26.75 m depth would be -9.55°C, which is about 0.75 °C colder than the temperature recorded in the borehole at the same depth and over the same period of time

(Section 4 - Discussion : A deeper scientific analysis would be very helpful, see comment in 'technical' section, below)

Section 5 - Conclusion

I feel the last sentence hasn't been fully justified. Looking at the rim and centre plots, these seem to have similar thermal regimes, especially within the uncertainty of the forcing data (e.g. snowfall!). Why should I distinguish between them in my model? Perhaps it would be good to emphasise which land surface types are the MOST important to include. This makes your study

more valuable - after all, you started with the premise that these are important land surface types, because those are the ones you chose to model, but you haven't really analysed your model results to decide whether you were right. There is more scientific understanding to be gained in this paper.

Reply:

It is true that the simulated thermal regimes of the rims and centers without water levels below the ground surface are quite similar. However, our study shows clear differences in the soil thermal regimes when the polygon centers are water-filled. Nevertheless, the main objective of this study was to validate the new polygonal tundra scheme of the DOS-TEM model, and to test its ability to simulate micro-scale land surface variations.

== Technical Comments ==

The main thrust of the paper is a little unclear to the reader at the beginning. In the conclusion it is presented in the two parts that I have described above (as two paragraphs in the conclusion), and this appears to be a better presentation.

Abstract - It would be beneficial to re-write most of the abstract (everything after the first 2 sentences) to make the aims and scope of the paper clearer. Perhaps consider the work as a two-part piece (as the two paragraphs in the conclusion would suggest, and as I have described above). A list containing 4 different conclusions is confusing in an abstract. It could be made clearer even just by removing the 4th point from the list and adding a couple of words to the following sentence.

Reply:

Thank you for your suggestion. We have modified the abstract, as suggested.

Section 1 - First, a small comment: The flow of logic would work better if the sentence beginning on line 14 ("However, few of the：：：") was moved up to begin instead on line 6.

Reply:

Thank you for your suggestion. We have moved the sentence, as suggested.

Secondly, a larger comment - The final paragraph of Section 1, which summarises the paper, is too short and not clear enough. For example, when "our dynamic organic soil version of the TEM" is mentioned it is not clear whether that includes any of the model developments that are described in the paper or whether this is an existing model. (I believe the latter is the case?). Please expand this paragraph and make it clearer.

Reply:

Thank you for your suggestion. We have modified the introduction, as suggested.

Section 2 - Methods

2.3 - Model developments (- Plus appendix A). Part of this needs to be explained more clearly. Specifically point (1) beginning on line 25. For someone who does not already know

what you have done this sentence doesn't make sense. I was only able to understand after looking at the appendix.

In the appendix itself, the diagram showing the mathematical structure of the model and its layers is not clear enough for the reader to understand. It is not apparent what the red and blue lines are, or the dotted lines, or how "m" fits into the numbering since it appears alone next to n+1.

Reply:

Thank you for your suggestion. We have elaborated subsection 2.3 and improved Figure A1 in the revised manuscript.

Section 3 - Results

3.2.2 The paragraph beginning "For the point site, the model underestimated：：：", this result may be clearer if you mention that "the amplitude of the seasonal cycle is reduced". Also the sentence "Setting a maximum snow thickness thus reduced：：：" simply repeats the information of the previous sentence and may be deleted.

Reply:

Thank you for your suggestion. We have modified the text, as suggested.

3.2.3 - The very last word on page 4896, "However", is confusing as the statement that follows seems to be perfectly in accordance with the previous one.

Reply:

Thank you for your suggestion. We have deleted the word "However" .

Section 4 - Discussion

4.2/3 : Can you make 4.2 and 4.3 into a single, more coherent, discussion section, not only of the effects of water and snow on the soil temperatures, but of the differences *between* the different land surface types (ostensibly the main thrust of the paper) particularly which ones show the largest differences in thermal regime relative to the others, and what the main differences are. Also emphasise what physical differences between the land surface types are actually included in your model. Perhaps it is just the snow and water? In which case this would explain why you have examined the snow and water. But this is not clear!

Reply:

Thank you for your suggestion. We have combined these two subsections and discuss the differences in snow and water cover on different land surface types in subsection 4.2.

4.4/5 : It would probably be better to put the uncertainly analysis before the outlook, as readers will want to know about the level of uncertainty in your studies before they can accept your recommendations for land surface models.

Reply:

Thank you for your suggestion. We have moved the uncertainty analysis to before the Outlook section.

In 4.4, where you say "the heterogeneity of the Arctic polygonal tundra results in marked differences in soil thermal dynamics", it would be a good idea to refer to figures 10 and 11,

which demonstrate this.

Reply:

Thank you for your suggestion. We have modified the text, as suggested.

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Hope this input is helpful. Looking forward to seeing a revised version.