Interactive comment on “On the Configuration and Initialization of a Large Scale Hydrological Land Surface Model to Represent Permafrost” by M. E. Elshamy et al.

Anonymous Referee #2

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This study aims to derive a robust, yet computationally efficient initialization parameterization approach that can be applied to regions where data are scarce and simulations typically require large computational resources. An upscaling approach to inform large-scale ESM simulations based on the insights gained by modelling at small scales was performed. The results show that the model has good performance in reproducing present-climate permafrost properties at the three sites at the Mackenzie River Valley. The results also demonstrate that the simulations are sensitive to the soil layering scheme, the depth to bedrock, and the organic soil properties.

It is really important to investigate the performances of hydrological and land surface models in permafrost regions under climate change. However, there are some shortcomings that might affect the contribution of this study. My main concern and comments are listed as follow.

We would like to thank the reviewer for the time spent to carefully review our manuscript. We greatly appreciate the important points raised. We present our response to reviewer’s comments below. The reviewer comments are listed below in regular black text, and our response in regular blue text. Some of the reviewer’s suggestions have been addressed in the revised manuscript under preparation while other responses point towards what we intend to do.

General comments

1. Lin 34:...however, are not so clear…You should give citations.

The statement is followed by a couple of sentences that provide further explanation and citations and was further strengthened based on comments from Reviewer 1 to read:
“Subsequent impacts on water resources in the region, however, are not well-understood and can be different in different parts. For example, a recent analysis of trends in Arctic freshwater inputs (Durocher et al., 2019) highlights that Eurasian rivers show a significant annual discharge increase during the 1975-2015 period, while in North America, only rivers flowing into the Hudson Bay region in Canada show a significant annual discharge increase during that same period. Canadian rivers flowing directly into the Arctic, of which the Mackenzie River provides the majority of flow, show little change at the annual scale. These analyses at annual scale, however, can mask larger changes at the seasonal scale. For example, Bonsal et al. (2019) report higher winter flows, earlier spring flows, and lower summer flows for some rivers in Canada. Further, they also state that “It is uncertain how projected higher temperatures and reductions in snow cover will combine to affect the frequency and magnitude of future snowmelt-related flooding”.

2. Line 39: What do you mean “uncertainty”?

With the modifications to the first paragraph given above, we rephrased our statement to read:

“The hydrological response of cold regions to climate change is highly uncertain with the current state of knowledge, because, to a large extent, of our limited understanding and representation of how the different hydrologic processes will interact under new climate conditions”.

The introduction will be refocused further in the revised manuscript.

3. Line 46-50: You give importance of permafrost here, which may be not suite for this paragraph. I suggest that you provide separate paragraph to show the importance of permafrost and the progress in interaction between permafrost and hydrological at the beginning of the introduction.

As we intend to refocus the introduction, this will be addressed in the revised manuscript.

4. Line 51 and 91: Here the authors give the modeling work in hydrological processes in permafrost regions, I noticed that the models were all land surface models. As I know, there were many modeling work that has been done by hydrological models in cold regions, such as VIC, GBHM. I would suggest that the authors to provide the different with hydrological models and land surface models on the previous modeling work in hydrological processes in cold regions, then clearly state why you choose the land surface model for this study.

While the contributions of the mentioned studies are significant, the emphasis herein was to consider those models that include robust representation of the energy balance and are able to produce detailed temperature profiles in multi-layer deep soil columns. Generally, hydrological models do not include the full energy balance and therefore they do not have a handle on permafrost unless they are coupled with other energy balance models, as Zhang et al. (2012) did with GBHM. VIC (Liang et al., 1994) is a special case of hydrological models and is often described as a land surface hydrological model which makes it similar to MESH in this regard. The modelling efforts also include thermal modelling (e.g. Wright et al., 2003). We intend to revise the introduction to make it more focused and to add some references to reviews of permafrost modelling such as Riseborough et al. (2008) and Walvoord and Kurylyk (2016) to guide the interested reader.

5. Section 3.2 Study Sites and Data: This section is too long. Please make it concise using figures and tables. In addition, you may combine Section 3.5 (Climate Forcing) with this Section. They are all data introduction.

We agree with the Reviewer that this section is too long and we intend to shorten it and move most of the details to a supplement. This is also suggested by Reviewer 1.

6. Line 170-171, Permafrost, which is defined as ground in which temperatures have remained at or below 0°C for at least two consecutive years. There is variation in temperature between different years, the bottom of the active layer is not necessarily connected to permafrost table, and a melting sandwich may occur. The author judges the active layer thickness by the change of soil temperature one year. This should be distinguished from the permafrost table.

Thanks for pointing up this discussion. We fully agree with the reviewer and that is the reason we use a “thaw rather than freeze criterion” in the definition of the ALT (lines 158-162) and explicitly mention that it has to be connected to the surface. We will be revising the text to emphasize this difference in the revised manuscript.

7. Line 190-193: As I know, there are two alternative schemes for soil organic layer in land surface models, one is assuming one or more organic matter layers cover the mineral layer at a vertical depth, the other is the weighted combination approach, such as in CLM. I suggest that you should compare the two schemes and give their different.

CLASS can either use a percentage of organic matter within a mineral soil layer or use fully organic layers. In the first case, the organic content is used to modify soil hydraulic and thermal properties - similar to CLM (Oleson et al., 2013). In the latter, CLASS has special values for those properties depending on the type of organic soil selected (Fibric, Hemic or Sapric) based on the work of Letts et al. (2000) for peat soils. This is described in the manuscript in L184-193. We adopted a 30% threshold to differentiate fully organic soils from mineral soils with organic matter based on Soil Classification Working Group recommendations.

L367-378: For the HPC site, we tested both approaches as the organic matter was only 18% (below the 30% threshold). We selected to use fully organic soils for BWC and JMR sites because of the high percentage of organic matter found from the soil dataset we used (above the 30% threshold). We thought a mineral soil would not be suitable for those sites. However, we have also conducted simulations at all sites using the mineral soil formulation with high organic percentage for the BWC and JMR sites and intend to discuss the differences in soil properties and their impact on the simulations in the revised manuscript.

8. Line 343-344, 557-560: I am confused by the description of the lower boundary conditions of the model. The author should clearly state which boundary conditions are used in the model, the Dirichlet condition (fixed temperature in boundary), Neumann conditions (fixed geothermal flow in boundary) or Robin conditions (fixed temperature and geothermal flow in boundary). In addation, the upper boundary conditions should also be properly explained.

CLASS uses a constant geothermal flux at the bottom boundary (i.e. Neumann type condition – constant derivative). We used the default value for this flux (zero) and thus used the term no-flux boundary as mentioned in L343-344 and on L559. We will revise the manuscript to further clarify that. We noticed in simulations with shallower soil column depths that the temperature at the bottom boundary changes over time as mentioned in L461-463, which confirms that the boundary condition is not type 1 or 3 (Dirichlet or Robin). The Upper boundary condition depends on the meteorological forcing and how it is modified by the canopy and snow cover to determine the heat flux at the soil surface. Following the recommendations of Reviewer 1, we intend to add a section on the mathematical formulation in the revised manuscript that should clarify the matter.

9. Line 436-438， 455 :You also should give the soil moisture figure using different number of cycles, and when it stabilizes. Your title is "…a Large Scale Hydrological…", and your results were only soil temperature, how about the soil moisture?

We agree with the reviewer and we intend to add figures of soil moisture profiles and convergence for a few cases to illustrate the point.

10. Line 466-467: Please check this sentence, the temperature difference reached 1.0 k between 100 times and 2000 times cycles. It revealed that 100 times cycle was not stable, but you said that "there is no significant change after 100 cycles and sometimes less."(In Line 453-454), Why?

Thanks for pointing out this potential contradiction. We think that a temperature change of 1K over a period of 1900 years (cycles) is negligible. That's about 0.0005 K/year (cycle). This is not visible in Figure 7 where we plot the temperature profiles but is more visible on Figure 8 where the temperature sequence is plotted. We amended the statement on L466-467 to include the rate to emphasize that in the revised manuscript under preparation. Additionally, the impact of the number of spinning cycles on the simulation of ALT and temperature envelopes is shown to be minimal in section 4.2.

11. Line 481-482: The simulations have very longer time period (1979-2016), and the deep soil temperature change was evaluated. As you know, the geothermal flow will have a great influence on the deep ground temperature at a long-time scale, which may be more than the impact of climate change. Strongly recommend that you should use the geothermal flow for the lower boundary by observed data from drilling or the relevant data from references.

We have done additional simulations using geothermal heat flux and will be reporting on that in the revised manuscript. They basically emphasize the previous findings of Sapriza-Azuri et al. (2018) for Norman Wells using the same land surface model we used (CLASS) that the geothermal flux has negligible impact on the results. In this paper, the authors compared two scenarios: 1) no heat flow at the bottom of the lowest soil layer, 2) a constant geothermal flow of 0.083 Wm-2 based on local measurement in Normal Wells. The scenarios were applied for a climate average year spin-up by 2000 cycles to several soil depth configurations and parameter values. Results reported by authors showed, as stated in the manuscript L342-347, that the impact of geothermal flux was minimal and the temperature difference between the two scenarios was small in most simulations and is within ±0.15°C in approximately 60 % of simulations. In fact, 1979-2016 is quite a short period specially to catch big difference for the deep soil temperature. In that sense Sapriza-Azuri et al. (2018) used a 2000-year simulation without getting too much difference.

12. Line 492-494: It is very confusing here. Active layer thickness is only 3m at JMR. The soil temperature and moisture should be stable values, which are the initial conditions for the next step simulation after 100 cycles (100 years) in theory. However, there were larger differences from simulation results given by Figure 9 because of the initial values of different cycles (50-2000 times). This is very abnormal. You should check the simulation results again, whether the cycle is not enough, or other reasons that make the initial value do not converge. Please give a detailed explanation.

The less stable conditions at JMR are possibly related to the small thickness of permafrost and the thick organic layers. These may have caused the drifting in temperature shown in Figure 7 for some layers under the slightly warmer conditions compared to HPC and BWC. We intend to check the results again to find a better explanation for the phenomenon and clarify that in the revised manuscript.

13. Line 527-528: Simulation results of temperature envelopes were lower than observed values, which may be caused by neglecting geothermal flow.

As mentioned in our response to point #11 above, we conducted simulations for both JMR and BWC using the geothermal flux and it had minor effect as we will be reporting. We are investigating the reasons, which might be related to the quality of snow simulations as well as the configuration of organic soils and the parameter values of the soil (drainage and thermal) set for such places.

14. Line 554-555: The explanation for the cooling effect of the model increased the depth of SDEP is unreasonable. From Figure 14, it can be seen that the location of SDEP after increasing is located in permafrost, and soil water content in this layer should be frozen throughout the year. I am not sure that the model could take into account the difference in thermal properties between permafrost including ice and ice-free bedrock, and the thermal convection generated by little unfrozen water in the frozen soil. These could explain the cooling effect. If so, further explanations should be provided.

We agree with the reviewer. SDEP remains below the active layer and therefore any moisture will be frozen. CLASS differentiates between ice-free bedrock (below SDEP) and permafrost that contains ice. However, we intend to further investigate the soil moisture content and to compare the thermal properties of the soil above and below SDEP to see if the differences can explain the cooling. Thanks for the ideas to close this loose end.

15. Line 575: I suggest that you should check variation in the upper boundary drive (climate) during the simulation time. This may be the reason why the temperature envelope tends to be at a given temperature at lower boundary.

The upper boundary condition (climate) is transient for the 1979-2016 simulation, yet the temperature of the lowest layer barely changes over that period. We tested with shallower soil profiles and found it more responsive to changes. We think that the thermal properties and deep profile are the reasons for having such response at the lower boundary. We intend to analyze the forcing climate as well as the thermal soil properties to further address this concern and revise the manuscript accordingly.

16. The discussion needs be strengthened. You should compare your results with others, then conclude what your new fingdings and contribution.

Thanks for pointing this out. We intend to strengthen the discussions in the revised manuscript by framing it around previous work to better show the contribution. This is also suggested by Reviewer 1.

Specific comments

1. Line 101: What is ALD? When you give an abbreviation for the first time, you should give the explain. I found the explain in Line 158, but this is the first time here. In addition, active layer thickness is more commonly used, I suggest use ALT instead of ALD.

Thanks for noting this. ALD and ALT are equivalent because our model does not include land settlement and therefore the fixed reference level used to measure ALD is the ground surface - definition is given in Geological Survey Canada reports (e.g. Smith et al., 2004). However, we changed ALD to ALT in the whole document (inducing figures) to use the more standard terminology. We made sure all terms are spelled out on first use.

2. Line 166: The no (or zero) oscillation depth (ZOD) should be instead of depth of zero annual amplitude (DZAA). DZAA is a professional vocabulary in the field of permafrost research.

As we replaced ALD with ALT, we replaced ZOD with DZAA in the whole document to be using the standard terminology of permafrost research.

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