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Hydrology and Earth System Sciences
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Discussions

Interactive comment on "On the Appropriate Definition of Soil Profile Configuration and Initial Conditions for Land Surface-Hydrology Models in Cold Regions" *by* Gonzalo Sapriza-Azuri et al.

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GENERAL COMMENTS

The manuscript presents a sensitivity analysis of a Canadian one-dimensional land surface model, MESH to the thickness of modeled soil profile and the length of model initialization period. The main conclusions are that a soil profile of 20m or greater is necessary for this particular model to represent the energy dynamics of permafrost, and that an initialization period much longer than 100 years is necessary to condition the model properly. The same results have been reported by a number of previous researchers using different permafrost models, and the present study confirms well

known facts. A new contribution of this study would have been to present a rigorous and systematic evaluation of the model sensitivity to soil profile thickness and initialization period. Unfortunately, the study falls short of delivering a new contribution due to a few important deficiencies in model boundary conditions as I explain below. I would suggest that the authors use appropriate boundary conditions and conduct new model sensitivity analyses that are scientifically defendable.

Response to General Comments

We thank the referee for his/her insightful comments.

While we certainly agree with the referee on the significance of geothermal flux, we would point out that the difference between the common implementation of the current generation of Land Surface Schemes (LSS), applied as the lower boundary condition for regional/general circulation models and hydrological applications, and that of permafrost models used to predict and evaluate the evolution of permafrost. In the former, the geothermal flux is commonly ignored in the literature and most of existing models do not have the parameterization to include it (the common assumption is no heat flux at the bottom of the soil layers), while in the latter, geothermal flux is considered an essential component of modelling.

In response to the referee's comment, we have extended the analysis by including a new set of simulations with a constant geothermal flux (0.083 W/m2) at the bottom, based on available measurements at Norman Wells (Garland, G.D. and Lennox, D.H., Heat Flow in Western Canada. Geophys. J.R. Astron. Soc., 6,245-262, 1962).We have run the same set of 50 parameters with 17 different soil configuration combinations (850 simulations) for the average climate year (1945). Figure 1 shows the results obtained. Although some small differences are observed, the conclusions remain the same (e.g., 20 meters of soil depth are needed). The main difference is seen in some soil configurations having slightly

warmer soil profile when "ggeo" flux is included. Figure 2 shows the cumulative distribution function of the difference of soil temperature at the non-oscillation depth with and without ggeo flux at the bottom. Approximately, in 60% of the simulation the difference is within +/-0.15°C.

Upper boundary condition. We have corrected the description of the site location and clarified the assumptions made about the upper boundary condition. The land cover in the manuscript has been corrected to be a composition of moss lichen groundcover, ericaceous shrubs, black spruce and tamarack trees in an open canopy density (Smith et al., 2004). In this study, we perturbed the canopy parameters by a Monte Carlo analysis, not using a specific land cover type based on a look-up table. The range of variation selected in such a way that it covers most of the possible land covers present in the area. The purpose of that was to analyse the uncertainty in parameter values on the definition of the soil configuration and in land surface schemes that are typically run at a grid size ranging from ~10*10 to ~250*250 km2 (which can be different from an analysis performed to represent the processes at a point).

We have included two new sections to describe the boundary conditions used and to show the results obtained of the geothermal flux at the bottom.

To better define the scope of our work we have restructured the introduction to better reflect the novelty of our contribution.

SPECIFIC COMMENTS

1. P3, L13-15. This paragraph seems to be out of place. I suggest deletion.

Thank for your suggestion, we have removed the paragraph.

2. P4, L8. It is very unusual to have a 'grassland' ecosystem in a place like Normal Wells. I would suggest that the model be run with appropriate parameters to represent the vegetation typical of this environment.

We agree with the referee. The confusion here it is was derived from the Land Cover map used in this analysis, that came from a reclassification of a land cover map from a bigger area for the Mackenzie basin, where shrubs, grass and other cover were grouped together in a single unit unfortunately named grassland . In addition also the original pixels were upscaled and we only pick the dominant land cover type.

However, in really, as the canopy parameters were perturbed by a Monte Carlo analysis, in fact, we have not used a specific land cover type based on a look-up table. The range of variation cover most of the possible land cover present in the area. The purpose to do not attach to a specific set of parameters representing a land cover was to show that regardless of their value you need to have a deeper soil configuration in cold regions. To avoid any kind misunderstanding we have corrected the land cover specific for the place adding a complete description of the site vegetation and canopy based on the site description reported in Smith et al., (2004). We removed any grass land cover reference from the text. As explained, we considerate that re-run the simulation are not necessary.

3. P5, L4. The data from this borehole (84-1) is critically important for the evaluation of model performance. The model should be set up with the top boundary condition representing the vegetation characteristic of the local site, because the model simulation is compared against local data. The borehole data and site characteristics (including photographs) are publicly available from a report published by Natural Resources Canada. It appears that this site is located in a wetland surrounded by black spruce forests, typical of the Normal Wells region. To present a rigorous analysis (P19, L8), the model should use a set of parameters for wetlands, not grasslands for the top boundary.

Please see response to comment #2.

4.P6, L22. Again, grass land cover is inappropriate for this particular model simulation.

Please see response to comment #3.

5. P7, L1. The critical importance of geothermal heat flux applied to the bottom boundary is widely recognized by researchers in the permafrost modeling community, and is considered the standard practice. Geothermal heat flux data for the study region is readily available and used in previous studies (e.g. Zhang et al. 2008, cited by the authors). The absence of heat flux at the bottom boundary calls the scientific rigor of simulations in this study into question. I would strongly recommend that the authors re-run the simulations using a proper bottom boundary condition. If the model cannot handle geothermal flux, then it is not an appropriate modeling platform for permafrost environments.

We understand and also share your point. As we respond to the general comments, the focus of the present analysis was more to address the kind of Land Surface Schemes commonly used as lower boundary condition for regional/global circulation models and hydrological applications, rather than the kind of permafrost models used to predict and evaluate the evolution of permafrost presence. In this kind of application there is almost no inclusion of geothermal flux (model do not have the parameterization) and the common assumption is no heat flux. Class allow to include a constant geothermal flux at the bottom, we have included a sub set of simulation to compare the effect of geothermal flux as was described in the response to general comments.

6. P8, L6. Please report mean annual air temperature and total precipitation for these years, preferably in a table format.

Added in Table 2.

7. P9, L15. This statement is true with respect to annual temperature oscillation. However, the effects of lower-frequency temperature fluctuations (see Figure 6) can penetrate much deeper into the soil (see Figure 2). For a proper evaluation of model sensitivity, the non-oscillation depth should be defined using simulated temperature

over multiple years.

Thanks for the comment. However, in the Experiment 1 we are running in a spin-up mode recycling the same year over 2000 times. After that cycling we assume that a quasi-equilibrium between climate condition and the ground thermal state was reached for a year. One of the things that we are trying to show here is the effect on the selection of climate condition to stabilize a model, so only one year is used.

8. P10, L9. It is not clear what is shown in Figure 5. The figure caption says it is annual average temperature, but it clearly is not. Please explain.

We apology for the confusion here, maybe the selection of words were not the best. The label: "... Trend comparison of annual average air temperature with subtracted mean for the whole period ..." have been modified to "…Trend comparison of residual of the difference between annual average air temperature and...".

9. P14, L7. As I mentioned above (P9, L15), the temperature invariance in annual time scale does not necessarily indicate the insensitivity of the model to soil profile depth when lower-frequency fluctuations in atmospheric forcing are considered.

Please see response to comment #7 P9 L15.

10. P16, L12-14. In addition to temperature, important variables in permafrost environments are the depth to the permafrost table (i.e. top of the permafrost) and the thickness of permafrost, as they exert strong influences on energy and water transfer processes. It is highly desirable to evaluate the model performance with respect to these key variables.

Thanks for the comment. However, as we responded to comment 5, the kind of model that we are addressing in the manuscript are more related to the common Land Surface Model used in Regional/Global Circulation and hydrology models.

It is out of the scope of the paper to have a complete and exhaustive permafrost simulation. Finally, we try to keep it simple, the analysis already have huge number of comparison, and we prefer to maintain the selected variables showed.

11. P19, L8. I cannot agree that this study presents a 'rigorous' analysis, as it suffers from fundamental problems concerning model boundary conditions. Please revise the boundary conditions and re-run the model simulations.

We appreciate your comment. Of course that word 'rigorous' has some implications, however, we have jointly cover many source of uncertainty not analyzed before, that could affect the definition of the depth of the soil configuration and how is initialized. Again, as we pointed out in response to comment #5 and #10 the scope of the paper is in other kind of models.

Figures Caption

Figure 1 2d-Histogram of SCD and hT-non-oscillation depth. Counts are normalized by the number of simulation by SCD. The black line represents the limit to reach or not the hT-non-oscillation conditions. Bins to the left represent SCDs that never reach the hT-non-oscillation condition.a) No Geothermal flux, b) Constant Geothermal flux at the bottom of the soil layers

Figure 2. Cumulative distribution function (CDF) of the error of soil temperature at hT-non-oscillation condition computed as the difference between without and with ggeo flux at the bottom of the soil layers.

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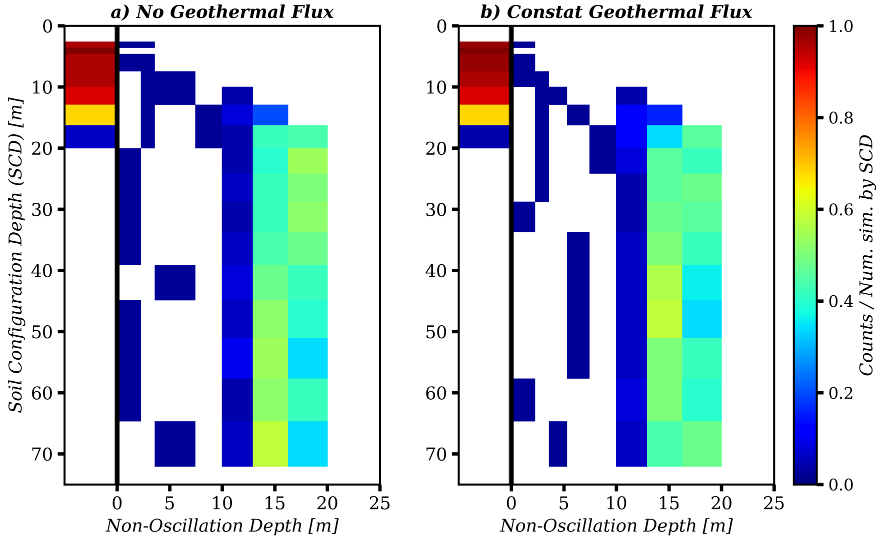

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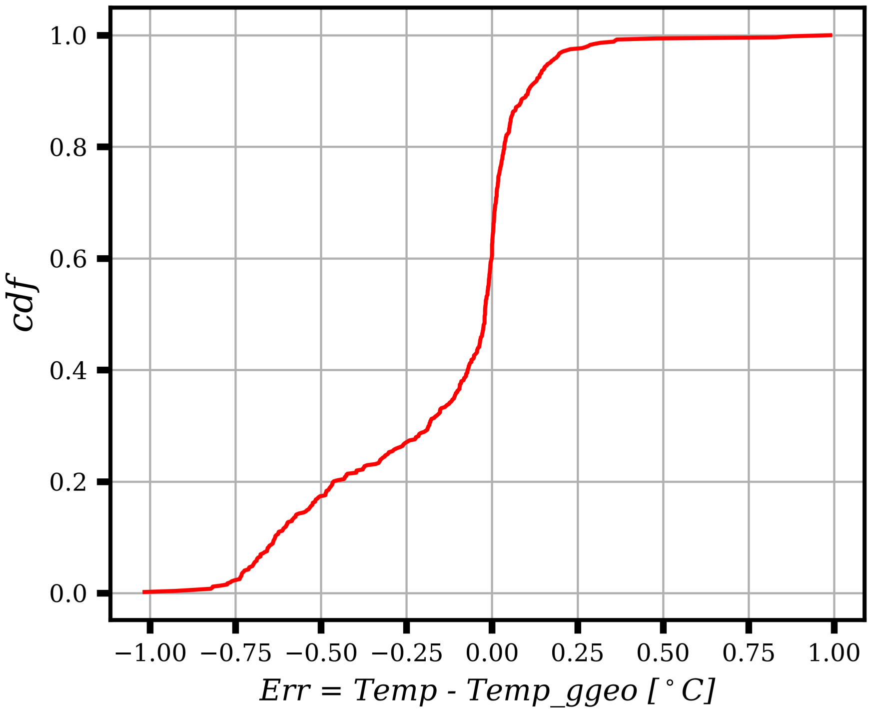

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