

## ***Interactive comment on “Modeling the spatio-temporal variability in subsurface thermal regimes across a low-relief polygonal tundra landscape” by J. Kumar et al.***

**Anonymous Referee #1**

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The manuscript “Modeling the spatio-temporal variability in subsurface thermal regimes across a low-relief polygonal tundra landscape” by Kumar et al. presents high-resolution simulations of the ground thermal regime for tundra polygons in N Alaska. The manuscript seems publishable in TC after major revisions, although some crucial information on the model setup (mainly the exact choice of the lower boundary condition, see details under minor points) is missing, so that the soundness of the approach cannot be finally determined.

Major Points:

1. The simulations are driven by temperature measurements at the surface, as it was common in early 2000s-publications on 1D-heat conduction schemes. Such simple

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approaches are generally no longer publishable in a journal like TC today. The authors add 3D-coupling of both heat and water transfer as a new state-of-the-art feature, which removes many of the limitations inherent in earlier approaches. However, the results are rather mediocre at best. While Figs. 8 ff seems to suggest a rather OK fit with measurements, it is actually not the case for active layer thickness (ALT). Although it is hard to see in the figures, the model predicts an ALT of ca. 100 cm at Site A, while it is in reality <50 cm. The same is true for Sites B, C, and D. This is much worse than many traditional 1D-schemes (which may or may not be tuned) and makes the model results virtually useless for further applications, where ALT is of interest. Even worse, the authors can present only a single year, so that it is impossible to determine whether the model can reproduce interannual variability of ALT and ground temperatures, or (e.g. decadal) trends in these variables, which is crucial for applications in the context of climate change. If the bad performance for ALT is indeed true, it must be clearly stated and the reasons investigated and discussed. The conclusion of the manuscript should then be that the model in the presented set-up is NOT suitable for studying the ground thermal regime of polygonal tundra near Barrow.

2. I doubt that the model scheme presented by the authors is scalable due to the computational requirements, so that it could be included in ESM frameworks or similar schemes. So what do we learn from the simulations of the four polygons then? I do not see that the study can provide any new insight in processes or process parameterizations. The main message seems to be that the authors managed to launch a model scheme of unprecedented complexity and computational requirements, but new insight in cryospheric processes and model parameterizations hereof are largely absent in my view. An example for a slightly similar study that does a significantly better job in this respect is Weismüller et al. (2011). They demonstrate a coupled 1D-scheme for heat conduction and water flow and use this model to show that a heat-conduction-only model scheme is more or less sufficient to reproduce the ground thermal regime at their study sites. So it would for instance be highly interesting, if a comparatively simple 1D-heat conduction model (e.g. GIPL2) with year-averaged

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ground properties/water contents could yield a similar performance for the center/rim sites. The authors could investigate if 3D-coupling for both heat and water fluxes is really needed, or if 3D-coupling only for water fluxes is needed, or 3D-coupling for heat fluxes only. Such information would be crucial to help designing a robust scalable scheme for representation of polygonal tundra in ESMs.

Ref: Weismüller, J., Wollschläger, U., Boike, J., Pan, X., Yu, Q., and Roth, K.: Modeling the thermal dynamics of the active layer at two contrasting permafrost sites on Svalbard and on the Tibetan Plateau, *The Cryosphere*, 5, 741-757, doi:10.5194/tc-5-741-2011, 2011.

3. Excess ground ice is a main driver for the evolution of polygons and melting of excess ground ice will lead to changes of the microtopography, which in turn changes the hydrological regime. Are such processes represented in the model scheme? This should be explicitly stated and commented upon in the manuscript. If yes, is the surface stable during the 1-year test period? Are there sites where excess ground ice melt is observed and which could be used to test the model performance? If not, a key element determining the evolution of tundra polygons is missing, and it should be clearly stated that the scheme is not suitable for climate change studies in polygonal tundra.

4. The authors should state more clearly that the presented model is only a very first step towards a physical model of energy and water transfer within tundra polygons. Many of the key drivers of spatial variability in the system are implicitly prescribed by the forcing data (2cm temperature measurements) and not modeled. The authors present curves of snow depths at the various sites, but which factors lead to these differences? (How) could this be modeled? The same is true for vegetation, surface energy balance, evapotranspiration, etc. The authors state that coupling to CLM is planned, but many crucial processes (e.g. wind drift of snow) are not contained in CLM, since it is mainly designed for large-scale applications.

Minor Points:

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Fig. 1: strange values given in the color bar, units ([m a.s.l.]?) should be provided.

Fig 2: zero-degree-line missing in b

P. 8, L25: This is a major design flaw of the study which questions the use of such a sophisticated model scheme. Are there plans to obtain such data sets in the future?

Sect. 3.1.3 How about the vertical discretization of the system?

Sect. 3.2: please provide a clear overview of the processes and parameterizations that are considered in the model (and the ones that are not), i.e. heat conduction, saturated flow, unsaturated flow, water vapor transport, how is the freeze curve determined, etc., etc.

P.14, l. 10: Why -1 degree, that appears to be much too warm??

P. 14, l. 10: At what depth is the deep bottom boundary? How does the choice of the lower boundary condition interfere with the selected spin-up-procedure? What are the resulting temperature gradients below the depth of zero annual amplitude? Why not use a heat flux as lower boundary condition, and perform a spin-up so that steady-state conditions are reached in the entire model domain?

P. 14, l. 11: West Dock is several 100 km E of Barrow – how realistic is this assumption and in how far would errors in this temperature compromise the results. Does this temperature roughly correspond to a steady-state condition given the applied surface forcing, or does it introduce a heat sink/source at the bottom? See also comment above.

P. 14, l. 23: What is “thermal hydrology”, and why is soil moisture not discussed? This doesn’t make sense to me since it is one of the assets of the new model, that the 3D-interplay between moisture and heat fluxes is explicitly considered. The authors should investigate their model results further to show how for instance water fluxes change the thermal properties of the system, which in turn affects heat conduction.

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P. 14, L. 5: How is it determined that periodic steady-state conditions are reached?

P. 14, l.9ff: The authors should quantify the magnitude of the advective heat flow, and set them in relation to the conductive heat fluxes. Could a similar model accuracy (considering Figs. 8 ff) be achieved when such fluxes are neglected, as it is done in most model approaches? See major comments.

P. 14, l. 20: Any idea on the accuracy of the precipitation measurements? And why do the authors use daily precipitation, not better resolved in time? Are only daily values available?

Fig. 13: Why is the thermal conductivity for saturated soils higher with some liquid water compared to fully frozen conditions? Is that a real physical process, or an artifact of the employed parameterization? If it's the latter, what is the effect on the simulation results?

P. 20., l. 1ff: I very much agree with this statement! Therefore, many of the results could be strongly influenced by the particular parameterization of the thermal conductivity chosen by the authors. It is a standard parameterization used in many models, but it is not based on first principles and could thus be prone to biases at the particular study site. This is in particular crucial since the authors attempt to reproduce fine-scale ground temperatures, rather than provide a coarse assessment of the ground thermal regime with a simple thermal model.

P. 23, Sect. 5.1: I like that the authors present non-optimal fits between measurements and model, rather than tuning the model to fit the available ground data perfectly. In addition, the authors could/should present a sensitivity analysis at least for some of the crucial model parameters, to show which parameters need to be better estimated in order to improve results. But this is probably difficult due to the model complexity and computational requirements?

P. 24, l. 3: Not sure this is explainable by missing soil properties, etc. The bias is

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systematic, and the authors should also investigate and comment on the short time period (1y) of their runs and the way they handle spin-up and the lower boundary condition (see comment above).

P. 26, l. 3: The authors do not provide any quantitative evidence that the hydrology is really reproduced. In a qualitative way, it is (high rims are dry, depressed centers wet, etc.), but quantitative validation information is not presented. Therefore, this statement should be formulated more carefully.

P. 26, l. 7: The authors must present evidence (e.g. sensitivity analysis) that the bias in temperatures is really explainable by deep soil properties (see comments above). If so, they should elaborate on which soil parameters have the largest influence on simulation results.

P. 26, l. 7: The statement on C fluxes is misplaced in this discussion.

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Interactive comment on The Cryosphere Discuss., doi:10.5194/tc-2016-29, 2016.

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