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Interactive comment on "Evaluating integrated surface/subsurface permafrost thermal hydrology models in ATS (v0.88) against observations from a polygonal tundra site" *by* Ahmad Jan et al.

Anonymous Referee #2

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In the article the authors evaluate the permafrost hydrology model ATS (v0.88, but with equivalent physics to v0.86 described in Atchley et al. (2015)), against various observations (soil temperature, water table, evaporation, snow depth) from a field site in Alaska which is characterized by ice-wedge polygons. Compared to the model description paper by Atchley et al. (2015), the article evaluates the simulations against further types of observational data, and the authors used a novel mesh geometry intended to represent various polygon morphologies by a single effective geometry. The authors furthermore conducted sensitivity analyses for a range of hydrology and snow parameterizations.

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Discussion paper

General comments

The paper is well written and addresses a relevant type of numerical model, which will be useful for an improved understanding of the thermal and hydrological dynamics of permafrost-affected terrain. Whether the presented model is also suitable for "watershed-scale projections of permafrost dynamics in a warming world", as suggested by the authors, is less obvious, as it lacks representation of surface deformation processes resulting from melting of massive ground ice.

The methodology of the study is sound, but some more details could be provided in order to assure reproducibility and to facilitate the comparison of model performance with similar types of models. Some of the assumptions should be motivated and justified in more detail (see specific comments below). While most conclusions are supported by the presented data, some conclusions would need support by the presentation of further results and/or the conduction of additional simulations (see specific comments). The title of the article is adequate and the language is mostly concise and understandable. Some sections (e.g., 3.2, 3.3, 4.5) would benefit from adding dedicated subsections. Some figures could be combined, left out, or moved to the appendix. The quality of some figures should be improved (see technical comments). The provided references are appropriate and up-to-date.

To my opinion, the article bears the potential for publication in GMD, if the authors (i) provide further justification for parts of the methodology, (ii) provide further evidence for some of the conclusions, (iii) discuss limitations of the model setup, and (iv) improve the contents and quality of the display elements.

Specific comments

- The assumption that the "abstracted geometry" of mixed polygon types is simultaneously representative for polygons of different types and polygonal tundra in general, is not supported by any evidence. There is evidence from other studies (e.g. Liljedahl et al. (2012)) that different polygon morphologies affect lateral hydrology in a non-linear

way. It is thus not a trivial step to assume that a “linear mixing” of different morphologies in a single radially asymmetric polygon is representative for all these morphologies at the same time. This is particularly problematic because one of the findings of the study – the strong coupling between water tables in troughs and centers – might not hold true for other types of polygons. For example, in polygons with (radially symmetric) high elevated rims, the centers and troughs would be hydrologically disconnected until the thaw depth in the rims reaches down to the elevation of the water table.
Proving that the abstracted, radially asymmetric polygon geometry is indeed representative for several polygon morphologies at a time, could instead become an objective of the study. This would, however, require complementary simulations for radially symmetric geometries of both types (high and low rims). If no further evidence for the representativeness of the “abstracted” geometry can be provided, the limitations of this setup and the validity of the conclusions should be discussed more clearly.

- Using measured water tables in polygon troughs as a forcing at the lateral boundary of the surface model domain seems rather unconventional, as such data are typically not provided by other models (as it is the case for the meteorological forcing data). In my view, the dynamic evolution of the water table throughout the thawing season is a variable a permafrost hydrology model seeks to predict based on the meteorological forcing, and the thermal and hydrological processes in the surface/subsurface system. If the elevation of the water table above the surface is, however, prescribed at the model boundary, as it is the case in the present study, the good agreement with measured water levels in the center is not very surprising, at least for low-elevated rims (see also previous comment). This procedure thus clearly limits the transferability and scalability of the approach.
Based on these simulations the authors find a good agreement between simulated and observed water tables in the polygon centers, suggesting an important role of lateral water fluxes between troughs and centers (p. 19 l. 351ff). This conclusion would become stronger if a further simulation with more simple hydrological boundary conditions for the surface (e.g., no-flow, seepage face, or a spill point at a fixed elevation) would

be conducted and included for comparison. In this respect, it might also be interesting to provide data on simulated lateral water fluxes between polygon centers and troughs (either as a time series or as net fluxes), and to assess the contribution of these fluxes to the water balance of the centers.

- The evaluation of the modeling results is mostly limited to a visual comparison between simulations and observations. For the scope of a model evaluation paper it would be desirable to provide also more quantitative measures of model performance such as RMSE, R^2, and/or bias. This would also facilitate the comparison with other studies that provide such numbers (e.g., Kumar et al. (2016), Abolt et al. (2018), Nitzbon et al. (2019)).

- As the active layer thickness is a key quantity for permafrost ecosystems, it would be desirable if the authors could also provide an evaluation of the temporal evolution of thaw depth, and its spatial heterogeneity between the different parts (center, rim, trough) of the polygon (provided that suitable observational data exist for BEO).

- The presented evaluation of the simulated evaporation is not very convincing. Figure 7 shows only simulation data and is thus not helpful in terms of comparison with observations. Providing the accumulated net evaporation (in [mm]) for the micro-topographic units (centers, rims, troughs), as well as providing the corresponding measured values of Raz-Yaseef et al. (2017) would be more insightful. Fig. 7 could then either be merged with Figure 8, moved to the appendix, or just left away. The time series of upscaled evaporation in Figure 8 is not suitable for a quantitative comparison between observations and simulations. It would be more insightful to provide accumulated values of net evaporation over those periods for which both measured and simulated data are available. Discussing the net evaporation together with precipitation and lateral runoff, i.e. putting it into context with the full water balance of the site, might add further relevance to the study.

- The additional simulations conducted for the sensitivity analyses are not described in the Methods section, but rather in the Results section. The respective paragraphs should be moved to the Methods section. Making use of subsections in section 3.3 might improve readability.

- The claimed existence of a “null space”, i.e. an opposing effect of saturated hydraulic conductivity and the parameter d_l (p. 16, l. 309ff), is not sufficiently supported by the provided results, since only one parameter is varied at a time. Showing that a co-variation of the parameters (e.g. decreasing d_l while increasing K) does not affect the results significantly, would strengthen this point. However, it might still be valid only for the considered polygon morphology and is not necessarily a general relation between the parameters.

- In general, the results of the sensitivity study could be explained and discussed in more detail. For example, it is a very interesting result that the initial snow density dynamics is crucial for accurately simulating accurately the duration of the zero curtain. Such insights are valuable for other modelers and could thus be elaborated more prominently.

- The limitations of the model setup should be discussed more extensively, particularly if the model is supposed to be used for projections of permafrost dynamics in a warming climate. One of these limitations is the static surface topography of the polygonal terrain, which cannot change in response to melting of massive ground ice.

- It might be considered to restructure the Results section into two parts, one for the comparison with measurements, and one for the sensitivity analyses, but each with appropriate subsections.

Technical corrections

- The lower right panel of Fig. 1 lacks a legend with a colorbar as it seems to be different from the one in the upper right panel.

- The information provided in Fig. 2 are not essential for the main text and could thus be moved to the appendix. Instead, it would be sufficient to provide annual or seasonal averages for the temperature and the precipitation in the main text. It would also be interesting to provide longer-term climatological characteristics for the study area.

- The figure and axis labels in Fig. 3 should be increased and a colorbar added to the panels on the right.

- Presentation of temperature data (Figs. 2, 5, 9, 11) is much more convenient in degree Celsius instead of degree Kelvin, and would thus facilitate easier comparison with the results of other studies.

- The labels of the time axes should be presented in a more convenient format, e.g. mm/yyyy, instead of decimal years.

- The legends of Figs. 6 (right panels) and 10 are incomplete.

- Table 1 should be complemented by the values for saturated hydraulic comparison in order to facilitate comparability with other models or field sites.

- It would be nice to provide an overview over the settings of all conducted simulations, including the sensitivity analysis, e.g. in form of an additional table.

- P. 8, l. 175: not clear whether the mentioned “depth hoar” option is enabled or disabled for the presented simulations.

- P. 9, l. 194 “from literature”: Provide further references if not all values are taken from Hinzmann et al. (1991).

- P. 9, l.199 “100-200 times”. How many exactly? Or, why is there a range?

- P. 9, l.205-208 “As described ... sublimation, and melt.”. The same information were provided already in the Methods section (where they belong) and can be left out here.

- P. 12, l. 249: "." missing

- P. 15, l.294 “Fig. 10(left)”: Should be "Fig. 10(right)", correct?

References

All references in this review are contained in the reference list of the discussion paper.

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