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# *Interactive comment on* "Soil moisture redistribution and its effect on inter-annual active layer temperature and thickness variations in a dry loess terrace in Adventdalen, Svalbard" *by* C. Schuh et al.

**Anonymous Referee #1**

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The work presents a very simple endpoint sensitivity analysis of van Genuchten parameters and therefore soil water retention affect subsurface thermal hydrology, with specific attention paid to ice re-distribution due to cryosuction and unsaturated hydrology. The work is very specific to a dry site, and therefore has limited broad application to other sites within the pan-Arctic region. Similar and more extensive studies of subsurface hydro thermal parameters have been conducted previously, but to my knowledge few if any have been done in 'dry sites' and which compare results to more than just observed subsurface temperature, namely the inclusion of soil moisture, which is absolutely necessary when assessing the sensitivity of van Genuchten parameters.



I believe this inclusion of field data with the in depth modeling exercise produced some valuable insight into unsaturated thermal hydrology, which may prove valuable to the cryosphere community if the authors are able to focus both in the introduction and discussion of the need to quantify water retentions properties. The work is generally sound and free of technical errors and the authors do a fairly good job of making appropriate conclusions given the constraints of the modeling approach. Furthermore, the writing is clear and grammatically correct though not very concise or focused.

While I believe that this work will eventually achieve full publication I recommend that authors consider revising the manuscript to clearly state assumptions made in the modeling application, which have implications with regards to the interpretation of the results, though not necessarily problematic implications in my view. Furthermore, given the simplicity of the modeling exercise and the narrow scope of only perturbing two parameters within the van Genuchten equation, I believe it is vitally important to clearly motivate within the introduction why understanding water retentions regimes in permafrost systems is necessary. In this version of the manuscript, the introduction is unfocused and instead reads like a history of what research has been done regarding permafrost without much attempt to link it to soil moisture redistribution.

Major comments: 1) I therefore assume that there is no prescribed or simulated water fluxes in and out of the model domain, though it is not explicitly stated. While I see no huge reason why this would affect the validity of most of the results presented here, it should be remembered that any interpolation of the seasonality of the results should be taken with a grain of salt. In the results section the authors do an admirable job of pointing out when the model set-up without water fluxes in or out of the domain is responsible for deviations from observations. However, it maybe good in be supper clear about this set-up and state that what the boundary conditions of the model is. Particularly, that there is a no (water) flow in and out of the domain. In some documented cases water fluxes in and out as well as the shifted water retention location can have big consequences on the thermal regime of the subsurface i.e. [Atchley et al., 2016;

Helbig et al., 2013; McKenzie and Voss, 2013; Sjöberg et al., 2016].

Atchley, A. L., E. T. Coon, S. L. Painter, D. R. Harp, and C. J. Wilson (2016), Influences and interactions of inundation, peat, and snow on active layer thickness, Geophysical Research Letters. doi: 10.1002/2016GL068550.

Helbig, M., J. Boike, M. Langer, P. Schreiber, B. R. Runkle, and L. Kutzbach (2013), Spatial and seasonal variability of polygonal tundra water balance: Lena River Delta, northern Siberia (Russia), Hydrogeology Journal, 21(1), 133-147. doi: 10.1007/s10040-012-0933-4.

McKenzie, J. M., and C. I. Voss (2013), Permafrost thaw in a nested groundwater-flow system, Hydrogeology Journal, 21(1), 299-316. doi: 10.1007/s10040-012-0942-3.

Sjöberg, Y., E. Coon, K. Sannel, A. Britta, R. Pannetier, D. Harp, A. Frampton, S. L. Painter, and S. W. Lyon (2016), Thermal effects of groundwater flow through subarctic fens: A case study based on field observations and numerical modeling, Water Resources Research. doi: 10.1002/2015WR017571.

2) The work presents mainly endpoint and mid point evaluations of parameter space. While this type of exercise provides some insight into how parameters effect model output, there is no information about the middle parameter space and any non-linearity raising from combinations of van Genuchten parameters is hidden or lost. I would suggest that the authors attempt to simulate or at the very least discuss how combinations of van Genuchten parameters between those that are tested might behave. Could there be non-linarites as a result of untested combinations of van Genuchten parameters that lie with in the range of parameters tested?

3) It seems the central focus of the paper is how does unsaturated soil moisture distribution in the ALT and near surface permafrost layer affect the subsurface thermal regime at this relatively dry site. The introduction on the other hand reads like a history or what has been done, but it is my preference to use that history to highlight



why answering the unsaturated soil moisture distribution effect is important. This usually helps focus the paper and reader to why the results matter and produce a more precise manuscript.

Minor comments: Page 3 L 14-15: “Soil water retention is a critical, but highly uncertain parameter” I agree with this statement, and I believe the available literature also has evidence that supports this statement. Unfortunately, and despite the extensive literature cited in the introduction, the case that soil water retentions is critical, has not been made within the introduction of this paper, and therefore this statement and the purpose of the paper seems out to come out of no where. I suggest reshaping the introduction to be less of a history of what has been done to how the existing literature suggests that soil water retention may be important.

Page 5 Line: 12-13: “Active layer thickness was considered both for the grid centre, which is the point nearest the location of the ground temperature measurements, as well as for the average of all grid points” This is an awkward sentence. Do you mean ALT was measured at the grid center points and then averaged across an array of grid center points? I only see one observed time series in the figures, is this the average across the site?

Page 5 Line: 30: The unsaturated version of Darcy’s law is Richard’s equation.

Page 6 Line 31: Omit ‘Then’ in “Then different. . ..”

Page 7 Line 2: It should be noted that setting residual saturation to zero in all cases 1) may produce the largest change in soil water content as all the water can drain out in dry cases, and 2) this formulation will allow all the pore water to go to ice during the winter, which will increase the winter thermal conductivity compared to systems where some pore water remains in a liquid state. Even thought the authors rightly point out that this assumption is often made it may still be worthwhile discussing these result in comparison to other more complete subsurface sensitivity studies such as [Harp et al., 2015], which includes residual saturation.

Harp, D., A. L. Atchley, S. L. Painter, E. Coon, C. Wilson, V. Romanovsky, and J. Rowland (2015), Effect of soil property uncertainties on permafrost thaw projections: a calibration-constrained analysis, The Cryosphere 10(3), 1-18. doi: 10.5194/tc-10-1-2016.

Page 7 Line 5: Omit 'Then' in "Then, both alpha . . ."

Page 7 Lines 5-10: I think this can be rephrased to be more clear and concise. Also, why were only 7 parameter combinations explored? Even though endpoint combinations can provide a lot of information about the behavior and sensitivity of parameters, there is little information about the model response to multiple combinations of parameters. Specifically any nonlinearities within the parameters space remain unknown.

Page 7 Lines 11-14: I think this needs rephrasing to be clearer, I would suggest something like, "Given that each combination of van Genuchten paramters will result in different soil moisture profiles under frozen conditions, each simulation test case with unique van Genuchten parameter combinations was spun-up and froze to attain unique ice-liquid-gas states"

Page 7 Lines 23-27: This provides reasoning in this modeling experiment to neglect water fluxes in and out of the domain. However the approach to neglect water fluxes is not clearly stated. While this is a huge simplification of the system I am ok with the approach, as long as it is clearly stated that a no water flux boundary is assigned. Please clearly state this boundary condition. Second, without the model able to represent transient water flows during the spin-up how can it be assured that the model is correctly representing the approximate amount of water in the system? Could this approach cause the mismatch between the observed and simulated water content in Figure 4? The reason I am ok with this approach here is that later the authors point out in the results when the model is unable to match observations. Which in my opinion highlights when representing a flux of water in and out of the system is necessary and when it is not, even for relatively dry sites, and thus becomes somewhat of a high-



lighted result in my opinion. This then begs the question, how much more important would representing surface and subsurface water flows in wet or highly transient sites be? Furthermore, given that van genuchten parameters were somewhat insensitive to subsurface temperatures in this study, would they be in sights the experience more transient hydrology?

Page 11 Lines 14-26: Though somewhat addressed in the next section (5.2), it maybe beneficial to discuss why the vertical movement or spreading of the ice thermal mass is important. I can invasion scenarios that create sharp or diffuse thermal gradients in the subsurface due to where and how concentrated the ice is.

Page 12 Lines 21-25: It would be interesting to extend the effective thermal conductivity evaluation to include differences in the location of ice mass in the subsurface, specifically compare the striated ice distribution (Fig 3, b) to the diffuse distribution (Fig 3. C). Does the striation of ice change effective thermal conductivity?

Page 13 Section 5.3: I appreciate this discussion that addresses ALT characteristics beyond the scope of soil moisture distribution and how seasonal differences i.e. winter versus summer, have been shown in literature and the present study to act differently on ALT. However, I think it too should be discussed within the context of soil moisture distribution. While in general it may be counter intuitive that ALT is more responsive to winter conditions then summer, but for those of us working on permafrost it makes since. In the Arctic winters are long, summers are short and the ground is mostly in a frozen state. Furthermore, ice is more thermally conductive than water and therefore a cold signal or lack thereof in the winter will propagate further into the subsurface. Given that winter conditions are important, this work should then address how does soil moisture distribution and therefore ice distribution in the winter moderate the winter time signal. Does it at all? If so, how does it? Given that this experiment is in a dry site with little water moving through the subsurface, can the conclusions be applied to wet sites with lots of subsurface flow? What further research would be necessary to answer these issues?

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