Response to Anonymous Referee 1

Referee comments shown as "RC:", author replies as "AR:".

RC: 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.

RC: 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.

RC #1: 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.

***AR #1:** We thank Anonymous Referee 1 for the valuable suggestions to improve and strengthen our paper; the text has been thoroughly revised and all comments have been addressed in our responses below. We acted on your suggestion and condensed the introduction section to better carve out the particular aim of this study. Also, we now stress the importance of soil water retention properties and the resulting moisture distribution more clearly when discussing thaw progression and active layer thickness variations over time.*

Major comments:

RC #2 MAJOR COMMENT-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.

AR #2 MAJOR COMMENT-1: *It is correct that we assume no water fluxes in and out of the system and therefore assign no-flow boundaries to the model domain. Advective heat transport by lateral water flow is likely to be important for many active layer systems and we now more clearly highlight this and refer to the relevant literature in our revised introduction section. However, at the very dry UNISCALM site, there is strong evidence that the system is largely unaffected by lateral water fluxes and very little infiltration occurs (see page 4, lines 19-24 of the revised text). This has been investigated in detail in previous studies [cited as Schuh, 2015], where summer rainfall and snowmelt infiltration derived from hydro-meteorological field site data was applied to the surface. In that study it was shown that the small amount of infiltration in comparison to the existing amount of ground/pore ice did not have a notable effect on the inter-annual ALT variation at that site. Section 3.3 of our manuscript has been revised to more clearly state the assigned boundary conditions (page 7 line 30 – page 8, line 2) as well as the motivation for deliberately neglecting water fluxes at this particular study site (page 8, lines 15-21): "Lateral water fluxes through and infiltration into the system were neglected and no flow boundaries assigned to all faces of the model domain. While recognizing that flowing water can have considerable thermal effects on active layer processes, its exclusion is considered an appropriate assumption for the dry UNISCALM site (cf. Sect. 2)."*

RC #3 MAJOR COMMENT-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 within the range of parameters tested?

AR #3 MAJOR COMMENT-2: *This study is laid out as a scenario analysis which is why we used a combination of average parameter values as reference case, varied both parameters α and m independently by ±50%, and defined two more cases as the higher and lower end of this parameter space. This is a careful selection of parameters corresponding to a systematic range of values for the soil texture type observed at the UNISCALM site (silt loam) and consistent with information obtained from the UNSODA unsaturated soil hydraulic database as well as with the range of retention parameters reported in literature for that soil class. It is correct that by testing these seven cases, any other parameter combination within this range or potentially outside this range remains unconsidered. While this choice of representative scenarios should be sufficient for our study, we acknowledge the necessity to point out the general value of systematic parameter investigations due to the nonlinear nature of the retention curve. This is now highlighted in our revised text (section 3.3 of the revised manuscript; page 7, lines 27-30, as follows: "Given that each combination of van Genuchten parameters will result in different soil moisture profiles, each simulation test case was set-up to attain unique ice-liquid-gas states. Still, despite the systematic approach of testing relevant parameter combinations, note that any potential nonlinearities arising from untested combinations within the parameter space remain unconsidered in this analysis.")*

RC #4 MAJOR COMMENT-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.

AR #4 MAJOR COMMENT-3: We acted on your suggestion and revised the introduction accordingly. We removed those parts that did not contribute directly to the main objective of this study, i.e. the more general review of permafrost-hydrological modeling studies and instead, we placed more focus on the relevance of soil moisture for active layer processes and its importance for climate change *investigations, especially in cold regions: "Both soil moisture and the thickness of the active layer have been identified as Essential Climate Variables (GCOS, 2015). Soil moisture is an important variable for energy exchange and governs the processes occurring at the land-atmosphere interface by partitioning incoming solar radiation into fluxes of latent and sensible heat (GCOS, 2015). Soil moisture controls subsurface physical properties such as thermal conductivity and heat capacity, so that the movement of moisture within the subsurface is essential for understanding the water and heat balance of the ground, particularly in cold climates (Wu et al., 2016). Still, most studies on permafrost thaw projections mainly investigate the structural uncertainty in climate models, whereas the parametric uncertainty in soil properties is barely accounted for (Harp et al., 2016)." (page 2, lines 12-18 of the revised manuscript).*

Minor comments:

RC #5: 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.

AR #5: *This section has been revised substantially. Please refer to our response to MAJOR COMMENT-3 (AR #4).*

RC #6: 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?

AR #6: *ALT was measured annually at all 121 nodes of the 100 m x 100 m UNISCALM grid (see page 5, lines 12-14). First, we consider the ALT measurements exclusively from the node in the grid center because it coincides with the location of the utilized subsurface temperature dataset (obtained from the Tinytag loggers, see page 5, lines 14-17). Second, we use the annual ALT average of all 121 grid nodes to reduce measurement uncertainty and obtain a more robust dataset. In our analysis, we refer to the ALT measurements as "grid average" and "grid center" (see Fig. 6a-b, Tab. 5 of the revised manuscript). In the manuscript, we clarified this paragraph by minor adjustments to section 3.1.*

RC #7: Page 5 Line: 30: The unsaturated version of Darcy's law is Richard's equation.

AR #7: *Yes, the model (ATS) couples RE with an equation for heat transport which accounts for latent heat transfer, where several constitutive relationships are used to close the system of equations, including Darcy's law and thermodynamic constraints. This is further clarified in the revised manuscript (page 6, lines 14-16).*

RC #8: Page 6 Line 31: Omit 'Then' in "Then different. . .."

AR #8: *Deleted: "Then".*

RC #9: 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.

AR #9: *We have updated the text and now include a highlight of our results in the context of the highly relevant study by Harp et al. (2016), as follows: "The missing sensitivity of soil water retention parameters to inter-annual ALT variation is not surprising. Also Harp et al. (2016) found the van Genuchten parameters not to affect subsurface temperatures in a long-term thaw projection study. This might be explained by the fact that in areas subject to a seasonal freeze/thaw cycle, the water retention parameters mainly control the seasonal soil moisture (re-)distribution during freezing and thaw (cf. 5.1). While we found the different retention curves to affect the rate of thaw progression and thus the respective annual thaw depth, the total period of active layer development from thaw to freeze-back was similar in all cases (cf. Sec. 5.2). Water retention characteristics therefore seem to be relevant mainly on a short time scale during thaw and freeze-up." (page 15, from line 30 onwards of the revised version).*

RC #10: Page 7 Line 5: Omit 'Then' in "Then, both alpha . . ."

AR #10: *The manuscript text has been clarified (page 7, lines 21-23).*

RC #11: 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.

AR #11: *This part of the manuscript text has been re-written to make the presentation more concise (page 7, lines 21-26). For the choice of seven parameter combinations and potential nonlinearities of the parameter space please refer to our response to MAJOR COMMENT-2 (AR # 3)*

RC #12: 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"

AR #12: *The section has been rephrased accordingly. We also included a clearer statement of the applied boundary conditions (page 7, line 27 ff).*

RC #13: 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?

AR #13: *This section was reformulated and is now more precise in stating the applied boundary conditions. For the assumptions made on water fluxes through the system please refer to our response to MAJOR COMMENT-1 (AR #2). Regarding the correct amount of water in the closed* *system, we generally represent field site conditions by saturating the unfrozen model domain below about 2 m depth. Assigning a pressure of -200 hPa as the top boundary condition before model freezing and spin-up resulted in a water content in the top cell of 0.11-0.32 (depending on scenario) and a linear increase up to full saturation (0.4) in the model domain below about 2 m depth. This is consistent with the water content both in the permafrost (0.4) and in the active layer (0.1-0.25) recorded at the field site. Please note that after freezing the model domain, each simulation test case was subject to a transient spin-up (allowing water to redistribute in the subsurface) until a periodic steady state was achieved (page 8, lines 6-9). Furthermore, previous modeling studies by Schuh [2015] (already cited) showed that infiltration estimated from meteorological field site data only accounted for an insignificant fraction of the existing water content in the system. Therefore, it seems unlikely that the mismatch in water content seen between field site data and some of the model cases is solely caused by the assumption of no water fluxes in to the system.*

RC #14: 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.

AR #14: *The manuscript was substantially revised with regard to the particular effect of ice striation on thermo-hydrological subsurface processes e.g. for the progression of the thawing front (please see our response AR#15 to comment RC #15 below). We also included a short general discussion on the relevance of the vertical spreading of the ice in section 5.3 (page 15, lines 30ff).*

RC #15: 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?

AR #15: *We extended our analysis on thaw progression in section 5.2 insofar as we now also consider the local rate of thaw within the soil profile in addition to the integrated consideration of the 1 m soil column (Tab. 7). We use the scenarios shown in Fig. 3 with different striation to illustrate the different rates of thaw in systems characterized by different soil moisture contents and distribution, ranging from diffuse to highly striated ice distribution. Please see the revised manuscript and the new Fig. 5 (relevant excerpts of the text reproduced in the following).*

"Furthermore, model simulations show that not only the amount of soil moisture, but also the distribution of ice within the active layer impacts the progression of the thawing front. Fig. 5 compares the ice content profile before the onset of thaw in 2011 to the respective thaw rate when the ground surface temperatures become positive. As shown previously (cf. Fig. 3), model scenarios 'ref case' (black), 'high m' (green) and 'max case' (orange) resulted in a distinct stratification of ice, whereas 'low m' (blue) shows almost a linear increase in ice content towards the permafrost table (Fig. 5a). In all scenarios, thawing the upper 10 cm of the ground occurs at a relatively low rate between 1.3-1.4 cm/d (Fig. 5b) mainly because air temperatures are still low, but also because of latent heat consumption due to the increased ice content in the thin layer just beneath the surface. Once the thawing front passes the ice layer at 5 cm depth, the thaw rate increases to its maximum of 5 cm/d, fuelled by markedly increasing air temperatures (not shown) and regardless of the respective soil moisture content. Below 20 cm depth, all scenarios show a generally decreasing thaw rate, but with notably case-specific differences only between 40 cm and 60 cm depth. Here, discrepancies originate from the particular ice-liquid-gas composition in the thawing ground and its effect on heat propagation, as discussed above. Furthermore, enhanced thaw due to advective heat transport through previously frozen water is likely to occur in more saturated systems such as 'ref case' (e.g. at 50 cm depth). Scenarios 'high m' and 'low m' show discrepancies in their thaw rate, while having comparable ice contents. In 'high m', the thaw rate drops to 1.4 cm/d when reaching the clearly defined ice-rich zone at 60 cm depth, whereas the thaw rate at that depth is much higher (2.5 cm/d) in the 'low m' case, characterized by a more stable ice content profile." (page 14, lines 7-23)

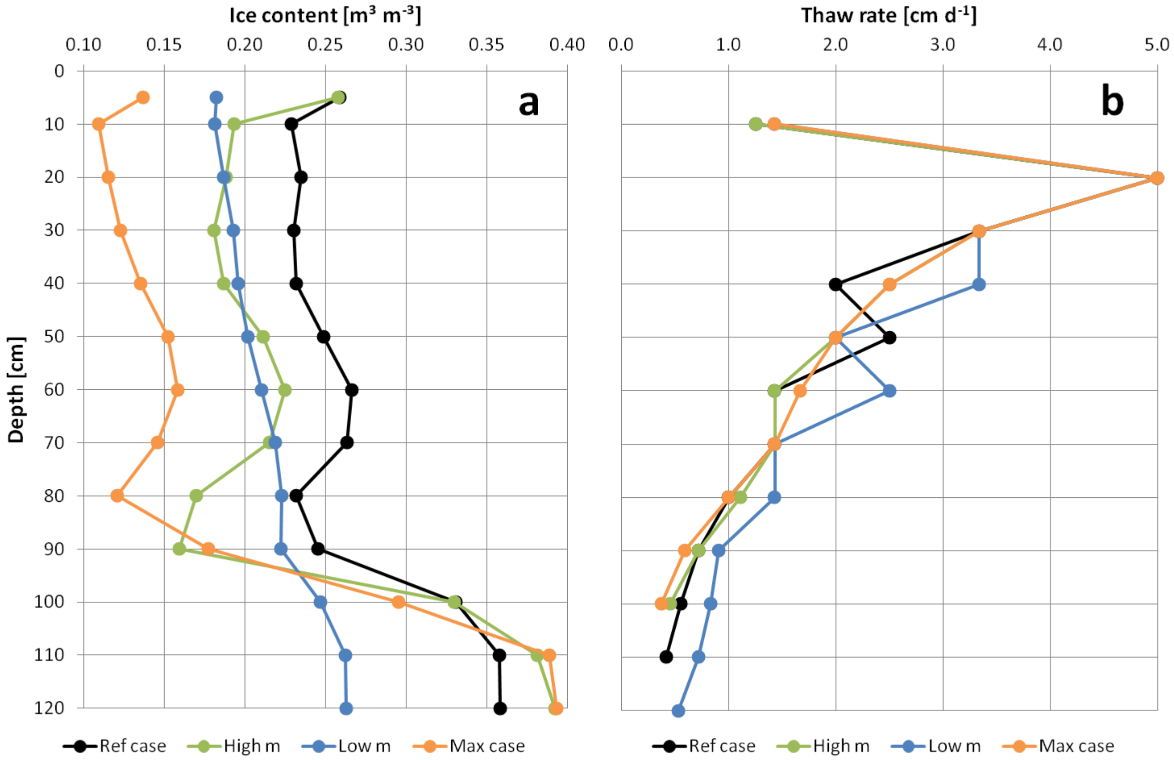

Figure 5: Comparison of (a) simulated ice content distribution with depth at the end of winter 2011 (01.05.2011) and (b) the corresponding modelled rate of active layer thaw starting after the first day of positive ground surface temperatures (25.05.2011), exemplified by four selected model scenarios.

RC #16: 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?

AR #16: *We appreciate your suggestion. Section 5.3 has now been extended by a more detailed paragraph about the role of the particular water retention curve for inter-annual ALT variation. This was an important step also to synthesize our findings and conclusions from the previous sections regarding the sensitivity of retention parameters to seasonal vs. inter-annual active layer dynamics. This way we believe to have polished the manuscript further to make in more focused on the importance of soil moisture (re-)distribution. Nonetheless, this comment certainly highlights a need for further research, in particular considering how sites with different hydro-climatic conditions may yield different active layer moisture/wetness conditions and ice dynamics.*

"The missing sensitivity of soil water retention parameters to inter-annual ALT variation is not surprising. Also Harp et al. (2016) found the van Genuchten parameters not to affect subsurface temperatures in a long-term thaw projection study. This might be explained by the fact that in areas

subject to a seasonal freeze/thaw cycle, the water retention parameters mainly control the seasonal soil moisture (re-)distribution during freezing and thaw (cf. 5.1). While we found the different retention curves to affect the rate of thaw progression and thus the respective annual thaw depth, the total period of active layer development from thaw to freeze-back was similar in all cases (cf. Sec. 5.2). Water retention characteristics therefore seem to be relevant mainly on a short time scale during thaw and freeze-up." (page 16, lines 30ff).