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The Cryosphere Discussions

Interactive comment on "Water flow in the active layer along an arctic slope – An investigation based on a field campaign and model simulations" *by* Sebastian F. Zastruzny et al.

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General comments:

This manuscript presents a study of hydrologic transport in the active layer of a slope in a permafrost environment on Disco Island, Greenland. It is based on a vast range of field data, including geophysical measurements, soil water content and electrical conductivity monitoring data, weather data, as well as a tracer experiment and numerical modeling. The objective is to quantify flow and transport mechanisms in the active layer.

The topic is very relevant as our understanding of transport in these systems is limited and our need to quantify these processes is growing as the Arctic warms and permafrost thaws. The methods are also suitable for achieving this objective.

However, the manuscript (and study) lacks focus. Many methods are applied, but they are not all well motivated and described in the Methods section. This mainly concerns the geophysical methods, which are used to delineate permafrost at the study site. As the thickness of the active layer is monitored by manual probing at the site, it is not clear how the geophysical data add any information for the active layer. There is also no estimate of uncertainty at all for the geophysical data, which also make it impossible to judge if they provide any valuable information for deeper parts of the ground. Finally, as the focus of the study is transport in the active layer, I wonder if there is at all a need for the geophysical data in this study. If the Authors do a more thorough uncertainty analysis of the geophysical data, it could perhaps be used to add some information about hydrologic connectivity through taliks. Otherwise, I think the geophysical data could be removed from the study, yielding a more focused manuscript.

Reply:

We think that the geophysical data provides valuable extra information about the spatial variability of field site, which is an important issue in terms of describing the water flow in the area.

The GPR data is used to measure the active layer thickness in between the positions, where the stickprobing was done. As simple interpolation of the probing yielded results comparable in quality to the GPR data, the latter was used to represent the active layer thickness in the model domain. Thus, the GPR profiling is a valuable additional source of information to represent the natural spatial variability of active layer thickness.

The resistivity data provide valuable information on the lower boundary of frozen material as well as the presence of taliks.

All together, we consider the geophysical data to be important for the study. Consequently, we have kept the geophysical data as an integrated part of the paper. As suggested we will analyze the impact of uncertainty to explore if more information can be extracted from the data.

A related concern is the inclusion of the subpermafrost aquifer in the modeling. The configuration of the modeling domain is based on the electrical resistivity tomography data. However, it seems like all the transport results presented in the manuscript regard the active layer, and it is unclear if including the subpermafrost aquifer yielded any additional insights.

Reply: The existence of the open talik at the top of the slope creates a hydraulic connection between the active layer and the subpermafrost aquifer and may therefore impact the water balance and the flow velocities in the active layer. Although this dependency was qualitatively tested in a sensitivity analysis, currently we do not have field evidence, which can document the interaction between the supra- and subpermafrost aquifers. It is correct that the focus of the study is on flow in the active layer and also that most of the water flow takes place in this horizon. Nevertheless, we find it important to allow for the interaction between the two horizons in the model. Furthermore, the turn-around time for the numerical model is very fast and therefore inclusion of the subpermafrost aquifer is unproblematic.

To summarize, I believe the manuscript (and study) could be much improved by focusing on its core methods, results and strengths. This might mean excluding some methods and data, but also highlighting more strongly what new insights were gained by this study. I hope that the Authors have the possibility to take the time needed to rework this manuscript and that my comments can be helpful in this endeavor.

Reply:

We will consider focusing the manuscript as suggested. However, we believe that the strength of the study is that it is based on a variety of experimental and measurement

methods and we are therefore inclined to retain them in the manuscript. Nevertheless, the comments provided by the reviewer are extremely valuable for the revision of the manuscript.

Specific comments:

Page 1, L19 What does “frost topography” mean? Do you mean the topography of the permafrost table?

Reply:

We thank for the clarification. Yes we mean the topography of the permafrost table. However, we did refrain from using the term ‘permafrost’ table, as we used the thermal definition of permafrost throughout the manuscript, which therefore does not represent a physical state.

Page 2, L2 First sentence: add that this regards permafrost areas.

Reply:

Will be done.

Page 2, L11 Frampton et al. (2011) does not deal with transport times, however Frampton and Destouni 2015 does: Frampton and Destouni (2015) Impact of degrading permafrost on subsurface solute transport pathways and travel times, Water Resources Research 51(9): 7680–7701.

Reply:

Yes, we agree and apologize for this confusion.

Page 2, L14 Please specify what lateral means in this context.

Reply:

In this context lateral means flow perpendicular to the primary flow direction.

Page 2, L14 Atchley et al. (2015) does not observe or study any “complicated flow netorks” or lateral flow per se, but acknowledges that this is a potentially important factor to consider when modeling heat and water in permafrost terrains. Perhaps there is a better reference for this (I don’t know of any – sorry!) or the sentence could be slightly reformulated?

Reply:

We will rewrite the paragraph to correctly refer to the results by Atchley et al. (2015).

Page 2, L28 – page 3, L12 This paragraph, focusing on nitrogen transport, takes up a substantial part of the introduction, yet this study does not focus on nitrogen transport per se. I suggest focusing the introduction more on general transport processes in the active layer, as this is the focus of your study. The existing text could easily be rewritten and the same studies could be cited, but with a stronger focus on the transport aspects.

Reply:

We will shorten the paragraph and focus the introduction more on solute transport.

Page 2, L29 – L30 What “surface” did Yano et al. apply nitrogen to? This sentence needs some specifications.

Reply: We will elaborate on the setup Yano et al. applied in their study.

Page 3, L20 Is this study designed to test this hypothesis? As you state that it is an hypothesis, readers will likely expect you to test this in the study, and I cannot see that you test for example ecosystem responses to additional nutrients and water. The idea that water and nutrients can be transported down a hillslope is not debated in our sciences, and your study design is more specific than that. Maybe you can reword a bit – “our underlying assumption”?

Reply:

We will reword this sentence.

Page 4, L8 This sentence needs some rewording to make it clearer.

Reply: We will reword this sentence.

Page 4, L15 The existence of a DEM does not need to be mentioned here. If it was used in some way for the study, it can be mentioned in the Methods section.

Reply:

We will mention the DEM in the method section and describe in which way it was used.

Page 4, L28 Was snow depth also measured at 30 minute intervals? (Was it used for this study?)

Reply:

The snow depth is measured at 30 minute intervals. However, we agree with the referee that the data is of no relevance for the present study and we will thus remove the sentence.

Page 5, L7 What does the reference to Woo (2012) refer to? The alfa of 1.26 is taken from Preistley and Taylor (1972), right?

Reply:

We agree that the reference at this point is not needed.

Page 5, L9 - L10 Where did you get the values for the wilting point and field capacity?

Reply:

Field capacity was based on repeated observation of soil moisture content after multiple days without rain at sensors close to the surface.

Wilting point was taken from the paper Moskal, T. D., Leskiw, L., Naeth, M. A., & Chanasyk, D. S. (2001), Effect of organic carbon (peat) on moisture retention of peat: mineral mixes. , Canadian Journal of Soil Science, 81((2)), 205-211, where wilting

point for peaty-sand sediment type is given.

This reference was by mistake not given in the manuscript.

Page 6, L1-5 These sensors were in addition to the ones at the weather station? Were the sensors calibrated in anyway? What precision and accuracy do you expect?

Reply:

The sensors were not specifically calibrated, but based on manufacturer specifications (Decagon Devices 5TE). The accuracies are according to the manufactorer's specification:

Apparent Dielectric Permittivity (εa): $\pm$ 1 εa (unitless) from 1 - 40 (soil range), $\pm$ 15% from 40 - 80 Soil Volumetric Water Content (VWC): Using Topp's equation: $\pm$ 0.03 m3/m3 ($\pm$ 3% VWC) typical in mineral soils that have solution electrical conductivity < 10 dS/m Electrical Conductivity (EC): $\pm$ 10% from 0 to 7 dS/m, user calibration required above 7 dS/m Temperature: $\pm$ 1°C

Page 6, L6 – L25 The idea here, as I read it, is to use ERT and GPR to get some constraint of the distribution of frozen ground, that can be used for the setup of the numerical model. However, to do this, some information on the quality of these data is needed. What is the resolution of the ERT results for this Wenner array with 30 cm spacing, in the active layer as well as deeper in the ground? Does the ERT result give you any information about the AL, given this resolution and inherent uncertainty? What data is used for determining the resistivity boundary of frozen/unfrozen ground? What uncertainty do you have on this boundary value? Generally, the uncertainty of an ERT inversed model increases with depth. There are ways to identify areas of high uncertainty in the image (e.g. Oldenburg and Li, 1999; Marescot et al., 2003; Fortier et al., 2008), which I urge you to use if you want to say something about the thickness of permafrost.

Oldenburg, D. W. and Li, Y. G.: Estimating depth of investigation in dc resistivity and IP

surveys, Geophysics, 64, 403–416, doi:10.1190/1.1444545, 1999.

Marescot, L., Loke, M. H., Chapellier, D., Delaloye, R., Lambiel, C., and Reynard, E.: Assessing reliability of 2D resistivity imaging in mountain permafrost studies using the depth of investigation index method, Near Surf. Geophys., 1, 57–67, 2003.

Fortier, R., LeBlanc, A. M., Allard, M., Buteau, S., and Calmels, F.: Internal structure and conditions of permafrost mounds at Umiujaq in Nunavik, Canada, inferred from field investigation and electrical resistivity tomography, Can. J. Earth Sci., 45, 367–387, doi:10.1139/e08-004, 2008.

Reply:

We agree with the referee that the quality and uncertainty of the geophysical data should be analyzed and described in more details. We will take advantage of the literature provided to strengthen this part of the manuscript.

Page 6, L7 Add that these are Electrical Resistivity Tomography (ERT) measurements.

Reply:

Will be done.

Page 6, L26 – L27 A lot of information is missing on the GPR measurements. What antennas were used (frequency, shielding)? Were the measurements done in a common offset approach? What was the antenna distance? What was the sampling time window? Were traces stacked? How were the measurements done along the transect, every X cm or every X s, or other method? How did you convert travel times to depth? What were the uncertainties, and the resolution? What uncertainties will stem from an assumption of homogenous velocities in the active layer, considering your observed variability in water content? How was the GPR data processed? What software was used? Did you use any filters on the data, or time-zero correction? Finally, considering the inherent uncertainties in GPR results and the fact that you have observations of active layer thickness from probing, what do these GPR measurements add to your

studies? If you get the same information but with higher accuracy from probing, I would recommend excluding the GPR data from the manuscript.

Reply:

The reviewer is right in pointing out that much information is missing on the GPR measurements. We will improve on this in the revised version.

Page 7, L8 What was the area of the grid?

Reply:

The top grid had the dimension 3m x 3m (9m2) and the middle one 3.5m x 2.5m (8.75m2). This will be mentioned in the revised version of the manuscript.

Page 7, L14 - Page 8, L15 Please add a description of your model experimental setup, including information about the mesh, boundary conditions, initial conditions, hydraulic properties, simulated time, time stepping. Most of this information appears in the results section, but it should be here instead. Figure 4 The organic layer thickness line varies also in places where there were no soil samples, but should be an interpolation between soil sample points (based on organic carbon content). Can you explain this? I can find no mention that the active layer thickness was measured, in the methods section. I assume this was done manually with a probe (?). Please specify this in the methods section. The active layer also seems to be thicker on August 4th than on August 21st, which seems odd.

Reply:

We will move the details on the numerical modeling to section 3 as suggested.

The interpolation of the organic layer thickness was done using kriging. In order to facilitate this, additional points were specified at the top and at the bottom of the domain representing maximum and minimum values for the carbon content, respectively. This led to variation of the organic thickness line in places with no measurements. We will

describe the procedure in more detail in the revised version.

The thickness of the active layer thickness was measured with a probe. This will be mentioned in the methods section.

As described on p. 11, L8-11 changes in the active layer thickness on the average is minor (3.5 cm). As also noted backfreezing has started between these two days in the depressions of the permafrost.

Page 9, L18 What is the uncertainty around this estimate? From just looking at figure 6, it seems that you could have a rather large range of resistivity values at the bottom of the active layer. You could perhaps provide a standard deviation as well as the 1000 Ohmm value. Do you expect the permafrost boundary to be at the same resistivity value also deeper in the ground, based on what you know about the ground substrate? What do you base this assumption on?

Reply:

We do expect that the permafrost boundary will be at the same resistivity value at depth since the underground settings are the same, which are unsorted moraine throughout the depth.

Page 9, L19 - When I look at figure A2 (the GPR image) I don't see any interpretation of the permafrost table that is based on the actual image, and I therefore cannot see how this image supports the frost probing depths. The frost probing seems to be the better data set here, and I cannot see how you need the GPR at all for what you do.

Reply:

This is a flaw in the figure. We will include the permafrost table in the revised version of the manuscript for reference.

Although the interpreted depth from the GPR is not directly used in the setup of the model it demonstrates the spatial variability of the settings and thus the complexity of

simulating water flow and transport in a permafrost affected hillslope. We consider the GPR data as supplementary data to the stick probing.

Page 10, L1 Does the active layer probing support this? Again, without any constraint on the uncertainty you cannot draw much conclusion here – but if you do some analysis of the uncertainty in ERT results you could actually say something about this potential talik.

Reply:

The active layer probing at that point showed non-frozen ground to a depth of 150 cm. Water samples from that location from a depth of 150 cm show a different isotopic signal than more surface near samples, which support the existence of a connection to a sub-permafrost aquifer.

Page 10, L2 How do observations of palsas and a lake suggest that there is a talik at the top (bottom?) of the studied moraine?

Reply:

The formation of palsa and pingos requires the presence of water (Woo, 2012). Especially pingos are often related to upwelling groundwater in permafrost areas, a phenomenon, which is termed 'pingo spring' (Gurney, 2000). Considering these geomorphological features, the size of the lake and that the permafrost in the area is classified as being discontinuous, we conclude that the lake provides thermal insulation, leading to the presence of a talik in the area.

Gurney, S. D. (2000). Relict Cryogenic Mounds in the UK as Evidence of Climate Change. In S. J. McLaren & D. R. Kniveton (Eds.), Linking Climate Change to Land Surface Change (pp. 209-229). Dordrecht: Springer Netherlands.

Woo, M.-K. (2012). Permafrost hydrology: Springer.

Page 10, L7 Where is higher, in relation to the talik (or taliks?) that is mentioned earlier

in the paragraph?

Reply:

Here we mean 'more uphill'. We will change this term in the next version of the manuscript.

Page10, L7-11 What do you actually know about the sub-permafrost aquifer? If this is all based on the ERT data, then you have a much stronger case if you do an uncertainty analysis on those results. However, this study is interesting enough if it focuses on only active layer transport. That could make a more focused paper and reduce some of the uncertainty that stem from assumptions based on geophysical measurements.

Reply:

We agree with the referee, that a uncertainty analysis could be beneficial and we will consider this, but as stated by the referee the focus of the study is on the active layer. We consider inclusion of sub-permafrost aquifer to be important for achieving good results for the active layer, however, the detailed shape and depth of the sub-permafrost aquifer is secondary in this regard.

Page 10 L21 Which location is this velocity for?

Reply:

The velocity of 170 cm/d refers to the middle location, directly at the lowest location of the active layer, observed for a short period of time.

Page 11 This is text that should go in the methods section. Figure 6 Is not a good figure for showing the model domain, as the actual domain is difficult to see behind the ERT model.

Reply:

We will move the numerical details to the methods section.

We will improve Figure 6 such that the model domain appears more clearly and we will consider splitting the figure in two separate figures.

Page 11, L1-3 I wonder if you need the sub-permafrost aquifer in your model. What are the important information that you gain from including it? I believe you need to motivate better why it should be included in the model. It would be very cool if you could say something about exchange between supra- and sub-permafrost aquifers with your modeling.

Reply:

Although the focus lies clearly on the dynamics in the active layer, the sub-permafrost is important to take into account when modeling the field site. Through the open talik at the top of the field site, water coming from the uphill active layer may enter the talik or remain in the active layer, depending on pressure in the sub-permafrost aquifer. Observations in the piezometers confirm the concept of this exchange. We will try to quantify this exchange between active layer and open talik based on the modeling in the revised version of the paper, to emphasize the necessity of the inclusion of the sub-permafrost aquifer.

Page 11, L15 What does it mean that the mesh ranges from 0.1 to 2 m? Is this a resolution of the mesh? Please specify this more clearly. Equation 8 Explain this equation in the text.

Reply:

An irregular mesh was defined for the model domain with the node distance ranging from 0.1 m for the active layer to about 2 m for the sub-permafrost aquifer.

Page 11, L22 Which soil moisture sensors were used for the calibration? Maybe I misunderstand something here, but you have sensors west and east of the transect which show rather different values for water content. Did you calibrate against a mean for those sensors?

Reply:

Where available the mean of the east and westwards located sensors was used as target for the calibration. In lack of other measures, the water content at the sensors (i.e. offset to the transect) were used as confidence intervals.

Page 12, L11-13 Just a thought: Is RMS the best or only meaningful measure here? Would it be helpful to include something like the Nash-Sutcliffe model efficiency to evaluate the model fit? Just something to think about.

Reply:

We will consider other measures such as the Nash-Sutcliffe coefficient for evaluating of the model.

Page 12, L27 – Page 13, L7 This would fit better in the methods section.

Reply:

Point taken.

Page 13, L9 “arrival correct times” should be “correct arrival times”?

Reply:

Will do. Page 13, L15 – L23 As the results did not show very strong evidence (or focus) on the distribution of taliks, this does not feel like the most motivated issue to start the discussion section with. Connection and exchange between sub- and supra-permafrost taliks are very interesting research topics, but, as I read this manuscript, this study focuses more on the active layer. I suggest to start this section with a discussion on that.

Reply:

Point taken.

Page 15, L15 – Page 16, L13 The Conclusions section could be streamlined by moving

some parts to the Discussions section. For example, the discussion on nitrate is more in depth here than in the Discussion section. I suggest you look through this section and make sure to move new lines of discussion to the Discussion section.

Reply:

We will make sure that discussion lines are in the discussion section and not in the conclusion section.

Finally, for inspiration - A couple of studies that I didn't see in your reference list but that might be of interest to You, came to my mind when I read Your manuscript: Johansson, E., S. Berglund, T. Lindborg, J. Petrone, D. van As, L.-G. Gustafsson, J.- O. Näslund, and H. Laudon, 2015, Hydrological and meteorological investigations in a periglacial lake catchment near Kangerlussuaq, west Greenland – presentation of a new multi-parameter data set, Earth Syst. Sci. Data, 7, 93-108, doi:10.5194/essd-7- 93-2015 Bosson, E., Sabel, U., Gustafsson, L. G., Sassner, M., and Destouni, G.: Influences of shifts in climate, landscape, and permafrost on terrestrial hydrology, J. Geophys. Res., 117, D05120, doi:10.1029/2011JD016429, 2012.

Reply:

We will take advantage of the above studies when revising the manuscript.

Technical corrections Throughout the text the phrasing "active layer depth" occurs. As the active layer is a layer, it has a thickness rather than a depth. I suggest you change the wording from depth to thickness throughout the manuscript.

Page 2, L14 "a complicated flow networks"

Page 9, L23 "This reflection is found a greater depth. . ."

Page 14, L15 Perhaps both of the two occurring "generic" are not needed in this sentence?

Reply:

We will adapt the technical corrections in the revised version.

We very much appreciate the insightful comments from the reviewer, which will be extremely useful for the revision of the manuscript.

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