The Cryosphere Discuss.,
https://doi.org/10.5194/tc-2017-97-RC2, 2017

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.

Anonymous Referee #2

Received and published: 15 July 2017

This paper presents 1) extensive data on a particular permafrost site, 2) performs a tracer experiment, and 3) builds a 2D model to simulate transport of a conservative tracer by calibrating to volumetric soil water content. The central focus of this work attempts to describe the control of advective flow in the active layer just above the permafrost. This is largely motivated by the need to understand nutrient transport and vegetation response. In all, there seems to be a very impressive amount of work here and I applaud the effort to describe all that work! Unfortunately it is not presented in an organized fashion, and connection between the measured field data, simulated tracer experiment, and nitrogen transport is not well described. The unorganized presentation style makes it very hard to understand the work, the assumptions used, and what

the central findings are, as well as how the details presented support the conclusions of the work. While I believe these subjects to be of high value to the cryosphere community, this manuscript is not suitable for publication in its current form. I recommend major revisions, and would encourage the authors to focus on what the measured field data and accompanying modeling are actually quantifying, rather then attempt to make conclusions regarding the fate of nitrogen and plant response. I see opportunity for scientific contribution if the authors focus on how these environments affect the transport of the conservative tracer, which in my opinion is a wide-open topic of cryosphere research. However, the authors must better justify their assumption of a stationary ALT. This is a critical weakness of this work; even if the change of ALT during the simulation is small, it could have a large impact because of the unsaturated conditions result in a smaller water table above the permafrost and lower lateral hydraulic transmissivity relative to a fully saturated ALT. At the very least a discussion on how a temporally variable ALT can affect transport is warranted.

Major Comments: 1) It is a very un-organized manuscript. Model assumptions are made, and the justification for those assumptions are not addressed until much later in the manuscript. Methods appear in the results section and the introduction and abstract are misleading.

2) For example it seems from the introduction, abstract and conclusion that this work intends to address how nitrogen transport may trigger feedbacks on increasing carbon sequestration due to additional plant growth, and to describe using a tracer experiment and model how nitrogen transport happens. Unfortunately, the methods and results do not get at nitrogen transport. Rather it only attempts to simulate a conservative tracer, which I believe could actually be a valuable science contribution unto itself. But the understanding of how the conservative tracer would move through the heterogeneous and dynamically adjustable ALT are not well described, and it is unclear how these processes contribute to the observed and simulated results.

3) Specifically, nitrogen and most nutrients are not a conservative tracer that is applied

to the landscape on a single event as represented in both the field and modeling experiment. Rather it is affected by biogeochemical processes and is continually applied to the system as a result of the nitrogen cycle. No discussion was provided regarding the difference of the experiments and how nutrient transport will behave, which are substantial. However, I would recommend abandoning the discussion and perhaps some of the motivation of nutrient transport altogether because the jump between the conservative tracer experiment and simulation to nutrient transport is too big and lot of necessary process understanding of the conservative tracer transport in these environments is lost in this manuscript. Here, I believe it is vital to fully understand the complexity of transport in the cryosphere before moving on to how biogeochemical reactants behave.

4) The transport of the conservative tracer in the heterogeneous environment is not well described nor is there much recognition of the copious work in hydrology regarding transport in heterogeneous environments, which could suggest a lack of critical understanding regarding all the processes that may affect the observed results. These processes include advective mixing and dilution, multiple breakthrough times from preferential flow ect. (e.g. Rajaram & Gelhar, 1995; Cirpka & Kitandidis 2000; Kung et al., 2000). Or how might highly transient adjective flow affect the tracer spreading, dilution and transport (Goode & Konikow, 1990), or diffusive flow into and out of low permeable areas that cause late time tailing affects (Schumer et al., 2003; Zhang et al., 2007), especially during times of low flow and unsaturated conditions. Here I suggest that the authors address these processes in the interpretation of both the site experiment and simulated results. Clearly, advective flow from snowmelt and precipitation pulses will drive system response, but I imagine preferential flow in the heterogeneous soils to be very important here. As is the transient boundary conditions from precipitation pulses, snow melt and an even moderate deepening in the ALT, that will result in different advective directions at different times. Goode, D. J., & Konikow, L. F. (1990). Apparent dispersion in transient groundwater flow. Water Resources Research, 26(10), 2339-2351. Cirpka, Olaf A., and Peter K. Kitanidis. "Characterization of mixing and dilution

in heterogeneous aquifers by means of local temporal moments." Water Resources Research 36, no. 5 (2000): 1221-1236. Kung, K-JS, T. S. Steenhuis, E. J. Kladivko, T. J. Gish, G. Bubenzer, and C. S. Helling. "Impact of preferential flow on the transport of adsorbing and non-adsorbing tracers." Soil Science Society of America Journal 64, no. 4 (2000): 1290-1296. Rajaram, Harihar, and Lynn W. Gelhar. "PlumeâĂŘScale Dependent Dispersion in Aquifers with a Wide Range of Scales of Heterogeneity." Water Resources Research 31, no. 10 (1995): 2469-2482. Schumer, Rina, David A. Benson, Mark M. Meerschaert, and Boris Baeumer. "Fractal mobile/immobile solute transport." Water Resources Research 39, no. 10 (2003). Zhang, Yong, David A. Benson, and Boris Baeumer. "Predicting the tails of breakthrough curves in regionalâĂŘscale alluvial systems." Groundwater 45, no. 4 (2007): 473-484.

5) While the authors attempt to justify not resolving a moving ALT, how it affects the results is not concisely described, which in my opinion would be a extremely interesting contribution to the current state of the science. This is an especially odd boundary condition to assume because the authors partly motivate this work by citing the importance of variable ALT (see Lines 17-23 on Page 2). Here a small deepening of ALT of 4cm could still have a considerable affect to the results especially considering that ALT is approximately 40cm deep and is not fully saturated, which amounts to a greater then 10% change in subsurface storage of the saturated zone. This assumption probably needs to be tested or better yet the question ‘how much does even a moderate change in ALT affect transport times?’ needs to be answered.

Minor Comments

Page 1, Lines 8-11, This is a bit excessive motivation for an abstract, and may mislead the reader into thinking that the paper is dealing specifically with nitrogen transport in the ALT. Really the paper is about modeling the water flow in the ALT, perhaps it is better to narrow the motivation to what the paper actually discusses.

Page 1, Line 18. What are the units m/a? Meters annually? If so, it probably better to

use meters / year.

Page 1, Lines 20-23: This seems more of a discussion point and not a result of the paper. How specifically does this paper address nitrogen speciation, transport and subsequent vegetation growth. It is not clear that it does, and there for these lines should be removed from the abstract. Also, on line 22, 'climate changes' should be 'climate change.'

Page 2 Lines 7-8: Is it typical for all permafrost sites to have late summer precipitation or just the site investigated here? I would expect variability, and that some permafrost sites do not get late summer precipitation. You may need to specify that this only pertains to your site, and therefore this statement would be better served in the site description section rather then the introduction.

Page 2 Lines 13-14: While I agree that microtopography creates complicated flow networks that affects how lateral flow occur, unfortunately Atchley et al., 2015' is not an appropriate citation for this statement as the modeling in that paper was all 1D vertically, and the inclusion of any lateral flow discussion in that paper was only offered as a possible reason calibrations were not always successful. Painter et al., 2016, demonstrates 2 and 3D flow in polygons would be a good alternative citation as would Helbig et al., 2012, which is a more observationally based conceptualization of lateral flow and microtopography.

Painter, Scott L., Ethan T. Coon, Adam L. Atchley, Markus Berndt, Rao Garimella, J. David Moulton, Daniil Svyatskiy, and Cathy J. Wilson. "Integrated surface/subsurface permafrost thermal hydrology: Model formulation and proofâĂŘofâĂŘconcept simulations." Water Resources Research 52, no. 8 (2016): 6062-6077.

Helbig, Manuel, Julia Boike, Moritz Langer, Peter Schreiber, Benjamin RK Runkle, and Lars Kutzbach. "Spatial and seasonal variability of polygonal tundra water balance: Lena River Delta, northern Siberia (Russia)." Hydrogeology Journal 21, no. 1 (2013): 133-147.

Page 2, Lines 14-16: It is true that models capable of simulating freeze thaw in the unsaturated zone is a relatively new development, but there are now several examples of models capable of simulating this. For example ATS - Painter et al., 2016, GEOtop - Endrizzi et al., 2014, JULES – Chadburn et al., 2015, Pflowtran – Karra et al., 2014. In fact I think SUTRA (used here) is also capable of simulating freeze thaw (Shemin et al., 2011). So the question I have is how does this work further that development, especially since freeze thaw dynamics are not simulated in this work?

Endrizzi, S., Gruber, S., Dall'Amico, M., and Rigon, R.: GEOtop 2.0: simulating the combined energy and water balance at and below the land surface accounting for soil freezing, snow cover and terrain effects, Geosci. Model Dev., 7, 2831–2857, doi:10.5194/gmd-7-2831-2014, 2014. Chadburn, S., E. Burke, R. Essery, J. Boike, M. Langer, M. Heikenfeld, P. Cox, and P. Friedlingstein. "An improved representation of physical permafrost dynamics in the JULES land-surface model." Geoscientific Model Development 8, no. 5 (2015): 1493-1508.

Painter, S. L. and Karra, S.: Constitutive model for unfrozen water content in subfreezing unsaturated soils, Vadose Zone J., 13, 4, doi:10.2136/vzj2013.04.0071, 2014.âĂĺ Shemin, McKenzie J, Clifford Voss, and Qingbai Wu. "Exchange of groundwater and surfaceâĂŘwater mediated by permafrost response to seasonal and long term air temperature variation." Geophysical Research Letters 38, no. 14 (2011).

Page 2, Lines 21-22: "The deepening of the active layer will change the flow and transport pattern and typically lead to longer travel paths and times (Frey and McClelland, 2009;Frampton and Destouni, 2015)." This statement and particularly the work of Framption and Destouni 2015, is why I think transport in permafrost and the dynamic ALT is so interesting. The freeze thaw cycles and the deepening of the ALT create additional complexity into transport systems that are not fully understood. Given that freeze thaw cycles are not simulated nor is the deepening and closing the ALT in this work, how does the work presented here contribute to understanding these complexities? From the methods, results, and discussion sections I cannot discern the contribution of

this work with regards to a dynamic impermeable zone, like permafrost. Page 3 Lines 18-20. It seems that that a goal of this study is to use a conservative tracer as a proxy for nutrient transport, specifically nitrogen. However, most nutrients are affected by biogeochemical process. There should be a discussion regarding the difference between a conservative tracer and a constituent that is affected by biogeochemical processes.

Page 4, Line 11: Replace 'retrieving' with 'retreating'

Page 4, Line 18: Replace 'activ' with 'active'

Page 7, Line 7: "To capture the two-dimensional spreading of the tracer", I suspect you mean 2D in the horizontal spreading rather then 2D in the vertical? Perhaps rephrase.

Page 7, Lines 16-18: If the transport is fast enough, then the assumption of a constant ALT and no thawing and refreezing holds. But what is the length of the simulation? In the above paragraph it states that a tracer was applied and observations were taken over the length of 21days. Was the modeling the same length and did the ALT change much over that time? Figure 7, shows both observed and simulated data beyond September 14th , so more then 40days? The length of observational data and especially the simulation need to be explicitly stated.

Page 8, Line 1: specify that αVN and nVN Are van Genucthan parameters.

Page 8 Line 18, All parameters where depth related? Do you mean where a function of depth?

Section 4.1: Soil parameters. Why not summarize this data in a table?

Figure 3: It would be my preference to summarize this data in a table rather then figure. But just a suggestion. Also, why is the plot Dry Density / Hydraulic conductivity?

Page 11, Lines 8-17: Here is the explanation of why thawing and freezing was not simulated, seems out of place. Consider reorganizing your observations and model configuration to be closer to Page 7, Lines 16-18.

Page 11, Lines 15-21: This seems like it should be in the methods section.

Figure 4: The two sets of gray lines are hard to read, could a different color be used? This is somewhat important as the difference, or lack there of between the two lines are perhaps used is justification of not modeling freeze thaw processes.

Page 11-13: Results largely cover calibrating to volumetric water content. Yes calibrating to volumetric water content was mentioned in the abstract but judging from the introduction this is not exactly what I thought the paper would be about. I thought it would be more about simulating transport. Only Page 12 lines 27 to Page 13 line 12 actually take about validating the simulated tracer break through to observed tracer. From the abstract and intro, I thought the paper would be more about tracer transport. And yes I see why calibrating to volumetric soil moisture content would help get a model that produced correct-ish transport times, but shouldn't the model be constrained by hydraulic head ore pressure? As it is that pressure gradients and hydraulic conductivity is most going to affect advective transport.

Section 4.5. This section is mostly devoted to model calibration. A lot can be learned from model calibration to observed results and so this could be very useful information and help shape the conceptual model. So I was happy to see this discussion. However in addition to the above comment about better introducing the calibration process, I think this section also needs to be re-organized. I would suggest linking the volumetric soil water content calibration to the tracer experiment in some way. Also, shortcomings were identified on page 12 lines 15-29. How might understanding these short comings change your conceptual or numerical model?

Page 12 Line 20: Specify what flux your are referring to in, "...indicate that the flux in..."

Page 14 Line 8-10: Awkward phrasing. How about "Rapid flow is only possible only if recharge exceeds a certain threshold allowing for built-up of a saturated zone at the base of the active layer that interconnects the local depressions.

Section 5.2 Solute transport in the Active Layer: This section should be the focus of the work, but seems to lack rigor, perhaps due to the unfocused writing. A am not sure what the take home message is hear. Is it that precipitation causes increases in advectve flow? That seems pretty obvious, but what about all the other complexities of transport in heterogeneous media? At this point there is very little mention of preferential flow, and virtually no mention of how the combined affects of transient adjective flow, diffusive flow into low permeable areas; especially during times of low flow, and preferential flow paths can affect results like this. I would encourage the authors to concisely describe the processes at work here and explicitly relate them to the observed and simulated results.

Page 14 Line 20: "...this effect..." Precisely what effect is being referred to here? Several ideas/process are being described here and it is very hard to follow what effect is being discussed. The writing is in need of clarification and conciseness. Given that this is a complicated subject it is critical that the writing is clear.

Page 14 Line 22-23: "The velocity by which the active layer thaws controls thereby where the rapid movement occurs:" Very awkwardly phrased, I am not at all sure what is said here.

Page 15, Line 4-5: Again, Atchley et al., 2015 is not a suitable citation here.

Page 16, Lines 1-4: This discussion is out of place in the conclusions, but may fit in the discussion section. However, I would rethink the whole nitrate motivation and discussion altogether. While I agree that transport of nutrients is important, and understanding advective flow through the ALT will go a long way in describing nutrient transport, this manuscript shows very little work that can be applied directly to the transport of nitrogen. 1) nitrogen is not a conservative tracer and is affected by biogeochemical processes. 2) Nitrogen release is not in a pulse fashion that happens in the same manor a tracer test occurs. Rather its release into the subsurface will happen at varying levels over the entire growing/decomposing season.

Page 16, Lines 7-9: "This study confirms that a complex topography with ridges and depressions leads to lateral flow and retention in small depressions until an increase of the water table allows for overspill of the tracer." I think this statement is more aligned with this work then the nitrogen transport motivation. However, the paper as written doesn't clearly show this result, because the writing is often distracting from what I thought or hoped the central work was, advective flow in the ALT.

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