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Hydrology and Earth System Sciences Discussions

Interactive comment on "Shallow soil moisture – ground thaw interactions and controls – Part 1: Spatiotemporal patterns and correlations over a subarctic landscape" *by* X. J. Guan et al.

Anonymous Referee #2

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The paper by Guan et al. is well written and of broader interest to the HESS community. There are very few studies of this nature in such remote environments, and some of the conclusions of the authors have the potential to reach beyond the locality of their research. Much of the discussion of the importance of this work, or its implication, is saved for paper 2. However, while paper 2 certainly 'stands along', I also believe that it is on much weaker footing than this paper. I suggest that the authors consider placing some of this material in the broader context here and bring in some of the mechanistic discussion from Paper 2 into this manuscript.

Specific Comments:

p34L12. This is an awkward sentence, please consider rewording.

More information on the TDR proves and the method of obtaining soil moisture is needed. For example, how long are the probes? How good was the calibration? Was it the same for dry and wet soils and among the sites? We have found that in organic-rich material, TDR can be calibrated well at low moisture contents, but becomes very poor at higher moisture contents.

A large uncertainty I have is in how the relation between soil moisture and thaw changes as thaw depth changes. I assume that soil moisture is obtained by vertically intreating the TDR (shallow soil moisture). As the thaw depth increases, the relation between this shallow soil moisture and total soil moisture in the thawed zone will become more and more divergent. Were any tests taken to relate shallow soil moisture to total water in the thawed zone? I can imagine a situation where organic soils at the surface are quite dry, but at depth, soils are wet. Of course, soil thermal admittance is largely governed by conditions at the surface, but heat transfer and consumption within the profile is very much dependent of the properties within the profile. This goes some way in explaining the reduction in correlation with time and introduces a significant experimental bias. In essence, what you are measuring is changes throughout the experiment due to the nature of the thaw. I would like the authors to consider this and include their thoughts in an expanded discussion.

P41L21. "appeared to be organized". I've looked at the figures of both soil moisture and thaw depths, and perhaps I don't see the same level of organization as the authors. There are many geostatistical metrics of similarity and organization that this dataset would be suitable for. I suggest that if the authors use this type of language, they consider assessing statistical measures of organization. Perhaps that is for Paper 3! However, I do think that there is room in this manuscript for organizational metrics, which will aid in the discussion, particularly with respect to the T3 discussion.

P45L14. The authors state that, contrary to other studies in polar environments, they did not see a relationship that show inverse relationship between soil ice content and depth. The explanation that surface flow is responsible for this accelerated thaw, essentially by providing 'heat'. I would argue that the authors are confusing the situation from the relatively simple thermodynamic assertion that greater ice volumes are harder to thaw due to latent heat. There are hundreds of permafrost scientists who would be puzzled by assertions that there is not a direct relation between ice content and and shallow thaw. The fact that running water and advection of energy provides more heat does not make this previous assertion wrong, but you are simply studying something different, and doing an 'apples vs. oranges' comparison.

P46L9 "was hysteric and complex". Is hysteric the correct word to use here? You are not examining freeze-back, which I assume would be the other side of the hysteric curve, or am I missing something?

P46L19 "... depth depleted further..." I'm not sure 'depleted' is the best word here. Consider revising.

The T3 concept, while interesting, is a little bit out of place with the data presented in the manuscript. The T3 concept is (I believe) largely useful to explain larger-scale responses of catchments and to explain their organization in a comparative framework. No flow or hydrological data is presented in this manuscript, and it is difficult to assess how different 'first order' controls are related to hydrology, as there is very little in the way of hydrological data beyond the soil moisture and thaw data presented here. I would consider not using the T3 concept explicitly as a section heading, but referring to it where appropriate in a more expanded discussion.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 7, 33, 2010.