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Interactive comment on "Determining the terrain characteristics related to the surface expression of subsurface water pressurization in permafrost landscapes using susceptibility modelling" *by* Jean E. Holloway et al.

Anonymous Referee #3

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Determining the terrain characteristics related to the surface expression of subsurface water pressurization in permafrost landscapes using susceptibility modelling

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General comments

In their paper, the authors evaluate the susceptibility of High-Arctic permafrost terrain to disturbances (ALD and ME) related to high pore water pressure. To do so, they used a GIS-based approach, statistics, and field validation, and made the demonstration that such an approach is exportable to other sites. The results indicate that terrain

characteristics of ALDs and MEs differed in the modelled high susceptibility zones, whereas they were similar in low susceptibility zones. They have shown that slope was the main variable driving ALD initiation and distance to water was the most important variable explaining ME formation.

Although this paper makes an interesting contribution to permafrost landscape hazards and permafrost landscape dynamics studies, I think it would be better suited for a GIS-dedicated journal or a hazards-dedicated journal. Indeed, my impression is that the cryospheric components in this article are not developed sufficiently to justify a publication in Cryosphere. If the editor decides differently, then the authors should develop a section on ground ice and particularly clarify the concept of 'transient layer' and how it applies at the landscape scale, how to model it and how to incorporate it in their GIS-based approach. A point should be made about the distribution of ground ice in a given watershed and along topo-sequences. Unfortunately, it is mentioned in the paper that ground ice was not specifically taken into account in the analysis due to a lack of data about this aspect.

Some of the results of the modelling makes a lot of sense although other are very surprising. I think the authors should explain better the 'correlations' they obtained. In particular, I would like to see more explanations on the 1) PISR: the peak for ME and the fact that the probability decreases as PISR increase for ALD (is this a ground ice effect? Less PISR, ice closer to the surface?), 2) distance to water (probability decreases and then increases with distance to water for ME and ALD), 3) TWI for ME: probability increases and decreases with rise of TWI.

Again, I would like to stress that I consider the quality of this paper to be good to very good but that the authors would benefit in terms of dissemination and citations to publish it in a different journal with a better-targeted readership.

Note to authors and editors:

âĂć English is not my first language. I provided suggestions to improve the sense of some sentences, however, I realize that some of them might not be appropriate given my level of English. I therefore leave to the authors some latitude regarding the request for change concerning the English and understand that some of the requested changes won't be implemented. âĂć 'Soil' is used in its geotechnical sense (unconsolidated sediment) and not in terms of pedology.

Specific comments:

Abstract, L12 : The link between high pore water pressure and landscape degradation isn't clear. I understand it but it is implicit in the text. The authors should clarify this in the abstract and later in the text. Perhaps by stressing which geomorphological processes can be triggered by high pore water pressure, how high PWP are generated and how these geomorphological processes can have an impact on landscape evolution, landforms, or, to a different scale, active-layer/surface dynamics.

Abstract, L17-18: 'distance to water' repeated in the same sentence. Correct please.

Abstract, L20: delete 'accurately'. Let the reader judge if this was indeed 'accurately modelled'.

Abstract, L22-23: the authors use the term 'relatively' (. . .low PISR, . . .far from water). I propose to eliminate relatively and suggest to change to something like 'lowest PISR' or simply 'low PISR' and 'far from water' or 'farthest away from water'.

Abstract, L23: '. . . areas that may be sensitive to high PWPs'. This sentence weakens the abstract. I think it is reasonable to say: '. . .areas sensitive to high PWPs' without 'that may be. . .'.

Introduction, L30: delete 'seasonal'. The active layer is a seasonal phenomenon.

Introduction, L31: 'water and ice enrichment at the base of the active layer'. I believe the authors should add 'and in the upper part of permafrost'.

p. 2, L3: 'during the summer months'. This should be either deleted or 'beginning of

winter' be added. Indeed, the bottom of the active layer often thaws as the top of the active layer is refreezing.

p. 2, L4-5: 'During the fall freeze-back period this water undergoes refreezing, consequently developing an ice-rich transient layer at the base of the active layer (Hinkel et al., 2001; Kokelj and Burn, 2003, Shur et al., 2005).'

The transient layer is not explained properly here. The authors have to explain that this water refreezes and remains in the 'permafrost portion' of the soil column during cold year (s) whereas during warm years the transient layer thaws partially, that is the active layer deepens (thawing of the active layer and upper portion of permafrost). The following two years (or more), depending if these years are colder or warmer than the previous ones, the active layer will continue to deepen or the lower portion of it will not thaw and then will be part of the upper permafrost. The authors should re-write the text around the concept of transient layer.

p. 2, L7-8: 'This addition of moisture, as well as infiltration from late season precipitation, results in high pore-water pressures (PWP) at the base of the active layer'. This is the case for saturated (porosity filled with ice and, upon melt, with water) fine-grained soils essentially. Unsaturated sediment will not develop high pore water pressure upon thaw and coarse sediment will usually drain and won't develop high pore water pressure. Please specify. The reference cited could be improved, perhaps cite specific studies concerning pore water pressure in permafrost environment or classic geotechnical literature about PWP and mass movements.

p. 3, L20-22: 'The site is underlain by Devonian sandstone and siltstone bedrock comprising the Weatherall, 20 Hecla Bay, Beverley Inlet and Parry Islands (Burnett Point Member) formations (Harrison, 1995), but outcrops are uncommon'. I suggest to change for: 'The site is underlain by sandstone and siltstone bedrock but outcrops are uncommon (Harrison, 1995).'

p. 4, L24-25: the reason why distance to water (10 m) and distance to ALD (20 m) needs to be explained. 10 m from water appears close to me for the topic and scale of the study.

p. 5, L4-5: the reason why large spatial clusters of ME were removed from the analysis needs to be explained. It could indeed be interesting to see these large clusters.

p. 6, L5-6: what is the scale of the surficial deposit map used? Could this map along with the marine limit elevation be used to infer, although very generally, the potential distribution of ground ice, given the general relation between grain-size distribution, frost-susceptibility and ground ice? The lack of data on ground ice is, in my view, one of the main weakness of that paper.

p. 6, L10-11: 'While ground ice content is linked to high PWPs, it is not used as an input variable as ground ice maps were unavailable and impractical to attain'. The authors mentioned that ground ice is more abundant below the marine limit (p. 5, L17-19). Was there a factor/weight added to the cells below the marine limit as PWP is more likely to be generated in areas with high ground ice content? Please describe surficial sediment/(cryo) stratigraphy above and below the marine limit. Models indicated 50 and 80 m as key elevations. What's going on around these elevations that could help understand the output of the model better?

p. 11, L20: 'Landscapes composed of fine-grained surficial sediments are susceptible to a wide range of permafrost degradation processes, including the development of high PWP in the active layer'. The development of high PWP is not a permafrost degradation process. High PWP and excess PWP lead to the development of mass movement and this could be included as a 'permafrost degradation process'. Please change.

p. 11, L25-26: 'While soil PWP measurements are not available to confirm pressurization in these instances, the inferred mechanism is diapirisation of sediment slurries from the base of the active layer caused by pore-water pressurization due to ice thaw' Diapirism of sediment slurries can be from the base of the active layer or from lateral

mass movement originating from upslope (there will be mass transfer, at least water, even with low angle slope). I also agree that is it probably more related to the base of the active layer, however the authors haven't shown data to support it. Furthermore, the liquid limit threshold can be attained due to water release upon ice thaw but it can also be attained by the infiltration of rain in the active layer or from subsurface flow. This should be mentioned.

p. 12, L11-13: 'Hence, while surficial materials are broadly similar across CBAWO, the landscape zonation of these two features appears to follow a slope continuum.' I agree and I think the authors should expand their explanation here. Please put this sentence in the context of High-Arctic polar desert watershed/toposequence so that readers could verify if these observations apply in other similar landscape settings. Clarify the link between toposequence, hydrology, moisture and the thermal regime of the active layer.

p. 12, L15-17: 'In 2007, the warmest year since regional records began in 1949, deep active layer development and late July rainfall triggered widespread ALD formation.' I would like to have more information about the effect of rainfall on ALD. There's not enough information about it in the paper, even though it could be an important factor. If rainfall data are available, they should be included in the results and discussed later in the paper.

p. 12, L20-22: 'Similar conditions were observed with MEs associated with terminated active layer fractures in 2012 further suggesting the presence of fluid slurries in situations approaching those that generate ALDs.' ...' These observations suggest that MEs, while clearly reflecting evidence for subsurface soil water pressurization also likely play a stabilization role through pressure release to the surface.' 'By contrast, ALDs are associated with sufficient pressurization to induce slope fracturing and downslope movement.' Are the authors suggesting that ME reduced the PWP and reduced therefore the occurrence of ALD? Please make it clear. Is it possible that ME occurred at the location of ALD prior to the slide? I would like the authors to provide their interpretation/opinion about this point. This can form interesting working hypotheses for future studies.

p. 14, L23-25: ‘The susceptibility models demonstrate that ALDs are most probable on hillslopes with gradual to steep slopes and relatively low PISR, whereas MEs are associated with higher elevation areas, low slope angles and in areas relatively far from water (drier)’. I suggest to add concave slope for ALD and convex slope for ME.

Format:

- For all the text: add space between number and unit. Ex: 100 m.

- In the pdf version, at several places, space is missing between words, punctuation, units, etc.

- Figure 1: add scale (1a, d), add complete date (a, b, c, d)

- Figure 6: add scale and complete date. Is the ALD visible in the background or are they more MEs? Please clarify in the figure caption or directly in the figure.

- Table 2. It would be interesting to add some basic statistics to this table. The table provides mean values of terrain variables. Please add the range, the median and the standard deviation for these variables. It would be very useful if one’s want to compare this study with other studies conducted in similar/different environmental set-ups.

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