The Cryosphere Discussions

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Comment on tc-2020-351

Anonymous Referee #2

Referee comment on "New insights into the drainage of inundated ice-wedge polygons using fundamental hydrologic principles" by Dylan R. Harp et al., The Cryosphere Discuss., https://doi.org/10.5194/tc-2020-351-RC2, 2021

The manuscript “New insights into the drainage of inundated ice-wedge polygons using fundamental hydrologic principles” presents evaluations of tundra polygon drainage characteristics with a simple analytical model. While I like the idea of simplified modelling to evaluate the properties of the polygon hydrological system, the key findings seem to be identical or at least close to the already published study by Zlotnik et al. (2020), even if the quantitative analysis is different. If the authors maintain that the manuscript contains novel research, they need to explain the relationship between the two studies much better. If the qualitative conclusions are indeed largely the same and the main novelty of this work is additional quantitative scenarios and model evaluation, the authors need to consider and discuss the model limitations in much more detail. From my limited understanding of the model, I have the impression that it cannot describe many relevant real-world situations, at least not quantitatively. In conclusion, the authors need to carefully argue what in their study is novel at a level that would warrant publication in TC.

Major Comments:
1. The central conclusions of the manuscript, as stated in the Abstract, appear to be largely identical with the ones in Zlotnik et al. (2020).
This manuscript (Abstract): “One of the primary insights from the model is that most inundated ice-wedge polygon drainage occurs along an annular region of the polygon center near the rims. This implies that inundated polygons are most intensely flushed by drainage in an annular region along their horizontal periphery, with implications for transport of nutrients (such as dissolved organic carbon) and advection of heat towards ice-wedge tops.”
In plain language: Drainage and flushing of the center is concentrated to the area adjacent to the rim. This (qualitative statement only) affects water-mediated transport.
Zlotnik et al., 2020 (Conclusions): “only a small fraction of the polygon volume near the rim area is flushed by the drainage at relatively high velocities, suggesting that nearly all advective transport of solutes, heat, and soil particles is confined to this zone.”
In plain language: Drainage and flushing of the center is concentrated to the area adjacent to the rim. This (qualitative statement only) affects water-mediated transport.

This manuscript (Abstract): "The model indicates that polygons with large aspect ratios and high anisotropy will have the most distributed drainage. Polygons with large aspect ratios and low anisotropy will have their drainage most focused near their periphery and will drain most slowly. Polygons with small aspect ratios and high anisotropy will drain most quickly."
In plain language: For a given fixed polygon radius, and for a given fixed vertical hydraulic conductivity: increasing the horizontal conductivity increases drainage, and increasing the thaw depth increases drainage as well. Both also lead to a less focused flow within the center, i.e. flushing by throughflow of water occurs over a larger volume of the center.
Zlotnik et al., 2020 (Conclusions): "Anisotropy in hydraulic conductivity (horizontal-to-vertical hydraulic conductivity ratio) has a secondary influence on the intensity of flushing. Increases of anisotropy values counteract the effects of increased geometrical aspect ratio increases and vice versa."
Zlotnik et al., 2020 (Appendix B): "…an increase in the anisotropy can redistribute the flux over the polygon, thereby reducing the edge effect."
In plain language: For a given fixed polygon radius, and for a given fixed vertical hydraulic conductivity, increasing the horizontal conductivity has the same qualitative effect on drainage as increasing the thaw depth. An increase in horizontal conductivity leads to a less focused flow within the center, i.e. flushing by throughflow of water occurs over a larger volume of the center.
The first conclusion seems to be identical, and the second one is very close, although stated more clearly in this work. Worryingly, the authors do not make an attempt to acknowledge this similarity and to explain the differences between the two studies to the reader. Zlotnik et al. (2020) is only presented briefly as a model description paper, without discussing the relation between the two studies. I can see that the present manuscript contains additional and more quantitative analysis of the model trajectories. However, I have the impression that it is largely an illustration and a more detailed description of the main findings published in Zlotnik et al. (2020).

2. Eq. A3 implicitly states that the absolute elevation of the frost table in the polygon rim is always equal to (or lower than) the thaw depth in the polygon center. Otherwise, there would have to be a condition, that kappa becomes zero (or very small) for z larger than the rim frost table elevation. This means that thaw depths in the centers are assumed significantly smaller than in the rims (due to the higher absolute surface elevation of the rim) in the model. While the authors write of a smaller "hydraulic conductive capacity" of the rims "due to a raised thaw table following the surface topography" (l. 135), this does not simply translate to a smaller kappa. In fact, all flowlines and the entire analysis change if the still frozen part of the polygon rim forms a threshold over which the water must drain. This for example means that the model is not really applicable early in the season, when thaw depths are low and naturally follow the microtopography. The authors need to present field measurements or other evaluations of the seasonal progression of thaw depths and associated microtopography that help evaluate in which situations the results can represent. It is important to know if the model is applicable 90% or only 10% of the time. They should also discuss in much more detail to what extent "general intuitive insights" (l. 105) from the model results can be transferred if the model assumptions are partly violated. Sect. 4.2 is not nearly enough and in my opinion omits the most critical limitations (see also next point).

3. Anisotropy: The authors need to provide a clearer picture how and why anisotropy in hydraulic conductivities exists and what real-world cases different values of anisotropy represent, e.g. Kr/Kz=100. In particular the model representation of horizontal layers with highly different hydraulic conductivities, as it occurs for real-world-polygons, should be

discussed. It looks like the simple model assumes horizontal and vertical hydraulic conductivities to be constant throughout the entire polygon center. This assumption should strongly determine the flowlines and thus the findings, but I am not at all convinced that it is a good representation of a real-world polygon center, where e.g. surface moss layers can have a strongly different hydraulic conductivity than mineral layers below. In the last point of their Conclusions, the authors explicitly describe layers with different hydraulic properties as a reason for the anisotropy, but this is not at all represented by the model (Kr and Kz in Eq. A2 have no depth dependency). Therefore, I do not think that the quantitative analysis is sound if there are layers with different hydraulic conductivities.

4. The manuscript largely uses model-specific terminology which is hard to relate to real-world parameters, e.g. thaw depth and polygon diameters, in an intuitive way. It would make the manuscript more readable if the authors reword some of the statements to more plain language (see above for examples).

Minor comments:
L. 82: How about the case that the thaw depth in the polygon rims is above the ground surface of the center, i.e. within the vertical interval of the pond? From my understanding, this situation is not represented by the model? In reality, there should be only negligible flow through the soil in the center. This could be an important situation early in summer.
L. 97: but that also implies a depth dependence of anisotropy, which does not seem to be accounted for in the model. See major comments.
L. 105: I have the impression that the limitations of the model are quite severe (see major comments), so the "general intuitive insights" might not be applicable for many relevant cases. It is important to discuss and present this in more detail.
L. 185: Is such a high range for the anisotropy reasonable (what kind of material would the outer limits correspond to)? See major comment on the layering.
L. 190/Fig. 6: See major comment on rim hydraulic conductivity and frost table. The seasonal deepening of the frost table in the rim which likely is a major control for drainage from the polygon does not seem to be accounted for in the model. Modeled depletion curves extend over periods of a month and more, for which this thaw progression is highly relevant.
L. 245: the term "ridgeline" could create confusion with "polygon rims".
L. 336: It would be nice to state some of this in more intuitive language, e.g. "for a given thaw depth advective heat transport to the thaw front in the polygon rims is higher for large polygons".