The Cryosphere Discussions

The Cryosphere Discuss., referee comment RC2
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Comment on tc-2022-134

Anonymous Referee #2

Referee comment on "Permafrost saline water and Early to mid-Holocene permafrost aggradation in Svalbard" by Dotan Rotem et al., The Cryosphere Discuss., https://doi.org/10.5194/tc-2022-134-RC2, 2022

Dear Editor and authors,
The manuscript by Rotem et al. investigates permafrost aggradation in the Adventdalen East (ADE) valley in High Arctic Svalbard. Following the last glaciation, the ADE was inundated by a seawater ingression, allowing for the deposition of saline marine sediments. Due to post-glacial rebound, these saline sediments were ultimately exposed to cold sub-aerial conditions, resulting in permafrost formation. Aeolian processes deposited more material on top of the marine sediments, resulting in syngenetic permafrost aggradation. The latter sediments contained fresh porewater. This interesting and useful paper uses a numerical model to quantify frozen permafrost formation rates under varying freezing conditions at the ground surface and sediment properties. For the sediment properties, the authors fine-tune the sediment porosity and freezing point (based on the porewater salinity). To compliment (but not necessarily validate) their thermal models, the authors show ground ice chemistry data from two boreholes down to a depth of ~13 m. The boreholes clearly show a fresh-saline transition in the epigenetic part of the permafrost, as well as evidence for "mixing" in the upper part of the epigenetic permafrost. This is important, because it demonstrates that the groundwater infiltration of freshwater was inhibited by ground freezing very soon after sub-aerial exposure. This point in particularly key to justify that the thermal models could neglect groundwater flow and the flushing of salts. Although I enjoyed reading this paper, I have some concerns about the numerical model. My comments are arranged into "major" and "minor" categories. While I point out a few typos, please note that I did not perform an exhaustive spelling and grammar check. For the next version of this manuscript, I recommend that more attention be given to proofreading. Overall, I classify this revision as "major" and that the paper can be considered for publication after the next iteration of the paper is reviewed.

Major:
1. The time step of 32,600 seconds is not 0.5 days as written in the text. Half a day is 43,200 seconds. Please check the simulations.
2. I think the thermal conductivity is not correctly calculated and this can have a major impact on the results. In equation 2, the dry soil conductivity is used for the mineral fraction of the soil. However, the dry soil thermal conductivity is a bulk value. In this equation for saturated conditions, the "mineral thermal conductivity" should be used and this is typically around 3.0 W/(mK). Therefore, a value of 0.35 W/ (mK) is excessively low.

Some nice examples of mineral conductivities are given in Overduin et al. (2019): https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2018JC014675
The authors should check if similar errors were made for density and heat capacity. Further, there is an extra bracket on the right side of all three equations.
3. Because of the thermal conductivity error, I am skeptical of the permafrost aggradation rates. I am a little bit surprised that changing the porosity has such a small effect on the results, especially because the latent heat associated with such a change is significant. If the authors re-run the simulations with the correct thermal properties, I hypothesize that, the permafrost aggradation rates would be more divergent when considering different porosities.
4. The numerical model does not consider salt diffusion and therefore salts cannot migrate during the advance of the freezing front. While I do not expect the authors to incorporate salt diffusion into their model, I would appreciate some more discussion on this process. As the authors point out, ground freezing results in ionic exclusion, thereby increasing the porewater salt concentration. Consequently, this creates a porewater salt concentration gradient. Since the advance of the freezing front slows with time, the porewater salt concentration can be sufficiently strong at a particular depth to increase the porewater salinity and create a cryopeg or partially frozen conditions. How would the permafrost aggradation rates change if salt transport were included in the model? For coupled heat and salt diffusion models, consider the following paper:
https://doi.org/10.1029/2018JF004823

5. Please add a conceptual diagram of the Ghyben-Hertzberg approximation and include two panels (1 with permafrost and 1 without permafrost). This would really help the reader visualize how the fresh-saline interface is expected to look in unfrozen and frozen environments.
6. If available, could you please include ground temperature data with the geochemical ground ice data in Table 1? At the very least, were in-situ frozen and unfrozen conditions recorded during drilling? Please add this information.
Minor:
Line 13: Should “valley” be capitalized?
Line 45: Consider rephrasing to “below 0 °C”
Line 51: Consider pointing out that permafrost can form in taliks beneath lagoons, as well as beneath bottom-fast ice conditions in shallow water. Consider the following paper: Solomon, S. M., Taylor, A. E., & Stevens, C. W. (2008, June). Nearshore ground temperatures, seasonal ice bonding, and permafrost formation within the bottom-fast ice zone, Mackenzie Delta, NWT. In Proceedings of the Ninth International Conference on Permafrost, Fairbanks, Alaska (Vol. 29, pp. 1675-1680). Fairbanks: Institute of Northern Engineering, University of Alaska Fairbanks.
Line 51: Replace “permafrost usually” with “permafrost is usually”
Line 54: Replace “Barents Sea” with “the Barents Sea”
Line 68: You mention that groundwater flow is practically impossible in continuous permafrost areas. Can you make a few comments about groundwater flow in cryopegs in continuous permafrost and if this is relevant to Svalbard?
Line 93: Replace “Exposed surface” with “The exposed surface.”
Line 95: Replace “Active layer thickness” with “The active layer thickness.”
Line 103: The units for “km” should not be capitalized.
Line 104: Replace “Permafrost section” with “The permafrost section.”
Line 105: Replace “1 to 5.5” with “1.0 to 5.5”
Figure 1: Please improve the resolution and include a higher quality figure.
Line 118: Replace “with serial” with “with a serial.”
Line 140: Replace “afresh” with “a fresh.”
Line 149: Should “Pingo” be capitalized?
Line 153: Please comment on why the high ratio of Ca/Cl and SO4/Cl at a depth of 5.45 m

is enigmatic
Table 1: Please add a row for “standard seawater composition” to help put the results in context.
Line 225: Please be consistent. In the text, water freezing temperature (WFT) is used and in some of the figures (e.g., Figure 3) Tf (freezing point) is used.
Table 2: Careful with the units of thermal conductivity. The units should be W/ (mK).
Table 2: Please use appropriate notations for multipliers and exponents. The table looks a little messy.
Line 320: Please define “winter inflection point.”
Line 320: Replace “freezing front” with “the freezing front.”
Figure 4: Please add labels for panels “a”, “b”, and “c.”
Line 380: Replace “When freezing” with “When the freezing”
Line 383: What do you mean by “low water activity?”
Line 389: For simplicity, why not state the eutectic point of the H2O-NaCl system (-21 °C)?
Line 427: Replace “Less saline” with “The less saline”
Line 428: I suggest replacing “exposure” with “sub-aerial exposure.”
Line 429: Replace “when rebound” with “when the rebound.”
Line 463: Replace “Assuming groundwater” with “Assuming the groundwater.”