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The Cryosphere Discussions

Interactive comment on "Microtopographic control on the ground thermal regime in ice wedge polygons" *by* Charles J. Abolt et al.

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Thank you for your comments on our manuscript! We appreciate your suggestions for improvement, which we believe have increased the clarity of our findings (through edits to the abstract and figures) and helped provide a more complete and succinct summary of where our research fits within previous work on ground temperature and ice wedge cracking (through additional references in the Background and Discussion). Please find below our responses to specific comments, marked with three stars (***). We also include a revised version of the manuscript with changes tracked as a supplement to this comment.

General comments:

The manuscript is well written and addresses an interesting topic of current relevance in the literature. The main conclusions support field evidence, conceptual models, and hypotheses from prior studies. The strength of this paper is that the authors were able to manipulate rim/trough morphology and examine their effect on the ground thermal regime explicitly using a numerical model. Perhaps the most interesting (and novel) conclusion was that varying rim height produced a greater change in minimum ice wedge temperature than varying trough depth. I think this point should be mentioned in the abstract.

I have 2 other general comments, which are further detailed in the next section.

(1) The authors presented two previous hypotheses regarding the formation of secondary ice wedges (Burn and O'Neill 2015 and Dostovalov and Popov 1966). The discussion paragraph describing the influence of microtopography on thermal conditions could be strengthened by relating the results of the modelling to these competing views. This needn't be lengthy and would simply involve minor changes. See below.

(2) The interpretability of the tables and figures could be improved significantly by renaming the field sites, and addressing some minor issues, particularly on Figure 6. See below.

***Thank you for your feedback on our manuscript! As suggested, we have modified the abstract to emphasize our finding that winter ice wedge temperature is more sensitive to rim height than to trough depth. We have also responded to your other two general comments, as described below.

Specific comments:

P. 2 Line 32. Suggest adding reference to recent paper describing temperature and cooling rate conditions during thermal contraction cracking: O'Neill H.B. and Christiansen H.H. Detection of ice wedge cracking in permafrost using miniature accelerometers. https://doi.org/10.1002/2017JF004343 Journal of Geophysical Research: Earth

Surface.

***Thank you. We have added this reference (page 3 line 3 in revision with markup).

P. 4 Lines 23-25. Is the sentence about thermokarst lakes needed? Nothing is mentioned about lakes later, and the thermal effect of lakes within 1 km likely have little to no effect on the temperatures at the depths measured in this paper.

***We agree and have eliminated this sentence (page 4 lines 25-27).

P. 4. Line 32. Rods removed in what month, 2015?

***We now specify September, 2015 (page 5 line 2).

P. 7 line 27. Can you quantify "comparing well"? What is the difference in the maximum snow depths?

***The maximum observed snow depths at Deadhorse and Sagwon were 53 cm and 58 cm respectively, which are slightly higher than our simulated maximum snow depth of 45 cm. We now state this in the text on page 7, line 31.

P. 8. Line 1. I presume the RMSE is calculated on daily temperatures, but can you clarify please.

***We now specify that RMSE is calculated on daily temperatures (page 8 line 4).

P. 8 Line 31. You hypothesized colder rims for your site, but this has been clearly demonstrated before. As this is the discussion section, I suggest referencing past studies that have explicitly addressed this (e.g., Christiansen 2005, Thermal Regime of Ice-wedge Cracking in Adventdalen, Svalbard. PPP DOI: 10.1002/ppp.523).

***Thank you. We have included a citation of Christiansen, 2005 (page 9 line 2).

P. 9 Line 18. I'm not sure if your model program supports it, but the heat loss from the rims could be very nicely illustrated with a closeup of the rim/trough area showing ground heat flux vector arrows. Not necessary but would make a nice addition if easy

to do.

***Thank you for this suggestion. We appreciate the idea and looked into producing this figure, but were unable to do so with the available model output.

P. 9 Line 21. This point about cooling at the wedge due to rim relief was explicitly mentioned in Christiansen (2005, reference above), and is stated in the conclusion: "Effective cooling of the active layer above the side of the ice wedges in the almost always snow-free ramparts permitted the top of the central part of the ice-wedge to attain the critical temperature of -15C. This appears to explain why thermal-contraction cracking is widespread even beneath snow-filled ice wedge troughs." So, to say that it has not been emphasized in previous conceptual models is somewhat inaccurate. In light of this, you may wish to reword parts of the discussion accordingly.

***We agree and have eliminated the sentence beginning with "The high sensitivity of ice wedge temperature to rim height has not been emphasized. . ." We also have included a new paragraph in the Discussion section that better contextualizes our results with a number of field studies, including the paper by Christiansen (page 9 lines 24-33).

P. 10 lines 7-16. "Regarding historical polygon development, the results provide evidence that feedbacks associated with..." I think this paragraph could do with more explicit reference to the two competing arguments by e.g., Burn and O'Neill (2015) and Dostovalov and Popov (1966). You set this up nicely in the background section, but the discussion paragraph would benefit by referencing Burn and O'Neill when you talk about primary wedge deactivation and secondary wedge cracking. E.g., line 10 could be reworded as something like "...the model results support the hypothesis of Burn and O'Neill (2015) that feedbacks associated with microtopographic change..."

***We have reworded the paragraph as suggested. The sentence in the middle of the paragraph now reads "Regarding historical polygon development, the results support the hypothesis of Burn and O'Neill (2015) that feedbacks associated with microtopographic change are sufficient to explain the presence of secondary wedges in modern

and ancient polygons. . .” (page 10 line 22).

Table 1. Typically both the frozen and unfrozen conductivity values are reported. Here you do not specify what you are reporting, but given the values I assume these are unfrozen? I suggest reporting both, as the difference is important. Also, may be worth clarifying what the “dry” thermal conductivity of an ice wedge means?!?

***We have updated the table to include frozen saturated thermal conductivity, which the model estimates as a function of unfrozen saturated thermal conductivity and porosity. In the model input files, we specified 0.02 W m-1 K-1, or approximately the thermal conductivity of air, as the “dry” thermal conductivity of an ice wedge. This number was essentially meaningless, however, as the ice wedge never became less than fully saturated. To avoid confusion, we have replaced this number with ‘N/A’. We thank the reviewer for pointing this out so that we could make this clarification and avoid confusing readers.

Table 2 and 3 (and Figure 1). I suggest renaming all of your instruments. The names a000 mean nothing to the reader, and make it difficult to determine which individual instrument is in the rim or center without looking at the map. I suggest renaming for Center and Rim, respectively. This will make it much easier for the reader.

***Thank you for this suggestion. We agree that renaming the sensors to indicate rim or center location is a far more logical system. We have replaced the old sensor codes with new names throughout the figures, tables, and text.

Figure 3. Suggest adding sentence to remind the reader that the simulation extends to 50 m depth and that only the upper ground is shown.

***We have clarified this point both in Figure 3 and Figure 7.

Figure 6. First, are the legend labels correct? Why are all either obs (a101) or sim (a109)?. Second, near impossible to differentiate the obs and sim. I suggest removing all but the 10 cm and 50 cm plots. We don’t need to see all the inbetweeners, do we?

This will make it possible to see the obs vs. sim.

***We have eliminated the plots of temperature at 20-40 cm depth as suggested, and changed the figure size and formatting to improve clarity. The figure is more focused and clear now. We thank the review for suggesting these modifications that make the figure more informative and interpretable.

Suggested technical corrections:

P. 4 l. 11. “centered on a low-centered” consider revising (word repetition).

***We have changed to “surrounds a low-centered” (page 4 line 13).

P. 8 l. 14. “was sufficient to make cracking favorable”. Better as “was sufficient to favor cracking”?

***We have changed the wording as suggested (page 8 line 17).

P. 9 l. 6 and 7. Suggest deleting “increasing the potential for pref. transmittance of heat”. An increase in conductivity is exactly this, so text after the comma is redundant.

***We have deleted the suggested text (page 9 line 9).

Table 4. Suggest changing “experience” to “with”.

***We have changed this wording.

Please also note the supplement to this comment:
https://www.the-cryosphere-discuss.net/tc-2018-2/tc-2018-2-AC2-supplement.pdf

Interactive comment on The Cryosphere Discuss., https://doi.org/10.5194/tc-2018-2, 2018.