

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

Anonymous Referee #2

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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

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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 re-naming the field sites, and addressing some minor issues, particularly on Figure 6. See 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.

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.

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

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

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

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).

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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.

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.

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..."

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?!?

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 the

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C1 to ... and R1 to ... for Center and Rim, respectively. This will make it much easier for the reader.

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.

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.

Suggested technical corrections

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

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

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.

Table 4. Suggest changing "experience" to "with".

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