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Interactive comment on "PERICLIMv1.0: A model deriving palaeo-air temperatures from thaw depth in past permafrost regions" *by* Tomáš Uxa et al.

Anonymous Referee #2

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PERICLIMv1.0: A model deriving palaeo-air temperatures from thaw depth in past permafrost regions

By Uxa et al.,

Major

The manuscript has a lot of jargon words in the Abstract and the Introduction. This makes it difficult to understand and follow from the beginning. The authors introduce the work from a very general perspective and do not include specific details applicable to the current study. The results section has only one figure with a lot of unnecessary discussion points, which are well-known and well-documented in the previous works. In addition, most if not all the formulas and notations can be found in Nelson and Outcalt

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(1987), listed in references. Note, that Nelson and Outcalt (1987) acknowledge the surface processes and do not jump straight to the Stefan's formula. I have a common criticism, which is well understood by authors, and I appreciate their effort in providing a detailed description of all the pros and cons of their model. I think that the length of the discussion should and could be reduced. Clearly, snow depth and organic peat layer are two major factors that will add a lot of bias to thaw depth calculation. Also, using a simple (one layer) formula has its significant limitations. However, for paleo-temperatures, it could be feasible.

I felt that authors are presenting the model as a proof-of-concept showing that this algorithm might work. The fact that it is performed well for homogenous soil is logical and not surprising. In addition, the model has a higher success rate for continuous permafrost regions, with minimum surface vegetation and climate-driven permafrost conditions (Shur et al., 2007). I do not think that the model will work well for the discontinuous permafrost areas. I suggest looking at early works by Clow (1992) on temperature inversion, that captures all the complexity dealing with inverse modeling studies applied to permafrost temperature reconstructions.

My major disappointment is that I was expecting to see how the model derives paleo-air temperatures on specific examples. That will be the best justification for me that high order bias can be neglected for paleo-air reconstruction. I have mixed feelings about this work. I appreciate the authors' effort and think that it can be valuable for a paleo-temperature reconstruction. I would be willing to suggest this work for publication once the authors will revise and paper, improve the flow, and get rid of jargon. Ideally, it would be nice to see some paleo-reconstructions cases. I suggest to be more specific from the beginning and clearly state the goal of this work. Below, I suggested minor improvements.

Minor

L2 Not sure what are the climatic controls? Rephrase and clarify.

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L5 Which ‘flaws’?

L6 What are the relict permafrost related features?

L11-12. Not sure what do you mean. Be more specific.

L14 ‘relict permafrost features’, need to define them first.

L17-18 ‘active features’, ‘relict periglacial assemblages’ need to define them as well.

L29 ‘periglacial features’, specify.

L30 ‘geometric attributes’, not sure what do you mean by that?

L34 ‘dimension of features’, specify

L69. Why authors did not Kudryavstsev’s formula instead, which incorporates the effect of soil moisture, snow, and vegetation. Need to better explain the choice, why not use more sophisticated numerical models like GIPL or Gryogrid?

Table 1 where thermal conductivities and porosities come from? Adding the effect of the organic layer will change the results of the thaw depth (e.g. Jafarov and Schaefer 2016).

L105 not sure what authors mean. Rephrase and add more clarity.

Table 2 Again specify where thermal conductivities and porosities come from.

L140 not sure why extrapolated ALT was 0.15m. it does not make sense.

L197. I would be super cautious with the high accuracy statements.

The rest of the discussion talks about caveats and explains when and why it fails. It is a fair discussion, but I found it rather long and not necessary. All these things are well-known and I would suggest to reduce it to a short summary of the pros and cons. I would rather see the applications as a justification of that this simple method was developed for a reason.

References

Clow, G.D. The extent of temporal smearing in surface-temperature histories derived from borehole temperature measurements. Global and planetary change, 6(2), 81-86 (1992)

Shur, Y. L. and Jorgenson, M. T.: Patterns of permafrost formation and degradation in relation to climate and ecosystems, Permafrost Periglac., 18, 7–19, doi:10.1002/ppp.582, 2007.

Jafarov, E. and Schaefer, K.: (2016), The importance of a surface organic layer in simulating permafrost thermal and carbon dynamics, The Cryosphere, 10, 465-475, doi:10.5194/tc-10-465-2016.

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