Author's response to comments of Referee #2

AC: We thank the anonymous referee for the review of our manuscript.

RC2: 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 (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.

AC: We will try to keep the number of jargon words to a minimum in the abstract and introduction in order to be more understandable, but please note that the model is intended to be used mainly by periglacial geomorphologists working in past permafrost environments, and thus some terminology may be difficult to leave. Also, we will make the introduction less general and more related to the aims of the manuscript.

Please note that we do not hide at all that some of the formulas can be already found in Nelson and Outcalt (1987), but we use them and have arranged them differently. Nelson and Outcalt (1987) introduced a scheme that is designed to decide whether permafrost is present at a given location based on air thawing and freezing index, while we seek to derive air temperature conditions using the thickness of the active layer. Definitely, snow cover and organic layer are important factors that affect the thickness of the active layer. Nonetheless, the effect of snow cover does not need to be accounted for in this case because the Stefan equation combined with thawing *n*-factor retrieves air thawing index responsible for a given thickness of the active layer, which is subsequently turned into annual as well as winter air temperature characteristics that are not affected by snow at all. Since most periglacial features develop under bare to grassy surfaces, we believe that the thawing *n*-factor, parameterizing the ground-surface–air temperature relations during the thawing season, can be reasonably estimated based on published values for analogous ground-surface covers. On the other hand, it would be a pure guess in the case of snow cover thickness and associated freezing *n*-factor. Consequently, we believe that the presented scheme, relying only on the thaw-season ground temperature conditions, is advantageous, also from that point of view that most active-layer features largely develop during the warm part of the year when snow cover is absent or very thin. As for organic layer, please note that it represents a substantial part of the Alaskan profiles included in the original version of the manuscript and indeed causes larger model scatters around the identity lines at these locations (see Figure 3 in the original version of the manuscript), but the overall accuracy is still very good if representative inputs are used, even in the case of an one-layer solution used. We will emphasize the above points more in the revised version of the manuscript and we will also trim down the discussion section.

We consider the referee's statements about the expected model failure in discontinuous permafrost areas as speculative because theoretical studies contrastingly suggested that it should fail rather in very cold locations where permafrost is supposed to be continuous (see Romanovsky, V.E., Osterkamp, T.E.: Thawing of the active layer on the coastal plain of the Alaskan Arctic, Permafrost and Periglacial Processes, 8, 1–22, https://doi.org/10.1002/(SICI)1099-1530(199701)8:1<1::AID-PPP243>3.0.CO;2-U, 1997). If the remark was to be based on the concern that in discontinuous permafrost regions the model could be applied to places with seasonally frozen ground, then we must assure you that this situation should not happen as the model should be exclusively applied on landforms and sedimentary structures indicative of the base of the palaeo-active layer, which indisputably formed in the presence of permafrost mostly during Quaternary cold stages.

We confirm that the original version of the manuscript was meant to be a proof-of-concept study, but now we recognize that real model application on palaeo-periglacial features is necessary. Consequently, we also intend to include in the revised version of the manuscript a palaeo-air temperature reconstruction using a palaeo-active-layer thickness and to compare its outputs with reconstructions based on other proxy records and/or model products.

RC2: Minor

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

AC: It should mean the range of climatic conditions, under which individual periglacial features form. It will be changed in the revised version of the manuscript in order to be more understandable.

RC2: L5 Which 'flaws'?

AC: It is related to the still poorly understood range of climatic conditions, under which individual periglacial features form. It will be changed in the revised version of the manuscript in order to be more understandable.

RC2: L6 What are the relict permafrost related features? L11-12. Not sure what do you mean. Be more specific.

AC: It means relict (inactive under present-day climate conditions) landforms and sedimentary structures, which formed in the presence of permafrost mostly during Quaternary cold stages. We will try to be more specific in the revised version of the manuscript.

RC2: L14 'relict permafrost features', need to define them first.

AC: As stated above, it means relict (inactive under present-day climate conditions) landforms and sedimentary structures, which formed in the presence of permafrost mostly during Quaternary cold stages. We will try to be more specific in the revised version of the manuscript.

RC2: L17-18 'active features', 'relict periglacial assemblages' need to define them as well. L29 'periglacial features', specify.

AC: The sentence will be rephrased in the revised version of the manuscript in order to be more understandable.

RC2: L30 'geometric attributes', not sure what do you mean by that? L34 'dimension of features', specify

AC: It means morphology (that is, shape and size) of periglacial features. It will be changed in the revised version of the manuscript in order to be more understandable.

RC2: 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?

AC: We build on the Stefan formula because of its simplicity and reasonable accuracy at the same time. Note that the Stefan formula also incorporates soil moisture (see Eq. 1), and the effect of vegetation (~ground-surface cover) is expressed via the empirical thawing *n*-factor (see Eq. 3), which converts ground-surface thawing index into air thawing index. Advantageously, the effect of snow cover does not need to be accounted for because the solution retrieves air thawing index, which is subsequently turned into annual as well as winter air temperature characteristics that are not affected by snow at all. This is advantageous also from that point of view that most active-layer features largely develop during the warm part of the year when snow cover is absent or very thin.

The Kudryavtsev formula requires much more additional inputs as compared to the Stefan formula, such as thawed volumetric heat capacity, frozen thermal conductivity, or mean annual ground temperature at the top of the permafrost, only to derive ground surface temperatures. Numerous other extra inputs related to snow and vegetation, such as their height or thermal conductivity, are required for the conversion between ground-surface and air temperatures (this is done using only the thawing *n*-factor in our solution). Such complexity can admittedly yield better results in present-day applications where the inputs may be easily available, but a larger number of input parameters is unsuitable for palaeo-applications as more numerous assumptions would have to be made. Obviously, it is also the case of the other models, such as GIPL or GryoGrid, which are even more sophisticated and solved numerically.
Some of the above explanations will be incorporated in the revised version of the manuscript.

RC2: 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).

AC: Table 1 in the original version of the manuscript shows what variables are inputs (upper section) and what variables are outputs (lower section). Values of the input parameters should be obtained directly from relict periglacial structures, while those that cannot be obtained directly should be derived using empirical relations (transfer functions) or should rely on representative published data that allow a meaningful range of their values to be defined. Please note that we intend to include in the revised version of the manuscript a palaeo-air temperature reconstruction, which should clearly show the above procedure.
Organic layer is certainly an issue in active-layer thickness modelling and indeed Figure 5 in the original version of the manuscript nicely documents its effect. Please note that organic layer represents a substantial part of the Alaskan profiles included in the original version of the manuscript and causes larger model scatters around the identity lines at these locations (see Figure 3 in the original version of the manuscript), but the overall accuracy is still very good if representative inputs are used.

RC2: L105 not sure what authors mean. Rephrase and add more clarity.

AC: Since the solution is designed to be used in permafrost environments, problems may occasionally arise in situations where seasonally frozen ground coexists under negative mean annual air temperature (MAAT) because permafrost–seasonal frost boundary rarely coincides exactly with MAAT of 0 °C. So the model should be applied on those features that indisputably formed in the presence of permafrost. Nonetheless, the statement will be changed in the revised version of the manuscript in order to be more understandable.

RC2: 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.

AC: Values of the input parameters should be obtained directly from relict periglacial structures, while those that cannot be obtained directly should be derived using empirical relations (transfer functions) or should rely on representative published data that allow a meaningful range of their values to be defined. Please note that we intend to include in the revised version of the manuscript a palaeo-air temperature reconstruction, which should clearly show the above procedure.

Please note that L140 does not state that the extrapolated active-layer thickness was at most only 0.15 m, but rather that it was at most 0.15 m below the deepest ground temperature sensor available for the estimation (extrapolation) of the active-layer thickness. Since the sensor was at a depth of 0.75 m, the maximum extrapolated active-layer thickness was 0.90 m. Given the maximum vertical distance between the sensor and extrapolated active-layer thickness is as low as of 0.15 m, it is assumed that the active-layer thickness is plausible and can be used for validation in this manuscript. We feel it is necessary to assure the reader about that because active-layer thickness is sometimes extrapolated to depths well under the deepest ground temperature sensors by other papers and the resulting values may thus be of questionable validity. Nonetheless, we will slightly modify this part in the revised version of the manuscript in order to be more understandable.

RC2: 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.

AC: We will moderate our accuracy statements and trim down the discussion in the revised version of the manuscript. Also, a palaeo-air temperature reconstruction will be added.

RC2: 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.