

AUTHORS' RESPONSE TO THE COMMENTS OF THE EDITOR

Dear Tomáš Uxa and co-authors,

I have carefully read through your response to the reviewer comments. You have added 11 forest and 1 shrub sites to your final list of 55 evaluation sites of permafrost monitoring. Concerning the critical comments of reviewer #3, I want to summarise your answers to each of the 4 major comments. First and with regard to the 'pseudo-observations', you now specify the sensor spacing as well as statistics on temperature differences and vertical distance between your interpolated observations and the closest sensor. Second, you explain that the CALM measurements are not suitable for a comparison due to the gridding, the measurement protocol and the often large vertical spacing of temperature sensors. Third, a baseline model comparison would require assumptions on unknown material properties (advantage of your method), which seems a somewhat vain exercise. Fourth, I am less convinced by your argument to reject a comparison to other approaches indirectly inferring bulk material properties. You state that the edaphic term for these methods can only be inferred if the ALT is measured. Please specify why ALT measurements are not available at your monitoring sites (the CALM database would hold such estimates, I guess). I request a firm argument why such a model comparison was not possible. If feasible, consider to present a brief comparison even if it was only for a few selected sites.

In summary, I continue to consider your manuscript for publication in TC and invite you to answer to my comment above (with a revised manuscript). This will only require a minor revision round.

Best,
The editor, Johannes Fürst

Dear Editor,

On behalf of co-authors, I am submitting the third revision of the manuscript ID EGUSPHERE-2024-2989 entitled "Simple analytical–statistical models (ASMs) for mean annual permafrost table temperature and active-layer thickness estimates" by Tomáš Uxa, Filip Hrbáček, and Michaela Kňázková.

Thank you for reading our previous response to the reviewer reports and commenting on it.

With regard to the fourth point of the reviewer #3. In fact, we already compared the thermal conductivity ratios and the edaphic terms modelled using Eq. (5) and (20) for the three depths pairs of 5/30 cm, 5/50 cm and 30/50 cm against these bulk material properties for the whole active layer inferred by rearranged Eq. (2) and (23) based on the observed MAPT, ALT and thawing and freezing indices for the uppermost available temperature sensors. Instead of the CALM database, however, we did this based on our validation dataset consisting of 55 locations. This is illustrated in Figures 4 and 5, and since it is a kind of by-product of the MAPT and ALT estimates that constitute the central part of our manuscript, it is only mentioned and discussed in several paragraphs of the Discussion on lines 231–240, 258–265 and 270–298. We also revised the text there slightly and provided some new citations. In addition, we included error statistics in Figures 4 and 5.

We believe that these clarifications and minor revisions of the manuscript are sufficient.

Thank you very much for considering the revised manuscript for publication in TC.

Yours sincerely,
Tomáš Uxa