Author's Reply to Reviewer Feedback: *Modelling active layer thickness in mountain permafrost based on an analytical solution of the heat transport equation, Kitzsteinhorn, Hohe Tauern Range, Austria.*

Dear Reviewers,

We appreciate the time and effort you have dedicated to reviewing our manuscript. Our responses to your comments are provided below and are written in blue text. Please note that all line references correspond to the manuscript version with tracked changes.

In revising the manuscript, we now explicitly emphasize a key limitation of our modeling approach: its clear focus on steep bedrock environments. This prompted us to make a slight adjustment to the title and various parts of the manuscript. To indicate that latent heat effects play only a subordinate role in our study, we have included porosity values, which led to the addition of a new co-author. Additionally, in response to the reviewers' comments, we made minor adjustments to improve clarity and readability and corrected typographical errors. Furthermore, an incorrectly assigned value in Table 1 has been corrected, and we have added six new references.

Review #1

General comments.

This study is on active layer thickness in an alpine rock permafrost environment with borehole temperature measurements and analytical modeling of the heat diffusion equation. The measurements and data set used is very interesting and the underlying problem of using shallow ground temperature measurements (here at 0.1 m) for making predictions of ALT and permafrost warming is very important especially in the context of climate change and its severe impacts for alpine regions.

However, the thermal modeling approach used is not new or novel, and has very limited applicability for active layer modeling. The equations described are well-known analytic solutions to the heat conduction (diffusion) equation and can be found in textbooks on the subject, see e.g. Carslaw and Jaeger (Conduction of heat in solids, 1959), Williams (The Frozen Earth, 1989), Woo (Permafrost Hydrology, 2012), and others. It is severely limited for active layer and permafrost modeling because it does not represent many of the critical freezing and thawing processes which control heat propagation in the subsurface.

Author's Response: Many thanks for your comprehensive review and insightful comments. We fully agree that the model approach used is neither new nor novel and has limitations in its application for modeling Active Layer Thickness (ALT) due to the neglection of latent heat and convection. We have now emphasized this limitation more clearly in the manuscript which also led us to change the title. However, despite these limitations, we firmly believe that our study provides valuable evidence for the first time that ALT in conduction-dominated bedrock permafrost can be effectively modeled using this straightforward, time- and cost-efficient approach.

Many of the assumptions imposed are mentioned in various parts of the manuscript and include neglecting latent heat exchange, advective heat carried by water migration (both lateral and vertical), snow insulation and snowmelt, and heterogeneity in subsurface thermal and hydraulic properties. Critically, even for homogenous ground, latent heat, thermal conductivity and heat capacity change due to variations in liquid-ice-air phase saturation in the active layer, and these changes need to be accounted for in order to be able to calculate heat propagation through the active layer and to the permafrost table and below. A possible exception could be if the ground is extremely dry. Or if the interest is only long-term and deep temperature responses, say below the depth of zero annual amplitude. Even if the intact/unfractured bedrock contains little pore space and hence may be suitably represented by heat conduction, there are evidently fractures feeding the subsurface with unfrozen water. This is noted in the discussion in Section 5.1 and is clearly seen by the prolongation of temperature near the freezing point (the zero-degree curtain) for the thermal sensors located at 3 m depth in Fig 3a.

Author's Response: We find strong evidence that ALT formation is primarily governed by conduction. Additionally, snow insulation is incorporated into our modeling, as the shallow boreholes at a depth of 0.1 m inherently include snow cover effects. Furthermore, we believe that the applicability of the presented approach is not confined to the studied rock faces (Kitzsteinhorn) but can be extended to other active layers in primarily conduction-driven systems within bedrock. We do not observe any site-specific conditions that would significantly distinguish the studied site from other high-alpine bedrock environments. We have now integrated complementary data (porosity values) and a new reference (Offer et al., accepted; preprint: https://doi.org/10.5194/egusphere-2024-893) into the manuscript to further support our point: the fact that the presented simplistic analytical solution provides excellent results in an environment that is – at least periodically (during the spring/summer season) – impacted by subsurface water flow indicates, in our view, that our approach should have wide applicability in frozen, steep bedrock environments.

Several analytical and semi-analytical approaches have been developed to address the limitations of the analytical solution to the heat conduction equation, other than full numerical solutions of the coupled system of differential equations for heat transport with water flow. For some background and an overview, please see the excellent papers by Riseborough et al. (2008) and Kurylyk et al. (2014). Perhaps some of those more robust methods could be employed to make predictions on ALT based on the shallow boreholes, or even better, numerical models which incorporate even fewer limitations.

Riseborough, D., Shiklomanov, N., Etzelmüller, B., Gruber, S., Marchenko, S., 2008. Recent advances in permafrost modeling. Permafrost and Periglacial Processes 19, 137–156. https://doi.org/10.1002/ppp.615

Kurylyk, B.L., McKenzie, J.M., MacQuarrie, K.T.B., Voss, C.I., 2014. Analytical solutions for benchmarking cold regions subsurface water flow and energy transport models: One-dimensional soil thaw with conduction and advection. Advances in Water Resources 70, 172–184. https://doi.org/10.1016/j.advwatres.2014.05.005

Author's Response: We are fully aware that an analytical solution is less sophisticated than a numerical solution and is not capable of integrating water flows and latent heat. However, in alpine terrain, missing information on water balance and subsurface flow is the norm (with only a few exceptions), which can also lead to inaccuracies in model calibration. This means that in conduction-driven active layers, an analytical solution does not necessarily have to be less robust than a numerical solution, especially when the goal is determining annual thaw depths rather than precise modeling of ground temperatures. This is the case for the present manuscript, which explicitly focuses on the simulation of the maximum ALT in high-alpine environments due to its significant impact on natural hazard occurrence and infrastructure management.

The analytical solution of the heat conduction equation eliminates computational constraints that can limit the number of measurement points and simulation runs. Simulations with different boundary conditions can be carried out very easily compared to numerical solutions. For example, using the idealized boundary conditions of the analytical solution, we found that a warmer initial thermal state leads to a greater increase in ALT compared to a colder initial state under the same temperature rise. Estimating ALT based on near-surface temperature data in combination with analytical modeling can therefore be a simple yet very help- and powerful tool, for example, to implement in an expert system for bedrock permafrost regions. However, to provide a better overview of different modeling approaches, we have now incorporated the review paper by Riseborough et al. (2008) into the Introduction (refer to lines 69-70).

Another problem is the biased implementation of the NSE as an error metric, which seems to be based on modeled values after discarding values which perform poorly (Fig 5a). This is a biased filtering and is probably the reason high values of the NSE metric are obtained. Clearly, if all the modeled data are used, including the freeze-up period, and compared against the measurements, the NSE metric will be much lower.

Author's Response: Thank you very much for this entirely valid remark. In the revised manuscript, we now also report the NSE without any model adjustments (refer to line 328-330). This feedback prompted us to further reflect on the statistical analysis. The analysis of daily values was conducted as a secondary model validation, explicitly focusing on the thawing rather than the freezing process. Therefore, we deemed it justifiable to adjust the model values after the maxima by setting them to 0 m depth. Since our primary focus is not on the day-to-day modeling of thaw depth throughout the year, the main model validation now centers primarily on the annual maxima (ALT). Given that this comprises only three years—and thus three data points—the model performance was evaluated only using RMSE, as NSE is statistically inappropriate for such a limited data set (refer to lines 23, 243-245, 335-337, 406-421, 531-534).

Finally, the interpretation of the ALT projections for the shallow boreholes SB 2, 3 and 4 in Section 4.3 and 5.3 is questionable. These boreholes seem to be grouped near a peak and the spatial distance between them only seems to be a few 10s of meters. Despite their proximity, the prediction on the ALT/PF table are very different (Table 5), from no PF ("infinite" ALT) in SB3 to relatively shallow ALT of 2-3 meters in SB2. It is unrealistic to infer that the permafrost table varies as extremely as this. The result is probably an artifact of the method which assumes 1D conduction only, and that the ALT prediction is based solely on a single near-surface temperature sensor. In reality of course, lateral subsurface heat transport distributes the thermal response and the permafrost extent will be smoother even if differential warming occurs on different faces of the mountain surface.

Author's Response: This is a very good objection and we acknowledge that we should have discussed this in more detail. Due to lateral heat flow between the three summit sites (SB2-4) the real/measured ALT will certainly differ significantly from the modeled ALT. We have revised sections 4.3 and 5.3 which now include explanations that the SB sites are interpreted by us as idealized representations of their respective slope aspect not affected by 3D topography effects (refer to lines 356-359, 444-448).

Detailed comments.

L125-130. Has the thermal effect of the concrete annulus been evaluated? It could be that the thermal properties of concrete have an effect on heat conduction between the rock and the brass segments and potentially bias sensor measurements. Also, there could be effects of vertical heat conduction in the concrete annulus which may differ from that of the bedrock.

Author's Response: We think the influence of the concrete annulus is negligible for the following reasons: (i) due to the 45° drilling angle the entire casing rests on the borehole 'floor', the bottomside of the casing is therefore in permanent physical contact with the surrounding bedrock; (ii) thermal conductivity differs only slightly between concrete (~ 2 W m^{-1} K^{-1}) and schists (~ 3 W m^{-1} K^{-1}). We revised the manuscript accordingly (refer to lines 150-155).

L130-135. Is the surface smooth bedrock or is there overburden and unconsolidated material? It is difficult to judge from the photograph in Fig 2b, and not entirely clear how the depth 0.1 m is obtained, because it seems surface roughness could easily vary within this range of a few dm.

Author's Response: Thank you for pointing that out. We have revised Fig. 2 to give the reader a better impression of surface properties and microtopography. We furthermore added one sentence to the manuscript (refer to lines 157-160). All shallow borehole locations are characterized by (i) a compact rock mass without significant fractures, (ii) a uniform microtopography ('clean' slope aspect), and (iii) the absence of any unconsolidated sediment cover.

L250-255. Water saturation generally increases with depth and, more critically, changes over time with variable infiltration from snowmelt, rainfall, and seasonal freezing/thawing.

Author's Response: The depth-dependent amplitude damping appears remarkably uniform. This result highlights that the effect of joint and pore water, including its spatial (depth) and seasonal variability, does not play a significant role in the damping of annual temperature waves. In the revised version, the effect of pore and joint water on local ALT formation is further elaborated upon in the discussion (refer to lines 399-404, 422-429).

L195-200. The problem with the damping depth calibration approach used is that it applies to the thaw depth of the time period considered. With the climate warming scenarios imposed later, the thaw depth changes, yielding the calibration obsolete. The calibration-validation exercise simply indicates no significant changes occur within the time period.

Author's Response: The damping depth is governed by the thermal diffusivity and the specified period length (see Eq. 5). This means that the propagation of the annual active layer into the subsurface was modeled based on the specific thermal diffusivity of the bedrock. Consequently, this specific thermal diffusivity of the active layer remains valid under warmer atmospheric conditions and, therefore, with increased thaw depths. This, of course, requires the fundamental assumption of a relatively homogeneous subsurface. In our study area, we consider this assumption to be well-supported.

Fig 3a shows the zero-degree isotherm, indicating presence of water and showing that latent heat exchange is playing a significant role. Thus, the assumptions used for the model are questionable.

Author's Response: Thanks to the helpful feedback, we now have explicitly limited the applicability of our model approach to bedrock permafrost (which has low water content compared to soils). Once again, the focus of our work is on modeling ALT using a time- and cost-efficient method, rather than achieving the most precise possible modeling of subsurface temperatures. We firmly believe that the assumptions made are valid for our purposes. While joint and pore water certainly had some influence (e.g., thermal offset and a zero-curtain effect), the combined evidence from the (i) remarkable homogeneous damping of temperature amplitudes with depth (Fig. 4c), (ii) smooth temperature change over time and depth without abrupt jumps (apart from small temperature jumps at 2–5 m depth at the onset of each thaw season), and (iii) excellent model results from a model approach that is exclusively conduction-based clearly indicates that conduction is the dominant heat transfer mechanism for local ALT formation. Laboratory tests of local calcareous mica schist core samples demonstrate an effective porosity of only 0.3–0.4 % (refer to line 106-111 and table C1). Based on this low porosity we assume that the effect of latent heat transport is only marginal and has no significant effect on ALT formation, which thus supports the validity of excluding latent heat effects from our model. In the revised version, the effect of pore and joint water on local ALT formation is now further elaborated upon in the discussion (refer to lines 399-404, 422-429).

Eqn 7. It is not clear how temperature comes in to play for the equation for d_phase?

Author's Response: Thank you very much for this helpful remark, it is now more clearly described (refer to lines 213-216).

L195-200. The NSE reference is not included in the bibliography. Also, values can be negative, indicating predictions are worse than the mean of the measured data, i.e., not capturing the variability of measurements.

Author's Response: Glad you noticed that; now the NSE is correctly explained (refer to lines 235-237), and the reference has been provided (refer to lines 698-699).

Fig 4b. Unclear why only two years are shown. Please make the markers more visible, currently they are difficult to see. The colors for 2017 and 2018 are inconsistent with Fig 4c. Is it correct there are only 3 measurements in the interval 0-2 m; if so, then a linear regression is of course expected to fit well. Same goes for Fig 4c.

Author's Response: In the figure caption, we have now explained in greater detail why only two years and three depths are shown in Fig. 4b (refer to line 304). Accurate calculation of the damping depth from the phase shift ($d_{\_phase}$) was only feasible for the years 2017 and 2018, and only across three measurement depths due to inharmonic temperature oscillations in other years and at the three-meter depth. You are right, in that case, the determination of $d_{\_phase}$ is less robust compared to the linear regression across four depths and all six years, as is the case with the amplitude damping shown in Fig. 4c. The colors in the Fig. 4b indicate different years and are consistent with those in Fig. 4c, so the color scheme remains unchanged. The markers have been made more visible in the revised version (thank you for this very helpful suggestion).

It would be useful to show a trumpet diagram of the temperature measurements with depth (similar to Fig 4a) for the deep borehole with systematically selected times, e.g., once a month. This serves to present data without interpolations as in Fig 3b and A1, and will help visualize the active layer depth, the depth of zero annual amplitude, if there is an inflection point, and if the temperature trend with depth indicates a transition to the permafrost base. This can readily be combined with annual averages for each depth.

Author's Response: We've included the suggested trumpet curves into the manuscript. Please refer to new figure A2.

Fig 5a. The modeled values can of course be calculated at all depths but the measurements are only made at specific depths. Therefore, it would be reasonable to plot lines for the simulations and markers for the measurements at their corresponding depths. This would help clarify the comparison between modeled results and measurements. Also, it would be helpful to clarify that the values in brackets indicate the ALT.

Author's Response: Very helpful suggestions, thank you. The graphic has now been revised. Please refer to Fig. 5a.

Fig 5a and Section 4.2. It seems by ignoring the data which does not fit the model is the reason high NSE and RMSE values are obtained – but this is by biasing the measurements and as such highlights the limitations of the model approach.

Author's Response: Thank you very much for this entirely valid remark. In the revised manuscript, we now also report the NSE without any model adjustments (refer to line 328-330). This feedback prompted us to further reflect on the statistical analysis. The analysis of daily values was conducted as a secondary model validation, explicitly focusing on the thawing rather than the freezing process. Therefore, we deemed it justifiable to adjust the model values after the maxima by setting them to 0 m depth. Since our primary focus is not on the day-to-day modeling of thaw depth throughout the year, the main model validation now centers primarily on the annual maxima (ALT). Given that this comprises only three years—and thus three data points—the model performance was evaluated only using RMSE, as NSE is statistically inappropriate for such a limited data set (refer to lines 23, 243-245, 335-337, 406-421, 531-534).

Fig 5a. The main reason the freeze-up is not represented well by the model is the absence of latent heat.

Author's Response: We agree that latent heat exchange is an important process when pore water is subject to phase shift. However, in our case pore water content is very low (refer to line 106-111 and table C1) so that we consider conduction to be the dominant factor driving ALT evolution in our case. However, the significant deviations observed at the onset of the autumn freezing process are very likely attributable to changes in thermal conductivity induced by phase changes of water in joints. This could explain why the freezing front might advance more rapidly into the profile (as ice exhibits higher thermal conductivity compared to unfrozen water), causing the model to lag during this short period.

Review #2

The paper, "Modelling Active Layer Thickness in Mountain Permafrost Based on an Analytical Solution of the Heat Transport Equation, Kitzsteinhorn, Hohe Tauern Range, Austria" by Wolfgang Aumer, Ingo Hartmeyer, Carolyn-Monika Göres, Daniel Uteau, and Stephan Peth, introduces a pioneering approach to modelling active layer thickness (ALT) in mountain permafrost. It is the first study to use analytical solutions of the heat transport equation for this purpose. The field protocol presented is highly valuable to the permafrost research community, as its methods have the potential to provide an enhanced understanding of permafrost characteristics in similar environments. The study meticulously analyzed methodological parameters and validated its results by comparing them with borehole data, achieving strong agreement. The analytical approach effectively identified the maximum ALT during the observation period. Additionally, the model incorporated IPCC climate projections to simulate ALT changes expected by the middle and end of this century.

The paper is well-structured, thoroughly illustrated, and includes critical reflections that lead to well-supported conclusions. The methodology is rigorous and consistent and the results support the findings. I therefore recommend acceptance of the manuscript, pending minor revisions as outlined below:

Author's Response: Thank you for your encouraging comments. We are pleased that our work has been well received.

In the Introduction, you mention that this approach is novel for modelling active layer thickness. However, I feel that the potential of this technique for addressing existing limitations is not sufficiently discussed. I suggest adding several sentences in Section 5 (Discussion) to clarify how this analytical approach can help overcome current limitations and how it might be integrated into a more complex model to incorporate additional variables (e.g., snow cover).

Author's Response: We have now added several new sentences, structured under a new heading in the Discussion section (refer to lines 492-513).

Lines 34–35: You mention large-scale rock avalanches at Piz Cengalo and Fluchthorn, but only specify the country for Fluchthorn. To maintain consistency, please also consider adding the country for Piz Cengalo.

Author's Response: Many thanks for the hint, the country has now also been added (refer to line 39).

Line 37: "Direct ground temperature measurements have shown, in some areas very rapid, warming in the European Alps over recent decades (Harris, 2003; Etzelmüller et al., 2020)." Harris (2003) is now 20 years old, so this phrasing feels outdated. I suggest rephrasing this sentence and reconsidering the use of this citation.

Author's Response: You are right, we are now only relying on the conclusive study by Etzemüller et al. 2020 (refer to line 42).

Table 2: The slope aspect for SB3 and SB4 seems unusual. Could you clarify the terms "ONO" and "SO"?

Author's Response: Good thing you noticed that—it was just an oversight, and now the correct abbreviations are in place. Please refer to the updated Table 2.

Figure 1: Could you overlay permafrost distribution on this map? It's unclear if all sites are situated in permafrost conditions.

Author's Response: Thank you for the helpful suggestions. We have now included the probability of permafrost distribution based on topo-climatic factors in the revised Fig. 1.

I'm also curious about the primary reasoning behind the sampling strategy. Why were three SBs placed close together, while one was set farther away? Was this influenced only by the slope aspect?

Author's Response: Thanks for pointing this out, this is something we should have explained in more detail. We've added a short paragraph to make it clearer. The selection of SB spots was mainly determined by slope aspect and altitude and some pragmatic constraints (accessibility and data availability) (refer to lines 119-125).

For model development, you calculate mean daily values using 12 measurements per day, but for model application, you used 8 measurements per day for the mean. Why not use a consistent approach for both?

Author's Response: The used 3-hour-interval (= 8 measurements per day) is related to the limited memory of the iButton. More frequent measurements would have resulted in readout periods shorter than one year which was not feasible. We added this information to the manuscript (refer to lines 175-176).

Line 191: You cite Dobiński (2011), but it is missing from the reference list.

Author's Response: Good that you noticed this, now it is fixed (refer to line 615).

Line 382: It would be interesting to compare your findings with existing permafrost distribution models.

Author's Response: We have now included permafrost distribution modeled with "PERMAKART 3.0" (empirical-statistical model based on a topo-climatic key) in Fig. 3, which indicates a high level of consistency between our ALT model and the mentioned permafrost distribution model. Additionally, we briefly discuss the potential of upscaling our ALT model to larger areas, similar to the application of (large-scale) permafrost distribution models (refer to lines 506-513). A direct comparison with permafrost distribution models over larger test areas would go beyond the scope of the present study but definitely represents an interesting avenue for future research.

Lines 378–389: You state that the slope aspect is the dominant topographic factor influencing ALT in this study. However, you also mention that the south-facing borehole is slightly colder and has a higher permafrost probability, likely due to local snow cover effects. This suggests it would be beneficial to discuss the interaction between snow cover and ground surface temperature in more detail. While slope aspect is a key factor in the evolution of ALT, its influence is straightforward only when the snow cover regime is uniform across all sites; otherwise, interpretations become more complex.

Author’s Response: This is a very good point indeed, which we have addressed in more detail now (refer to lines 466-473).

The IPCC scenario simulations consider only temperature, but you previously showed that snow cover also plays a key role. Please add a sentence addressing the uncertainties associated with excluding other controlling factors such as snow cover.

Author’s Response: Very good point, we have now added a sentence about this in the discussion (refer to lines 478-480).

Several references cited in the text are missing or contain inconsistencies. For example, Dobiński (2011) is missing from the reference list, line 573 shows inconsistent author name formatting (Schrott et al.), and line 585 lacks author initials. Please carefully review and revise the reference list for accuracy and consistency.

Author’s Response: Thank you very much for noticing that. We have updated the bibliography, ensuring that all references are included and consistent (refer to lines 596-737).