AUTHORS' RESPONSE TO THE COMMENTS OF THE REFEREE #1

RC1: GENERAL COMMENTS

The authors propose a simple method for estimating mean annual permafrost table temperature and active layer thickness solely based on temperature monitoring at two depths within the active layer. The approach, based on the TTOP formula, is elegant and could be helpful for the interpretation of field data. The main advantage is that, considering yearly integrated observations of soil temperature at two depths, the proposed method avoids the need of using ground properties measurements, at the price nevertheless of strong simplifying assumptions.

However, the assessment of the performance of the proposed method should be thoroughly improved. The validation based on numerical simulations in idealized cases is not relevant, using an outdated modelling approach as the reference. The validation against field data is good, but too few sites are considered. Once these problems solved, the discussion of the limitations related to the strong underlying assumptions (e.g.: constant ground properties) should be carefully made.

Thus I recommend major revisions of this manuscript prior to consider its publication in The Cryosphere.

AC: We are well aware that there exist highly sophisticated models of heat and water transfer in freezing and thawing grounds and that the numerical model used may be viewed as "outdated" or "old-fashioned" from this perspective. However, we still do believe that the numerical simulations for idealized scenarios are relevant because they are meant to evaluate if the analytical–statistical models were derived correctly. Hence, the numerical model is simple so that it can mimic the behaviour of the analytical–statistical models together with their (simplifying) assumptions and boundary conditions, which is a standard practice. We think it does a right (and required) job in this respect because the numerical validations are not obscured by processes that the analytical–statistical models do not account for. Conversely, even sophisticated numerical freeze-thaw models are frequently simplified (e.g., Westermann et al., 2023) so that they can be validated by some benchmark analytical solutions (sinusoidal oscillations or step changes in upper boundary temperature for subsurface temperatures, Neumann or Stefan solutions for freeze-thaw depths) under idealized/simple conditions. This might also be considered simplistic and therefore "outdated" or "old-fashioned", but is not, as the benchmarks are exact solutions for the given problems. We will incorporate some of the above thoughts in the revised manuscript.

Complex scenarios with heat and water transfer in heterogeneous freezing and thawing grounds are where field data have their place in terms of the model validations. We will roughly double the number of the validation sites in the revised manuscript, which will cover much of the world's major permafrost regions (Antarctica, Arctic, Tibetan Plateau and high mountains) and offer us greater heterogeneity in terms of permafrost table temperature and active-layer thickness (approximately −19 °C to −2 °C for MAPT and <50 cm to >200 cm for ALT), as well as in terms of surface cover, active-layer composition and stratigraphy, or permafrost zones.

Westermann, S., et al. (2023). The CryoGrid community model (version 1.0) – a multi-physics toolbox for climate-driven simulations in the terrestrial cryosphere, Geosci. Model Dev., 16, 2607–2647, https://doi.org/10.5194/gmd-16-2607-2023

RC1: SPECIFIC COMMENTS

l 8: “which corroborated their theoretical assumptions under idealized scenarios” ; unclear, please rephrase.

AC: We will remove this sentence from the abstract because it is unnecessary there.

RC1: l 66: “Besides surface temperatures, Eq. (1) is valid for temperatures measured at any depth in the active layer”. Please clarify here what is exactly meant by ‘is valid’ ; has it been validated against field data? With which general procedure?

AC: It should mean it works with data from any depth in the active layer, which was suggested in Riseborough (2004).

Riseborough, D. W. (2004). Exploring the parameters of a simple model of the permafrost–climate relationship. PhD Dissertation, Carleton University, Ottawa, 330 pp.

RC1: l 76: Eq. (5) (and generally the TTOP formula used here) imply the assumption that the thawed soil thermal conductivity kt is constant over time, both at seasonal and multi-annual time scale. The frozen soil thermal conductivity kf is also considered as constant at multi-annual time scale I guess. Since kt does depends on the soil water content, it varies within an active season and along the years according to the variability of precipitations (and thus infiltration, and thus soil water content). This is a strong assumption that must be pointed out here and extensively discussed in the paper.

AC: We agree that the assumption that thermal conductivities of both thawed and frozen ground, and thereby their ratio, are constant at (usually) multi-annual time scales is very simplifying, as ground water content varies within and between years. However, the thermal conductivity ratio expressed by Eq. (5) utilizes thawing and freezing indices at two distinct depths in the active layer. This means that the thermal conductivity ratio involves seasonal variations in ground water content, and thereby thermal conductivity, within the depth layer between temperature sensors used. And since the thawing and freezing indices are calculated annually, the thermal conductivity ratio varies between years. We consider this to be a major improvement over the classic approach with constant thermal properties/thermal conductivity ratio (that were, moreover, frequently only estimated, not measured!). We will discuss this more extensively in the revised manuscript.

RC1: l 94-95: "This documents that Eq. (8) for MAPT is analytical and statistical at the same time because it integrates both approaches." ; I don't understand.

AC: This should simply state that two different derivations ([1] analytical approach with two TTOP formulas for two distinct depths in the active layer and [2] statistical approach with linear extrapolation based on freezing indices for two distinct depths in the active layer) both lead to the same final formula, which is nice and notable. However, we will try to reformulate this a little in the revised manuscript so that it is more understandable.

RC1: l 100: Eq. (13) implies that the volumetric water content ϕ is constant over time, although it varies within an active season and at multi-annual time scale depending on precipitations. Same remark for kt (see also my specific comment at line 76). This is a strong assumption that must be pointed out here and extensively discussed in the paper.

AC: The same response as to that at line 76 also applies here. Similarly, we will discuss this more extensively in the revised manuscript.

RC1: l 128-129: "Usually, Eq. (21) has been referred to as the modified Stefan model and proved to be useful in situations where the ground physical properties were unavailable and/or for spatial modelling of ALT". Eq. (21) and eq. (13) are strictly equivalent. May be that the difference that the authors want to point out is that the edaphic term in (21) maybe calibrated in itself, without estimating the thawed heat conductivity and the volumetric water content separately. But would it be really different to make a two parameters calibration for kt and ϕ? Anyway these ones would be estimated averages, probably calibrated as well, since these quantities do vary in time (see specific points l 76 and l 100).

AC: It is true that Eq. (13) and (21) are equivalent, but they were used separately in previous publications because of their different requirements for input parameters. As you pointed out, Eq. (13) requires thawed thermal conductivity and volumetric water content as two separate variables, which can be determined by field measurements. By contrast, Eq. (21) uses a combination of both in the form of the edaphic term (Eq. 22), which can be calibrated using thawing index and active-layer thickness. Both approaches have their advantages and disadvantages. For us, it is important to show that thermal conductivity and water content can be combined, which essentially leads to Eq. (23) and (24) that, however, do not require the active-layer thickness to estimate the edaphic factor (…to estimate the active-layer thickness).

RC1: l 157-158: “As with Eq. (8), this documents that Eq. (27) for ALT is analytical and statistical at the same time because it integrates both approaches.” ; I don’t understand.

AC: Similar to above, this should simply state that two different derivations ([1] analytical approach with two Stefan formulas for two distinct depths in the active layer and [2] statistical approach with linear extrapolation based on squared thawing indices for two distinct depths in the active layer) both lead to the same final formula, which is nice and notable. However, we will also try to reformulate this a little in the revised manuscript so that it is more understandable.

RC1: l 168: The numerical model used for solving heat transfer in the active layer is a very old fashioned one (Carslaw and Jaeger, 1959). Since then numerous modelling works as been done for the simulation of heat and water transfers in soils with freeze-thaw (see for instance the benchmark of Grenier et al., 2018, or the reviews of Bui et al., 2020 and Hu et al., 2023). A more up to date model should be used.

C. Grenier, H. Anbergen, V. Bense, et al., Adv. Water Resour. 114 (2018) 196–218, https://doi.org/10.1016/j.advwatres.2018.02.001

M.T. Bui, J. Lu, L. Nie, A review of hydrological models applied in the permafrost-dominated Arctic region, Geosciences 10 (2020) 401, https://doi.org/10.3390/geosciences10100401

Hu G., Zhao L, Li R., Park H., Wu X., Su Y., Guggenberger G., Wu T., Zou D., Zhu X., Zhang W., Wu Y., Hao J.: Water and heat coupling processes and its simulation in frozen soils: Current status and future research directions, CATENA, Volume 222, 106844, ISSN 0341-8162, doi:10.1016/j.catena.2022.106844, 2023.

AC: As argued above, the numerical simulations are meant to evaluate if the analytical–statistical models were derived correctly. Hence, the numerical model is simple so that it can mimic the behaviour of the analytical–statistical models together with their (simplifying) assumptions and boundary conditions, which is a standard practice. We think it does a right (and required) job in this respect because the numerical validations are not obscured by processes that the analytical–statistical models do not account for. Complex scenarios with heat and water transfer in heterogeneous freezing and thawing grounds are where field data have their place in terms of the model validations.

RC1: l 179: Using eq. (32) for the reference numerical simulations prevent to consider the effect of the coupling of water flow and heat transfer, since volumetric water content is considered as constant (see table 1). Meanwhile, spatial and temporal variations of water content may be of primary importance for soil thermal regime (see for instance Kurylyk and Watanabe 2013, Sjöberg et al. 2016, Orgogozo et al., 2019). A more complete model should be used.

Kurylyk B.L., Watanabe K., 2013. The mathematical representation of freezing and thawing processes in variably-saturated, non-deformable soils, Advances in Water Resources, Volume 60, Pages 160-177, ISSN 0309-1708, doi:10.1016/j.advwatres.2013.07.016., 2013.

Sjöberg Y., Coon E., Sannel A. B. K., Pannetier R., Harp D., Frampton A., Painter S. L. and Lyon S. W., 2016. Thermal effects of groundwater flow through subarctic fens: A case study based on field observations. doi:10.1002/2015WR017571, 2016.

L. Orgogozo, A.S. Prokushkin, O.S. Pokrovsky, C. Grenier, M. Quintard, J. Viers, S. Audry, Permafr. Periglac. Process. 30 (2019) 75–89, https://doi.org/10.1002/ppp.1995

AC: As argued above, we agree that spatial and temporal variations in water content can substantially affect the ground thermal regime. On the other hand, the TTOP and Stefan models, on which the newly devised analytical–

statistical models build, explicitly assume (in their simplest versions) that water content is constant. Since the numerical model is intended to mimic the behaviour of the analytical–statistical models together with their (simplifying) assumptions and boundary conditions, it also considers the water content as constant. We think the numerical model does a right (and required) job in this respect because the numerical validations are not obscured by processes that the analytical–statistical models do not account for. Complex scenarios with heat and water transfer in heterogeneous freezing and thawing grounds are where field data have their place in terms of the model validations.

RC1: l 256: "Overall, however, these findings corroborate the theoretical assumptions outlined in Sect. 2.2" Please be more specific. Which precise assumptions?

AC: This should mean the modelling approach proved to be feasible based on the numerical tests. We will change this statement in the revised manuscript so that it is clearer.

RC1: l 263: "ALT estimates by Eq. (27) were more accurate in Antarctica" I think that this is due to the fact that soil water content varies much more in the Alaskan sites that in the Antarctica sites. The bimodal distribution in the bottom left graph of Figure 5 is maybe also due to this.

AC: This is right and we will incorporate it into the revised manuscript. Still, we think this is mainly caused by the active-layer stratigraphy, which is almost exclusively one-layer in Antarctica and two-layer in Alaska. However, this pattern can change because the number of validation sites will roughly double in the revised manuscript.

RC1: l 271: "a reasonable accuracy" ; reasonable according to which criterium ?

AC: This was meant in the context of previous publications that used the TTOP and Stefan models, but we agree it is vague. This will be changed in the revised manuscript.

RC1: l 272: "which corroborated their theoretical assumptions (see Sect. 2.1 and 2.2)" ; unclear, please rephrase.

AC: This should mean the modelling approach proved to be feasible based on the numerical tests. We will change this statement in the revise manuscript so that it is clearer.

RC1: l 273: "they can work reasonably well under a wide range of climates and ground physical conditions" ; this has to be demonstrated by the discussion.

AC: Especially, this was demonstrated by the validation results based on field measurements. However, as stated above, we will roughly double the number of the validation sites in the revised manuscript, which will cover much of the world's major permafrost regions (Antarctica, Arctic, Tibetan Plateau and high mountains) and offer us greater heterogeneity in terms of permafrost table temperature and active-layer thickness (approximately −19 °C to −2 °C for MAPT and <50 cm to >200 cm for ALT), as well as in terms of surface cover, active-layer composition and stratigraphy, or permafrost zones. This will be shown in the results and more extensively discussed in the revised manuscript.

RC1: l 277-278: "Under field conditions, the ASM deviations were close to zero on average" ; this statement seems not in line with the results shown in Figure 5.

AC: In fact, it cannot even be consistent with Figure 5 because it discusses the mean annual permafrost table temperature, whereas Figure 5 shows the active-layer thickness.

RC1: l 315-317: "Since ASMs build solely on thawing and freezing indices at two distinct depths in the active layer, the values of which reflect the rate of heat transfer across their intermediate layer, the solutions also intrinsically account for the temporal variability of ground physical properties." ; I do not agree. According to eq. (1), the TTOP formula on which is based the ASMs does not take into account these temporal variabilities.

AC: Yes, the TTOP formula given by Eq. (1) does not indeed account for temporal changes in ground physical properties. However, the ASMs do because they utilize thawing and freezing indices at two distinct depths in the active layer, the relationships (or gradients) between which are determined by natural variability in ground physical properties within the depth layer between temperature sensors throughout the year. This is a fundamental misunderstanding of the whole procedure. Think of it like, for instance, ground thermal diffusivity calculations, which build on damping temperature amplitudes or phase lags between two distinct depths, which is governed by the thermal diffusivity of the ground layer in between. The ASMs have in principle the same rationale. We will try to incorporate some of the above thoughts in the revised manuscript so that this is clearer.

RC1: l 317-319: "Likewise, they consider latent and sensible heat and any other factors that might affect the heat transfer in the active layer, some of which other models do not explicitly account for." ; same thing that the previous comment.

AC: Same response as the previous comment.

RC1: l 325-326: "Ground physical properties also commonly show more or less variability on seasonal and annual time scales […] which most other models cannot handle because they typically treat ground physical properties as constants." ; I think that it is also the case here, according to eq. (1).

AC: Same response as the previous comment.

RC1: l 359-360: "ASMs for estimating MAPT and ALT can find applications under a wide range of climates and ground physical conditions" ; it sounds to me like an overstatement. Two sites where investigated, largely not enough to sample the variety of permafrost environments: continuous/discontinuous/sporadic, in various environments such as for instance tundra or boreal forest, with diverse lithology and pedology, under various climatic (e.g. precipitation) conditions, etc.

AC: Please note we did not investigate only two sites, but two types of permafrost environments (Antarctica and Alaska) where there were a total of 17 sites. As stated above, moreover, we will roughly double the number of the validation sites in the revised manuscript, which will cover much of the world's major permafrost regions (Antarctica, Arctic, Tibetan Plateau and high mountains) and offer us greater heterogeneity in terms of permafrost table temperature and active-layer thickness (approximately −19 °C to −2 °C for MAPT and <50 cm to >200 cm for ALT), as well as in terms of surface cover, active-layer composition and stratigraphy, or permafrost zones. We will then update (or possibly not) the statement based on the results of these more extensive validations.

RC1: TECHNICAL CORRECTIONS

l 288-289: "in the order of tenths to first degrees Celsius" ; english language problem.

AC: We will change this in the revised manuscript.

RC1: l 346-350: Not necessary.

AC: We will remove this from the revised manuscript.