Author’s response

to reviewers on the initial submission of the preprint tc-2023-51

"Coupled thermo-geophysical inversion for permafrost monitoring"

by Soňa Tomaškovičová & Thomas Ingeman-Nielsen

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>> We thank the reviewer for the thoughtful review and for acknowledging the value of the presented work. We are particularly thankful for the excellent suggestions on how to improve the discussion section of the paper. All of the reviewer’s suggestions were thoroughly considered and implemented in the revised manuscript, which we look forward to uploading upon the editor’s invitation.

Our replies are indicated in “>> the blue paragraphs”.

Reviewer 1

The manuscript “Coupled thermo-geophysical inversion for permafrost monitoring” by Tomaškovičová and Ingeman-Nielsen presents a new inversion algorithm for ERT measurements in permafrost which uses a heat conduction model to constrain the possible subsurface temperature and thus resistivity distributions. Although the presented method still has serious limitations, it is an important step towards a more quantitative use of ERT measurements for permafrost monitoring. The manuscript is well-written and follows a logical structure. I have a few comments which the authors should resolve before the manuscript can be published.

-The authors should consider using a more standard structure with for example Introduction, Methods (Sects.2-5), Results (Sects. 6-7), etc. In my opinion, this will improve the organization of the manuscript.
>> We have previously considered a more standard article structure. The current structure highlights the methodological aspect of developing and validating a coupled inversion framework by sectioning the text into the respective model development steps. We consider this to be one of the main contributions of the paper and would prefer to keep the current structure, if possible.

-L. 81: I suggest replacing «advice» by «yield»

>> Edited in the revised version of the manuscript.

-L. 144: This is a very restrictive assumption which severely limits the usability of the method. At the study site (i.e. where the ERT system is deployed), is the soil really fully saturated? In general, these limitations of the method should be evaluated in more detail in the Discussion section, also clarifying if they are principle limitations, or if they may be overcome by future work. See comment below!

>> At the study site in Greenland, the full saturation is a good assumption, certainly for most of the year except between mid-June and end of August (based on consistent in-situ unfrozen water content measurements over three years). Also, it is only the fully saturated period that is used for calibration in the presented work, so the assumption is not violated.

For the thermal model, as long as the saturation is known, it is straightforward to include it in the formulation. If the saturation was unknown but constant in time, then optimizing for saturation would correspond to adding just another parameter to the heat conduction (and the coupled) model.

So, the current restriction of the method is that the saturation has to be considered constant in time.

To include saturation in a realistic way, it should be included as a time-variable parameter. This could be done in one of two ways: by i) parameterizing saturation (in a way similar to the parameterization of *theta_w* by the two parameters *alpha* and *beta*), which would of course add additional parameters to calibrate; or by ii) adding a hydrological model driven by the same climatological parameters that would calculate saturation and inform the coupled thermal-resistivity model about the time-variable values of saturation. This way, there would be no additional parameters in the coupled model to calibrate.

-Sect. 6 and its subsections: I am not sure “heat model” is a good term, the authors could consider using “heat conduction model” instead.

>> We agree to use “heat conduction model”; alternatively, we have introduced the shorter expression “thermal model” where brevity was needed.

-Sect. 6.1: The reasoning of this section is not too clear for me, as the authors first seem to treat all model parameters equal, not making use of the knowledge that some are essentially constants, while others are not constrained at all. Why do the authors consider the heat capacities of water/ice and thermal conductivities of water/ice as potentially variable parameters? Why not use the accepted literature values for these, potentially including parametrizations for their temperature dependence? What is the expected freezing point at the study site, is it close to 0? Also the volumetric heat capacity of soil minerals should be in a fairly small range, other than the thermal conductivity which might vary by a factor of three? So why not use a literature value for the heat capacity, is the idea to account for the effect of soil organics? In the end (Table

2), exactly the parameters that are generally assumed constant in environmental modeling, are assumed constant also here, using pretty much the literature values (on a side note, why use Tf=-0.0001 degrees and not 0 - this shouldn't affect the model outcome at all?). So why not restrict the sensitivity analysis to the parameters which in reality are variable, and drop the constant ones from the sensitivity analysis?

>> We found it interesting to demonstrate the sensitivity of the model to all of its parameters (as per the heat conduction equation (1)), including the ones that are in practice not calibrated (constants), to showcase their individual importance for model simulations, for two main reasons:

1) Such constants would normally be excluded from a sensitivity analysis, while we found it interesting to show the effect of changing values of these parameters on the model calculation.
2) These 'constants' are, strictly speaking, not constant, but temperature- and salinity-dependent (specific heat capacity and thermal conductivity of ice and pore water). The assumption of a constant is a standard simplifying modeling assumption that we found justifiable in a study *not* focusing on the temperature- and salinity dependence of these thermal parameters, and designed following the principle of parsimony. Nevertheless, a known temperature and salinity dependence could easily be incorporated into the current model formulation, if the model application required it and justified the increased complexity.

-L. 243: it would be good to specify "arbitrary, but realistic" some more. Is it "randomly drawn within the limits of Table 2"?

>> Indeed, Table 2 presents a wide range of values found across literature, and the "arbitrary but realistic" values are the most common values we found for our type of materials, from the values within this table.

-L. 251: Write out MRC. Is this mentioned before?

>> MRC is a rigid thermistor string, we have replaced the abbreviation in the revised version of the manuscript.

-L. 284: "of the permitted…"?

>> Edited in the revised version of the manuscript.

-Sect. 6.4: It would be good to include a discussion of the physical meaning of the Cs-parameter and if that can help understand the relative insensitivity of modeled temperatures towards this parameter.

>> The relative insensitivity of the model to the Cs (heat capacity of soil grains) parameter is well-known from experimental and modeling studies, but we found the causes to be little discussed. Our reasoning departs from the knowledge that significant

phase change is necessary to separately estimate heat capacity and thermal conductivity from field data; in a system with little phase change, only thermal diffusivity can be estimated. In a saturated system, when the rate of phase change is the largest, the energy consumed by the phase transition between water and ice is much larger than the energy needed to change the temperature of soil grains, and the value of Cs becomes comparatively insignificant. Outside of the period of signification phase change, the phase change actually taking place is not enough to allow for separate calibration of the heat capacity of the soil grains. This discussion was included in the revised version of the manuscript.

-L. 297/Table 2: use unit MJ instead of e6

>> Edited in the revised version of the manuscript.

-L. 310/311: Can you rephrase this sentence, I'm not sure what starting model-dependent means.

>> It means that the final, optimized parameter values will be different based on the initial guess of the parameter value before optimization. Rephrased in the revised version of the manuscript.

-Fig. 5 and Fig. 8: add unit to RMSE

>> Edited in the revised version of the manuscript.

-L. 398: in Fig. 8

>> Edited in the revised version of the manuscript.

-Sect. 8 Discussion: In my opinion, the discussion is missing a detailed assessment of the state of the proposed method, how it would be used in practice, i.e. what type of ERT measurements one would need (e.g. repeat frequency), which soil parameters need to be measured independently to constrain the model, etc.

>> The repeat frequency would depend on the rate of phase change (which varies throughout the year) and the sensitivity of the geophysical method to these changes. During the fastest rate of phase change (notably in the thawing period), it is beneficial to collect geophysical data daily, when possible. When daily acquisitions are not feasible, as well as outside of the period of the fastest phase change, the acquisition frequency could be optimized by evaluating the effect of sampling frequency on the parameter recovery. A discussion point on the repeat frequency was added to the revised manuscript.

In terms of independent model parameters to be measured to constrain the model - these would depend on the sensitivity of another geophysical method and on the desired outcome of the model and would need to be tested.

Naturally, having a fix on an important parameter such as e.g. porosity (a single value throughout the soil column, or variable porosity with depth) would improve the estimation of the remaining parameters. This would make sense to do if the purpose of the model was to obtain as close to true parameter values as possible (without directly measuring them on soil samples). However, porosity is also one of the trickiest parameters to know, and impose. If the purpose of the modeling is to come up with a model that can reproduce the calibration and validation data well (within acceptable errors), this can be achieved even when porosity is not known and is one of the calibrated parameters.

Secondly, it would be nice to discuss to what extent the performance of the method at the Greenland site is specific to this site, or if at least a roughly similar performance can be expected also at other sites, where e.g. some of the ground parameters could be different (). While this is clearly a difficult point and definite answers cannot be given, the authors could use the results of their various sensitivity analyses together with general knowledge on ground parameters (e.g. thermal conductivity) and the ranges within which they vary.

>> Definite answers cannot be given without testing, but we can base our assessment on the knowledge of the behavior of the ground thermal parameters, as well as the results of our sensitivity analysis. A site where the ground consists of coarse-grained dry sediment would present a very different scenario to the saturated silty clay geology on which we developed and tested the coupled inversion method. The parameters that we would expect to differ the most would be porosity, saturation, and the freezing parameterization alpha and beta. Porosity, alpha and beta were among the most sensitive parameters of the thermal model. Saturation sensitivity was not evaluated in this study, yet as it is a property that directly controls the amount of conductive liquid phase, we would expect it to be a very sensitive parameter. Based on these considerations, we expect that the method would likely be useful at a site of different geology. Discussion on how we expect the method would work at a site of different geology was added to the revised manuscript.

The authors should also discuss the impact of the limitations stated in L.141 on the practical use of the method and if it is possible to improve/adapt the method to overcome at least some of these limitations.

>> *Heat conduction assumption*: Heat conduction is assumed to be the dominant process of heat transport. This is a reasonable assumption at our site where for most of the year (from beginning of September to mid-June), we do not observe substantial lateral water movement. Evaporation and increased pore water movement produce water content variations in the unfrozen period (mid-June to end-August), but this is outside of the calibration period (1. September -- 28. February). Accounting for the processes of water movement and evaporation would require their description and parameterization, which could be handled effectively outside of the coupled inversion

scheme. Including such modules would only be of interest if the goal was to carry out year-round ground temperature modeling

>> *Full saturation assumption:* discussed in a reply to an earlier comment.

>> *Homogeneous ground assumption:* The assumption of homogeneous ground is indeed a simplification at our site. Heterogeneities are present, namely in the form of ice lenses (mainly in the depth between 0.9 - 1.5 m), and increasing pore water salinity (in the depth below4m). We implemented a heterogeneous model with four layers corresponding to the field situation (active layer, ice-rich permafrost with ice lenses, saline permafrost and bedrock). The performance of the heterogeneous model was comparable to the homogeneous model in terms of the RMSE, and different sets of parameters were identified for each of the four layers. However, the parameters were non-uniquely determined, as different sets of starting values converged to different optimized parameter values, all fitting the field data similarly well. We concluded that without further constraining information, the use of the more complex model was not justified, neither in terms of the model fit, speed of convergence, or the uniqueness of parameter estimation.

>> *Constant specific thermal properties and latent heat:* Specific thermal properties of the soil constituents were assumed constant, i.e. independent of the temperature or salinity. This is an acceptable approximation, as using constant parameters resulted in errors of less than 10% in the calculation of the effective (bulk) thermal properties in the temperature range between-20 - 0 degrees C. Latent heat of phase change varies with unfrozen water content, but using a constant value has been proved satisfactory for temperatures above \SI{-20}{\celsius} \citep{anderson1973unfrozen}. These are standard assumptions used in this type of modeling, and were previously successfully used in other (cited) studies. Temperature- or salinity-dependent variation could be readily implemented if the requirements on model output and quality of input data justified the increased model complexity.

>> *Fixed temperature as the bottom boundary*: A fixed temperature was used as the bottom model boundary. This assumption has no impact on our calculations considering the short modeled time span (up to 180 days), and the known yearly temperature amplitude at the bottom of a 6m deep borehole (<0.09 degree). If the model was to be used for future predictions, this assumption would have to be reviewed, just as in any model expanding its domain of application. Including variable bottom temperature or geothermal heat flux boundaries is technically straightforward in the current model setup. This would however require extra data input in the form of borehole temperature measurements (for the variable bottom boundary) or a reasonable estimate of the geothermal heat flux at the site (from a deep borehole, or from a regional geothermal heat flux model accounting for variations).

This discussion of all six model assumptions was added to the revised version of the manuscript.