From: *Soňa Tomaškovičová (corresponding author), Thomas Ingeman-Nielsen*

To: *Editorial Office, The Cryosphere*

In Kongens Lyngby, DK, 03. August 2023

Subject: Response to the second round of reviews. Upload of 2nd revision (3rd version) of the manuscript.

Dear Editor and Reviewers,

Thank you for your suggestions on how to improve our submission of the manuscript "*Coupled thermo-geophysical inversion for permafrost monitoring*". We are hereby submitting the 2nd revision of the manuscript, where we implemented the second round of changes. In addition to replies to reviewers' comments listed on the following pages, we have:

- rephrased the Abstract for conciseness and clarity.
- replaced the reference to PhD thesis (Tomaškovičová 2018) with a reference to a just published article (Tomaškovičová & Ingeman-Nielsen, 2023) "*Quantification of freeze–thaw hysteresis of unfrozen water content and electrical resistivity from time lapse measurements in the active layer and permafrost*" (doi: https://doi.org/10.1002/ppp.2201). This changed/updated reference was highlighted in red and used whenever we refer to the field site description, to the thermal, resistivity or water content regime at the site, and to the challenges with inverting resistivity acquisitions from permafrost settings.

Changes in the manuscript (compared to the 1st revision) are marked with red text.

We hope that you will find our updated contribution suitable for publication in The Cryosphere.

Respectfully,

Tomaškovičová

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Response to reviewers

>> We thank the reviewers for repeatedly reviewing the manuscript and for providing further comments to help us improve it. Our replies are indicated in ">>italics".

Reviewer 1

Dear authors,

thank you very much for addressing my and the other reviewer's comments and for providing detailed responses to our suggestions. The extended discussion really benefits the paper, and it is much clearer now why you chose to use the resistivity, and also what the current limitations are and how they may be addressed in the future. While I think that the paper can be accepted now, I just want to come back to one of my previous comments.

In lines 64 to 67 you state:

"The relationship translating a certain ground electrical composition into apparent resistivity is unique, and governed by equations for conservation of charge, Ohm's law and the geometry of electrode configuration used to collect the resistivity data. Conversely, any inverted resistivity model is only one of a large number of possible realizations that explains the measured apparent resistivity data acceptably well"

While I fully agree that working with apparent resistivities is beneficial (e.g., avoiding inversion constraints, dealing with non-uniqueness, etc.), I still find these two sentences misleading. The way you state the first sentence is of course correct. You can use the results of your thermal model to create a 1D subsurface representation from which you can calculate a unique resistivity response. But as for the inversion of apparent resistivity, where an infinite number of subsurface models will be able to explain your measured apparent resistivities, an infinite number of thermal parameters will be able to explain your measured apparent resistivities. With these two sentences, for someone who is not familiar with resistivity measurements, it sounds though as if there is a unique solution to your optimization problem. This then also feeds into the problem of spatial heterogeneity. In your model, you solve for a 1D subsurface, with no spatial only vertical variation in geophysical and thermal properties. And even if your set of apparent resistivity measurements will be sensitive to spatial heterogeneity, you are not addressing this in your model (or at least I don't see how). This may not be a problem at your field site, but, e.g., when working on polygonal ground, where subsurface parameters can vary at small spatial scales, it would certainly affect your apparent resistivity measurements, and thus may lead to errors in your coupled inversion. Of course, this cannot be addressed in a 1D model and is certainly outside the scope of this paper, but it might be something to keep in mind.

-> We have added a clarifying statement to make this more obvious, in lines 72-75.

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Reviewer 2

In general, the authors have provided convincing answers to my comments. However, the only major change to the manuscript is the much extended discussion, while the rest is more or less unchanged. Wherever applicable, the authors should incorporate the replies in the other parts of the manuscript as well (at least if the point is not taken up in the revised discussion, in this case please provide a reference to the discussion section where the original comment was raised). An example is my comment on Sect. 6.1. I have no problem with the explanations provided in the replies, but when reading the manuscript without these explanations in mind, I still have the same problem to understand the logic behind the setup. So please incorporate the replies in the manuscript!

>> We have tried to incorporate the points from the discussion throughout the manuscript where relevant:

- *in the expanded description of the heat model assumptions (section 4, lines 151-171)*
- *in section 6.1, lines 220-227.*

Minor points:

L.208: use the greek symbol for lambda, not "lambda"

>> Corrected.

Table 1: Please explain for each of the parameter how the value (or the range) was selected.

>> Explanation added in lines 261-265.

L. 423: I don't understand how saturation could be "parameterized" (point i) if it changes in time. What would be the input parameters for this parameterization, how can the soil water content respond to precipitation, evaporation, etc.

>> Parameterization would require knowledge of the soil moisture regime on the site. In our case, in the frozen period, for T < Tf: S=1. In the thawed period, saturation dependence would have to be investigated, e.g. it is known respond to two main forcing regimes: rainfall-driven wetting or radiation-driven drying.

L. 425: I don't agree that no additional parameter would need to be optimized when a hydrological model is added. In fact, a hydrological model would likely have multiple unknown parameters which strongly influence the soil water content and which would need to be optimized in the inversion unless they are known from other studies. Examples would be the hydraulic conductivity (which e.g. determines how fast water infiltrates vertically and drains laterally), the water holding capacity of the soil or soil hydraulic parameters (if for example Brooks-Corey or Mulaem-van Genuchten models for matric potentials are used).

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-> We completely agree that the hydrological model would need its own parameterization. The idea is that no additional parameter would be optimized within the coupled thermo-geophysical inversion framework, i.e. the hydrological model would be a separately calibrated model driven by climate variables. This would obviously greatly increase demands on input data amount and quality, and would only be justified if adequate data quality and hydrological model were available, and if a part of the year with variable saturation was used.

L. 447: If the soil is saline, the entire model for soil freezing needs to be adapted to account for the freezing point depression and changes to the soil freeze curve. This should be clarified. The fact that the soil thermal parameters do not vary strongly with salinity is a minor point compared to this.

>> Completely agree. At the same time, the focus of this paragraph is justifying the use of constant specific thermal parameters, rather than explaining how salinity should be incorporated. We have however included a sentence about the type of change necessary if the salinity was to be included (line 485-486).

L. 475: I don't understand this point, the daily temperature fluctuations are real, so they shouldn't influence the results of the algorithm. Is this mainly a computation problem, i.e. the model has a longer computation time if it needs to resolve the daily temperature fluctuations? In this case, it should be no problem to use daily averages of the ground surface temperature to drive the model. Or is it hard to resolve the effects of the surface layer which can have very different properties from the ground below (e.g. organic surface layers)?

>> According to Figure 5, the thermal model has shown to smoothen the temperature variation compared to the in-situ measurements. Use of daily averages could be a solution, however it should be tested how their use impacts the modeling results.

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