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The Cryosphere Discussions

Interactive comment on "Estimation of soil properties by coupled inversion of electrical resistance, temperature, and moisture content data" *by* Elchin E. Jafarov et al.

Anonymous Referee #1

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Summary

Developing a process-based understanding of permafrost thaw is an important research topic and in line with the scope of The Cryosphere (TC). The authors present an estimation of subsurface porosity and thermal conductivity during permafrost thaw based on a fully-coupled inversion of soil moisture, temperature, and electrical resistance data. While the first two quantities are provided by the physical model directly, the latter is achieved by linking temperature, liquid water saturation, and porosity to electrical resistivity for a subsequent process-based forward calculation. The parameter estimation framework is demonstrated on a synthetic case in analogy to a field

site near Barrow, Alaska. While this topic is definitely of interest for the readership of TC and should be considered for publication, the current manuscript has several flaws, which I detail in the general and more specific comments below. I hope that the authors take this feedback in the positive spirit intended during revision of this otherwise promising paper.

General comments

- Language: I feel that the writing in general should be more precise. For example, starting with the title, the authors refer to estimating "subsurface properties" several times. Porosity and thermal conductivity of peat and mineral layers are mentioned in line 48, but in a more general statement. Only in section 2 and Figure 1, the reader is finally introduced to the actual inversion parameters. These should be clearly stated much earlier (abstract, introduction, or even better, title). The unspecific term "subsurface parameters/properties" also claims generality where it is not appropriate, i.e. line 470: "The results of this study show that estimating subsurface properties even for a synthetic setup can be quite complicated.".

- Petrophysical coupling: This is still not clear to me. The authors state that they use the approach of Tran et al. (2017), where "subsurface temperature, liquid water content and ice content from CLM model were explicitly linked to soil electrical resistivities via petrophysical relationships", but there is no ice content in eq. 2. Instead, there is porosity, which in turn does not appear in Fig. 1 as an input to the third box. Porosities are estimated and hence updated during each iteration, are they not updated in eq. 2? Please clarify. The coupling is essential for the paper and should be clear to the reader without consulting additional literature.

- ERT layout and modeling: Essential information is missing in the manuscript: How many electrodes were used? What was the spacing? What type of measurement configuration (Dipole-Dipole, Wenner, Schlumberger, etc.) was used? How many measurements were made in total? Please clarify and consider adding the electrode loca-

tions to Fig. 2. Was the mesh in Fig. 2 also used for electrical forward modeling? How did the authors treat the fact that electrical fields diffuse much further than the parameter domain provided by the ATS simulations? Did the authors extrapolate to a larger mesh? The final recommendation on adding additional repeated ERT measurements strongly depends on the acquisition parameters, which are not discussed in the text.

- Objective function: The authors invert resistances. Why not follow the common practice and invert apparent resistivities instead? Resistances are not corrected for the geometry of the individual measurement, hence short-distance measurements (with higher sensitivity in the active layer) have less influence on the parameter estimation than measurements with larger layouts/geometric factors. The use of resistances could be justified with a more realistic error model, but w_r is set to 1 for all measurements, so the parameter estimation is dominated by measurements with a larger geometric factors, which would be more susceptible to noise in reality.

- Parameter estimation runs are performed with different starting models. While I think that this is a good idea, I do not agree that this is an appropriate measure of uncertainty, but rather of stability and suitability of the used inversion algorithm. If the Levenberg-Marquardt algorithm employed in this study gets trapped in local minima, one might raise the question if it is an appropriate approach? PEST provides some nice functionalities to study the sensitivity and resolving capacity of parameters. The manuscript could really benefit from a more detailed exploitation of sensitivities.

- Conclusions: The authors conclude that using more data as well as various types of observational data sets, and limiting the parameter ranges improves the parameter estimation. None of these conclusions are new to readers familiar with non-linear inverse problems. The conclusions should be more formulated more specifically to the present study and its implications for studying permafrost thaw.

Specific comments

- Fig 1: From the figure alone, one would draw the conclusion that only resistance

values are input to the objective function. Consider making a separate branch for the ERT part and adding an arrow from ATS (T, s) to the objective function.

- Fig. 7 & 8: The light colored numbers on top of the light colored matrix entries are not legible.

- Introducing (three) abbreviations already in the abstract is not necessary. In addition, there are some abbreviations (PCF, LSM), which are only used once and could thus be easily replaced by the full term.

- L26: Which soil properties?

- L30: Which specified subsurface parameters?

- L31: Which parameters?

- L34: Predictions of what?

- L64: Sorry for repeating myself, but "Nicolsky et al. (2007, 2009) used an optimization based inverse method [...] to estimate soil properties." is not very meaningful, i.e. the reader is left wondering: What type of observations were used? Which parameters were actually estimated? What was the outcome? How does it relate to the present study? I appreciate that additional information is given at the end of the paragraph, but in general too many statements in the text are too unspecific, which complicates reading.

- L66: Is "data calibration" different from "optimization based inverse method" and "parameter estimation"? The authors should strive to use a consistent wording.

- L93: Rephrase to "... matching multiple types of measurements to their model responses". Otherwise it is a bit misleading, as one could think that the different types of measurements are tried to be matched.

- L102-104: This is somewhat redundant to the statement in L99-101.

- L115: Replace by "etc." by something more specific. From equation 2, I assume that the porosity estimated in one iteration is also used in the petrophysical relation to calculate the corresponding electrical response. But this is not clear from Figure 1 and when reading the text.

- L228 and throughout the text: A "profile" is commonly used for a spatial location of measurements. Adding more profiles would certainly help, but I understand the authors take the exact same profile and propose more frequent measurements over time. Consider using a "measurements" instead of "profiles" to make this clear.

- L262: From what distribution were these random samples drawn? Did you also perform the parameter estimation with a homogeneous starting models, i.e. no differentiation between mineral and peat layer? This would be interesting.

- L273-274: Let the reader know how you came to this conclusion.

- Suggestion: I think that Figures 4 and 5 would be great in combination, e.g. the cost function as in Fig. 5 as an image (gray-scaled) in the background overlain by parameter trajectories on top as in Fig. 4. The same holds for Fig. 10 left and right.

- L296: Normalized to what? Maximum value? Please specify.

- L306: Why would one neglect the "outlier"? I thought the whole point of different starting models is an assessment of robustness? One out of five cannot be simply neglected.

- L417: "at least ten"? Why are there only five in Fig. 6 then? Please clarify.

- L424-426: I do not feel that the term "accuracy" is appropriate here. The "data accuracy" does certainly not depend on the regularization technique. Please choose a different wording.

- L426: "The regularization techniques". I suggest to leave out the "The" to be more general.

- References: Please use a consistent formatting of citations (e.g., sometimes the year appears at the end, sometimes after the authors, sometimes in brackets, doi: vs. DOI: vs. https://doi.org/).

Technical corrections

- L76: "soil electrical resistances" -> "soil electrical resistivities"

- L100: "total number measurements" -> "the total number of measurements"

- L189: Remove "as if the synthetic truth is unknown".

- L365: Remove "by"

- L396: "matching" -> "estimating" (to differentiate between data and model space)

- L427: "Its" -> "Their"

- L454: Replace "confuse"

- L462: "data" -> "parameters"

References

Tran, A. P., Dafflon, B., and Hubbard, S. S.: Coupled land surface–subsurface hydrogeophysical inverse modeling to estimate soil organic carbon content and explore associated hydrological and thermal dynamics in the Arctic tundra, The Cryosphere, 11, 2089-2109, https://doi.org/10.5194/tc-11-2089-2017, 2017.

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