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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 #2

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In the paper "Estimation of soil properties by coupled inversion of electrical resistance, temperature, and moisture content data" by Jafarov et al. the authors develop and explore a framework for estimation of soil properties (porosies and thermal conductivities) based on an optimization framework using Electrical Resistivity Tomography data and in-situ embedded sensor information. The paper is based on purely synthetic experiments, and aim to explore the types and frequency of data needed for good parameter estimation. While I find the topic of the paper very interesting and original, I also find that the paper has not been prepared with the attention to scientific quality and rigour that is to be expected from a paper published in The Cryosphere. I have outlined my concerns in the general and specific comments below, and hope that these comments

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Discussion paper

may help the authors prepare a revised version of the paper.

General comments:

The paper suffers somewhat from incosistent use of standard terms: Active Layer Thickness (ALT) is a well established term referring to the maximum thickness of the active layer (distance from surface to frost table), which is usually observed at the end of summer. The term is used inconsistently by the authors to refer to the "active layer" e.g. (L129), and the "frost table" (L147), and is used in several places to describe relative positions below the active layer ("below the ALT") where simply stating e.g. "below the active layer" or "in the permafrost" would be more clear to the reader. I would suggest that the authors implement the standard notation using the greek letter "rho" for resistivity and capital R for resistance to avoid confusion. (and that they use "rho_a" for apparent resistivity should they choose to change the representation of the ERT data in the cost function). For ERT measurements, the term "profile" is usually used to indicate the physical location of ERT measurements. The authors, however seem to use it to represent time slices of measurements along the same profile - which is confusing at first. It would help the reader if the authors implemented a more standard terminology using "profile" to refer to the physical location, and e.g. "data set" or "time slice" to refer to individual sets of measurements along the (single?) profile.

The petrophysical relationships used are essential for understanding the implementation and should be presented in the paper. One such relationship is given in equation 1, but no specific reference is provided for this relationship (although Tran 2017 is mentioned later as a general reference). It is unclear whether the soil is considered constantly fully saturated, or whether variation in saturation is modelled. It is unclear which component of thermal conductivity is optimized for (saturated frozen, saturated unfrozen, soil grain, ...?). k_m and k_p are mentioned (P15 L323-324) for mineral soil and peat. They are obviously single valued (no time variation), so what thermal conductivity component do they represent? What about thermal conductivity of water, ice and air - are they considered temperature invariant? What form of freezing characteristic curve has been chosen for the different soil types?

A critical problem of the paper is its faliure to describe the layout and measurement configurations of the ERT measurements. No mention is given to electrode locations and separation, and no discussion of measurement configurations is presented (4-electrode configurations I assume: wenner, gradient, schlumberger,...?). Furthermore, no discussion is presented of the sensitivity of the configurations used, and it is therefore unknown to the reader to which extent the layouts provide information within the domain of interest.

The data input to the optimization is not well described in terms of timeseries measurement frequency. Measurement frequency of temperature and liquid water saturation is not mentioned at all... are synthetic data used for a full year? 8 months like in some cases for the ERT? At what measurement frequency? ERT data sampling is confusingly described. It seems that the authors consider 3 situations in terms of ERT measurement frequency (P10 L227-229 and 243-245): A) One ERT data set (time of year is not specified) B) 8 ERT data sets, measured along the same profile, one data set per month for 8 months from January to August. C) 8 ERT datasets, measured along the same profile, one data set each day for 8 consecutive days (time of year is not specified) However, later a fourth case seem to be mentioned (P13 L304-305) with 8 monthly profiles (meaning 8 profiles collected every month? Equally spaced in time? Still for 8 months? so 64 data sets in total?). This must be much more clearly presented, and an explanation of the choice of timing (time of year) in the different cases must be given. This is very important as it will strongly influence the infomation contained in the resistivity data set.

It should be made more clear what data are used in the cost function calculations. I suggest the authors add and explain indices to the summations in the cost function, to represent both sensor locations / electrode configurations, and timing / time slices - to make it clear to the reader, how the cost function input is composed. For the temperature and liquid water content data, the positional index would be e.g. i=1..10

(representing the up to 10 locations indicated in figure 2) and a temporal index j=1...XX, would represent the number of observations included for each point. Similarly for the ERT data, the positional index would correspond to the individual 4-electrode measurement configurations used, i=1...YY, and the temporal index to the included data sets, e.g. j=1 for experiments with only one dataset, and j=1...8 for experiments with 8 data sets, etc.

The authors should explain why they chose to invert resistance values rather than apparent resistivities, as is standard practice. The measured resistances depend not only on the subsurface properties, but also strongly depend on the geometry of the electrode configuration used for the measurements. Furthermore, the authors have not chosen to linearize the problem, which is often done by applying a logarithmic transform to the ERT data. If this reflect active choices by the authors they should explain their considerations in the paper. If it does not, the authors should rethink their optimization strategy.

For the final analysis, the authors try to consolidate the the results of the RMSE tables (figure 8) using normalized measures to ease comparison. This is a good idea, however, I find the choice of normalization is unfortunate. The authors choose to normalize the mean RMSE by the maximum RMSE value obtained. The maximum RMSE corresponds to the worst fit obtained, and this value depend on the shape of the optimization surface, as well as the choice of intial values chosen by the operator for the parameter in a particular optimization experiment. The normalization basis is thus influenced by operator choice, and would be quite sensitive to outliers. I suggest the authors try to identify a different and more robust basis of noralization. I also do not understand how the authors calculate the "RMSE of the normalized mean values" (P17 L352), which are used in the clustering analysis. Please provide more detail or equations.

When these issues have been resolved, the discussion and conclusions should be reevaluated.

Minor and/or specific comments:

I suggest the authors complete their description of model initialization by also specifying how the model domain is initialized in terms of initial temperature distribution (only the lower boundary fixed temperature is currently presented) and initial water saturation of the domain. It would also benefit the reader to know if the authors have conducted some sort of spin-up, to ensure that the model domain is in equilibrium with the forcing data.

The authors define several abbreviations, some of which are used only few times. Please consider spelling out where abbreviation is not strictly necessary.

P5 Figure 1: Please expand the figure caption, and explain the parameters presented in the figure.

P5 L123: if this is a transect through an ice-wedge polygon site, why are the ice wedges not represented in the model?

L285-287: "The close proximity ...": very vague statement, please revise and elaborate. What is k_m?

P11 L262: If each experiment was repeated 30 times, why does figure 6 show only 5 samples? and how were these samples selected from the full set? How many repetitions were used for the cases forced by observed meteorological data?

P12/13 figure 4&5: Which experiment does these figures represent? Why do the axes extend to physically meaningless (negative) values? Why are the axes limits in figure 5 cropped to exclude some of the minima found in the optimization (around k=1.4 W/(m.K) for mineral soil). The contour interval chosen for figure 5 is insufficient to visualize the optimization path leading to the outlier solutions.

P14 figure 6: Plots should be labeled with the full experiment designation, (S)6T6sl1r, (S)6T6sl1r+n and (S)6T6sl8r+n. Again, the plot axes extend to physically meaningless values.

Figures 7 and 8: The text in the tables and table titles is not readable P17 L365-366: "... increasing the number of measurement locations, leads to more accurate parameter estimation". I don't believe you have actually documented wether it is the number of measurement points, or the choice of location of the measurement points that cause the improvement. What would happen if you still used 6 measurement locations, but distributed diferently (some deeper locations)?

P18 L389-391: "In reality, the depth of the mineral soil..." this statement is unclear, please revise.

P19 L397: misprint in matrix table reference, and wrong figure reference.

P21 L358-359: Again, you have not tested whether it is the location or number of observation points that matter.

P21 L475-476: "It is important to note that..." Unclear, please revise. Are you still referring to the ERT data?

P23 Figure 9: xlabel should read "Estimated properties accuracy" or similar.

P24 Figure 10: Axes extend to physically meaningless values.

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