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The Cryosphere Discussions

Interactive comment on "Brief communication: Estimation of hydraulic properties of active layers using ground-penetrating radar (GPR) and 2D inverse hydrological modeling" *by* Xicai Pan et al.

Anonymous Referee #1

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General Comments

The authors analyze whether it is possible to identify soil hydraulic properties of the active layer in a permafrost region by inverse modeling using the Richards equation in two spatial dimensions. The content covers the scope of the journal, the study is conducted well in technical terms, and the manuscript is structured well and written in an acceptable style. From the point of view of inverse modeling in vadose zone hydrology, the study does thus not offer many new insights and the outcome is not surprising to me. An innovative feature is the investigation of the effect of the amplitude of the undulating frozen layer and its influence on parameter estimation. However, the

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Discussion paper

results of this are, again, overall not surprising. My main criticism can be summarized in two points:

1. The study uses only computer-generated data and assumes that the model is a perfect representation of the system. The impact of model error on the results is not investigated. Such model error could be caused by wrongly parameterized hydraulic properties or imperfect knowledge about boundary condition, initial conditions, and structural features of the soil. If the flow model is correct, the soil is homogeneous and data is only contaminated with independently, normally distributed noise of equal variance, the soil hydraulic properties can of course be identified under transient conditions and this is not worth reporting. In reality, these conditions will never be fulfilled in a field situation and the conclusion of the authors that their method can be applied for field data is thus not fully supported.
2. The study focuses only on the accuracy of the identified hydraulic properties, i.e. on the question how well the identified properties match the true ones. However, the aspect of precision or uncertainty is not treated well. I appreciate that the authors tested 10 different realizations of random error as stated on page 5 (top) and shown in Figure 5. Such a bootstrap is well-suited for quantifying uncertainties, but a bootstrap using only ten bootstrap samples cannot lead to a robust quantification of uncertainty.

I think it is absolutely necessary to analyze the influence of deviations from the almost perfect conditions assumed throughout the analysis and to improve the statistical quantification of uncertainties. Therefore, the authors should include the following aspects before publication:

1. Studies on the effect of more complex errors on the accuracy of the identified hydraulic properties (most importantly model error, but autocorrelated error is also an interesting aspect)

2. A more rigorous quantification of parameter uncertainty and parameter cross-correlation to delineate under which settings the unique identification of soil hydraulic properties of an active layer is possible.

3. A study using real GPR measurements to illustrate the performance of the proposed method in a real situation and to critically assess its potential and deficits.

Specific Comments

[General] An important point is whether the undulating structure of the frozen layer is also identified by the radar measurements or whether it is assumed to be known exactly, i.e. without error. In reality, it will be unknown and may deviate from the perfect shape assumed in this study. As a result, the soil hydraulic properties and the depths of the active layer as function of the horizontal variable must be identified jointly. This has not been investigated so far.

[P1 L18] “The proposed method depends on the lateral water distribution . . .” – what do you mean? In which sense does the method depend on it? Do you refer to applicability, accuracy, general results? Please be more precise.

[P2 L16] “Normally, the inverse method using in-situ 1D monitoring profile yields accurate data in depth, but it is expensive to apply to larger spatial scales.” – what do you refer to exactly when you write “in-situ 1D monitoring profile”? Why is a 1D-method “expensive to apply at larger scales”? I don’t understand what you mean, please clarify.

I do not understand why the hydraulic properties of the frozen layer are obtained by Miller-Miller scaling of the soil properties of the active layer (P3 L20; P4 L24). No justification is given for this. Why do you assume water flow in the frozen layer? I would assume that a frozen soil is impervious. Is it possible to describe water flow in frozen soil with the Richards equation? Please mention the assumptions you make here and justify your approach.

[P3 L22] The authors use a Dirichlet condition at the top but do not mention the pressure head. I think that a flux boundary condition defined by the precipitation rate would be easier-to-implement and physically more realistic. Please justify the use of the Dirichlet condition and provide the pressure head value used in the simulations.

I miss information on the initial condition used in the numerical simulations (section 2.1). This is highly relevant for step 1 of the inverse procedure because the hydraulic properties are estimated using the assumption of a hydrostatic pressure distribution at the beginning. If a hydrostatic pressure head distribution was used as initial condition, step 1 becomes a trivial exercise, because the assumption of a hydrostatic pressure distribution made in step 1 is fulfilled. As a consequence, the results shown in the left three panels of figure 4 are not surprising.

Why do the authors use fminsearch for step 1 and Levenberg-Marquardt (LM) for step 2? I think LM is more efficient for step 1 than fminsearch which uses the Nelder-Mead-Simplex algorithm(NMS). The authors should mention the specific algorithm which fminsearch uses. The statement "As the Levenberg-Marquardt Algorithm is a gradient-based optimization method, it relies on good initial starting points of parameters." is misleading. The reason why LM needs good starting values is that it has only local convergence properties. The same holds for the NMS but this is not stated explicitly in the manuscript.

The authors state that they used "50 ensemble inversions" [P4 L10 L18]. I think the term ensemble is an exaggeration in this context. If I understand correctly what the authors did, they used different starting values for the model parameter Ks in the numerical minimization of the objective function and finally selected the one with the smallest value of the objective function. I would call this multistart LM minimization but not an ensemble inversion. The term ensemble is used in model averaging or ensemble Kalman filtering but these techniques are much more sophisticated compared to what the authors did. Neither do I understand the statistical background to show the best 34 functions in Figure 4. The optimization with the smallest value of the objective function is the maximum-likelihood-estimate and this is explicitly stated by the authors (P5 L15). But why would one include the next 33 results in the Figure? What is the statistical justification for this?

[P5 L 12] "Results from three panels shows the order of the estimates for step 1 and step2 are S1 < S2 < S3." – I do not understand what you mean with "S1 < S2 < S3". Do you mean that S3 is better than S2 than S1? How was this assessed? By the difference between the theoretical and identified hydraulic functions? If so, state it and provide some quantitative measure of goodness-of-fit, for instance root-mean-squared-error. I think such a statement on accuracy of the estimates must be complemented by a statement on the precision / uncertainty of the identified system properties. Such information can be based on the data shown in Figure 5, but the number of bootstrap samples is too small for statistical inference.

[Figure 5] Why are the results of the first step of the inverse method shown in Fig. 5? I thought that step 1 was used to obtain good initial estimates of the parameters for inversion steps 2 and 3. If this is correct, I don't see any reason to include the results of step 1 in Figure 5.

[P6 L12] "This method depends on the magnitude of lateral water redistribution, which is controlled by the undulating frost table, by the soil hydraulic properties and by the intensity and duration of the precipitation." – is this really a conclusion of your analysis? You have not varied rain intensity. Neither have you analyzed soil textures other than sandy. The only thing you have analyzed is the amplitude of the undulating frost table.

Technical Corrections

[P1 L13] “Provided an active layer with an undulating frost table, monitoring of spatial soil water dynamics” – incomplete sentence, please rephrase

[P1 L26] “Permafrost models” – consider to give a few examples and provide references or refer to a review article on such models.

[P1 L27] “due to the low spatial resolution soil information like hydraulic properties and architecture” – incomplete sentence, please rephrase. What do you mean exactly by architecture, structural features? Please rephrase.

[P2 L2] “they are normally estimated based on literature data” . I think the estimation is mostly based not only on literature data but additionally on texture information and empirical models. Please consider to rephrase.

[P2 L3] “Thus, knowledge of the soil hydraulic properties” – I don’t think that this follows from the preceding sentence. Maybe you mean: “Thus, a site-specific determination of hydraulic properties is essential for permafrost modeling”.

[P2 L18] “yield certain results” – of course they yield some results, but what do you mean? Do you mean “results of only limited accuracy” or results which are “only partly representative of the subsoil physical properties?” Please rephrase.

[P2 L23] “In order to yield good results, . . .” – the term “good” is not very specific, what do you mean, reliable, robust, accurate, . . .?

[P2 L29] “spatial-temporal” –> “spatiotemporal”

[P2 L33] “Provided significant lateral water redistribution induced by an undulating frost table in active layers and spatial-temporal GPR observations, efficiently estimating effective hydraulic properties could be viable” – is this sentence complete? Consider to rephrase.

[P3 Eq 1] The Richards equation is slightly wrong. The term “-1” is a scalar and thus cannot be added to the vector h in the square brackets.

[P3 L10] rephrase to “A widely applied model for these two relationships is the van Genuchten-Mualem model” (singular, not plural)

[P3 14] Provide units for the van Genuchten parameters in the text.

[P3 L20] “are approximated similar using” – please rephrase

[P4 L15] The authors provide a reference for the MuPhi solver by Ippisch but this reference is a bit misleading because the article by Ippisch et al. (2006, AWR) deals with a correction of van Genuchten-Mualem model close to saturation. Is it possible to give a more direct reference to the code? This would help the reader to access it.

[Fig 1] In the legend in the bottom plot (c), units are missing.

[P5 L18] “for rain-based cases” – please rephrase, it does not become clear what you mean.

[P6 L11] “The reasonable accuracy of the estimated parameters is as expected for the studied cases” – good that you have expected these results. But this is not a scientific statement. Would everyone expect them and if so: are they worth reporting? Please remove this statement and replace it.

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