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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 #2

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GENERAL COMMENTS

The manuscript describes a synthetic case study, in which a two-dimensional numerical model is used to estimate soil hydraulic parameters in idealized cross sections consisting of a permeable surface layer underlain by an impermeable bottom layer having undulating surface. Texts are generally well organized and written, and modelling approaches are technically sound. However, I am not convinced of the relevance and usefulness of the present manuscript in cryospheric science. It is an interesting numerical exercise, but the manuscript can be made much stronger and interesting to the readership of The Cryosphere. I will list my suggestions in specific comments

below.

SPECIFIC COMMENTS

Title. The study is motivated by GPR applications in the active layer, but the current version of the manuscript presents nothing specific to GPR or the active layer. For example, it does not deal with uncertainties and non-uniqueness in the relationship between dielectric permittivity, which is estimated by GPR, and volumetric water content. The model uses a two-layer structure consisting of permeable and impermeable soils separated by an undulating boundary, which is not specific to the active layer. The permeability of frozen soil is controlled by temperature, but the model does not account for coupled heat-energy transfer processes. Therefore, I think that the current title is somewhat misleading. It will be much better if the authors develop the paper to something that truly describes what is in the title.

P3, L7. This form of the Richards equation is incorrect. The gravity term should have a unit vector, not a scalar "1". Also, please define the direction of z-axis.

P3, L22. What value was used for the Dirichlet upper boundary condition? How was it determined?

P3, L25. GPR measures the travel time and amplitude of reflected radar waves, not the amount of soil water storage. The estimation of soil water storage from GPR data is not straight forward and has a large degree of uncertainty. To make this study relevant to The Cryosphere, it is highly desirable to incorporate uncertainties and non-uniqueness in GPR signal interpretation into numerical inversion. I believe that there is an established body of literature on this subject matter.

P3, L27 - P4, L2. I do not understand this sentence. Please rephrase.

P4, L2-3. It is assumed that the water table (i.e. matric potential = 0) is at the lower boundary. Does the lower boundary refer to the boundary between thawed and frozen soil? If so, does this "static condition" make sense hydrologically? For example, what is the condition at time step 7 in Figure 1? Should that be a more logical representative of the static condition after the complete drainage of the active layer?

P4, L5. It appears that a homogeneous soil is used in the model. It is well known that the near surface soil in natural environments is highly heterogeneous both vertically and horizontally. This severely limits the usefulness of the proposed approach to determining soil hydraulic property. I see this as a major weakness of this manuscript. It can be made much stronger by explicitly treating soil heterogeneity in numerical inversion.

Figure 1. It appears that a constant flux was applied to the upper model boundary, whereas the method section states that the upper boundary had a Dirichlet condition (P3, L22). What was the actual boundary condition?

Table 1. The alpha values should be positive. The pore-size distribution coefficient (n) has a high value, and the residual water content is zero. I would say this is rather an unusual soil. Is this a good representative of typical soils in natural environments? Was this unusual soil purposely chosen for the synthetic case study? Why?

P5, L27-28. As the authors acknowledge, the water distribution over an irregular frost table is inherently three-dimensional. Two-dimensional models provide a useful tool for theoretical discussion, but its utility for practical application is limited. In addition, soil heterogeneity and a high degree of uncertainty in GPR data interpretation makes the present approach impractical to use in active-layer studies in natural environments. I suggest that the authors develop a full-length paper describing the development and application of a more realistic and useful inversion model using actual field examples of GPR data.

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