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Discussions

Interactive comment on "Voxel inversion of airborne electromagnetic data for improved groundwater model construction and prediction accuracy" *by* N. K. Christensen et al.

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We would like to thank Anonymous Referee #2 for his valuable and relevant comments. Our replies are found below.

General comments 1. This paper uses geophysical "voxel inversion" to do resistivity field estimation, and linked the resistivity field with hydraulic conductivity field through power law. Those methods are already proposed and utilized in the past. Please highlight the new theoretical development and findings.

This paper is the first to demonstrate application of voxel inversion results directly in a groundwater modeling context. Furthermore, it presents and demonstrates a novel

parameterization method for a groundwater model for which the calibration is supported by the 3D geophysical voxel model. Finally, it demonstrates the importance of choosing a geologically plausible regularization when the geophysical model is to be used in a groundwater modeling context. Furthermore, it should also be pointed out that previous studies (linking resistivity field with hydraulic conductivity field through power law) cited deals with interpretation of tomographic data that provide a high degree of resolution, thereby allowing for interpretation of spatial variability in petrophysical relationships. In large scale applications (ten to thousands of square kilometers), this type of data is generally not available at this scale.

General comments 2. In the numerical part, all the simulations are done with predefined true/reference model without the realistic field data. It will be better to prove the idea with realistic field data than the synthetic model. We disagree with the referees saying that "it will be better to prove the idea with realistic field data than the synthetic model". Nothing can be "proved" from a real field case using real data; this can only be used to "demonstrate" that the method can be applied in practice and that it can produce results that appear to be plausible. The results from a real field case can only be evaluated by subjective plausibility.

This fact is actually our reason for using a synthetic model with "realistic complexity" and "synthetic data sets" that are comparable to typical data sets for a real field case. Using the synthetic case makes us able to compare model estimation results and predictions with "true fields" and "true values of the predictions". By using the synthetic case we can quantify actual estimation errors and actual prediction errors; we can for example quantify the improvement obtained by using sharp instead of smooth inversion.

Furthermore, in this case, we have tried to faithfully represent the standard practice of hydrologists in constructing models (first handling the geophysical data, hereafter the geophysical models are used as input to the hydrological construction/calibration)

General comments 3. In the section 3.3, how do you get those values of constraint factors?

This answer has also been given to referee #1: First we determined the constrain values for one smooth model. As explained in the manuscript no vertical constrains were applied (which we think is fine) considering the small number of layers and the shallow discretization. Normally, the vertical discretization is characterized by logarithmically increasing layer thicknesses. As explained in the manuscript we choose to work with the same model discretization for both the geophysical and hydrological model to avoid numerical discretization errors. So, to account for the fixed layer thicknesses in the geophysical model, the horizontal constrain factors was set to decrease linearly with depth (tighter bands for the deeper layers). Furthermore, the strength given to the horizontal constraints is based on experience, keeping in mind that the constrains must not be too strong preventing fitting the data. Furthermore we visually inspecting the inverted model and found (strong) inversion artifacts perpendicular to the flight lines when using the same uniform constraint factors along the flight lines as to perpendicular to the flight lines. This is a result of having more data along the flight lines compared to perpendicular to the flight lines, and why the horizontal contains is different for the two directions.

The same conceptions were applied for the “sharp” inversion. We were running the sharp inversion (for the same model) with a couple of different settings (again, based on experience and in all cases fair values) and choose settings that were producing sharp structures that looked fair (without the reference system in mind, of cause).

The usefulness of the resulting geophysical inversion models depends critically on an optimal choice of the vertical and horizontal regularization of the inversion. Set the constraints too tight, and the resulting models will become overly smooth and potential resolution is lost. Set the constraints too loose, and spurious model details will appear that have no bearing on the hydrogeology. Furthermore, we don’t use any model analysis to weight the geophysical inversion results into the hydrological estimations. The

constrain values (in all cases fair values) affects only the final geophysical models.

The way we chose the constraint factors are in agreement with common geophysical modeling practice, see for example: Sharp spatially constrained inversion with applications to transient electromagnetic data, Geophysical Prospecting, 63, 1, 243-255. 2015, Vignoli, G., G. Fiandaca, A. V. Christiansen, C. Kirkegaard, and E. Auken

A comparison of helicopter-borne electromagnetic systems for hydrogeologic studies, Geophysical Prospecting, 2015, 1-24. 2015, Bedrosian., P., C. Schamper, and E. Auken.

General comments 4. In the section 3.4, the choices of weights for head and discharge data are significantly different. Why it has such a big difference? In the reality, how could you get the weight based on “trial and error” method?

This answer has also been given to referee #1: If the model is expected to not have structural defects then it would be ideal to choose the weights ω_h=σ_hˆ(-1) and ω_r=σ_rˆ(-1). However, in this case (as in all real cases) the model has structural errors that make misfit between hydraulic head data and equivalent simulated values much larger than what can be explained by observation error (σ_h). Using ω_h=σ_hˆ(-1) will therefore cause overfitting the head data because the head misfits (the residuals) are contaminated by structural error. Residual analysis and a few experiments were therefore made (as explained in the manuscript) to show that the choice ãĂŰω_h=(10·σ_h)ãĂŮˆ(-2) is in agreement with the magnitude of the total head error (which is the sum of observation error and structural error). Hereby we avoid overfitting the head data. As explained in the manuscript, in this case simulation of river discharge does not appear to be contaminated by the structural errors of the model, so for the discharge data we used the normally preferred weight ω_r=σ_rˆ(-1). The way we chose the weights are in agreement with common groundwater modeling practice of using residual analysis for this purpose, see for example: Christensen, S., K.R. Rasmussen & K. Møller (1998): Prediction of regional ground-water flow to streams. Ground Water, vol. 36, no. 2, p.

351-360. Christensen, S. (1997): On the strategy of estimating regional-scale transmissivity fields. Ground Water, vol.35, no. 1, p. 131-139. We can add a few more sentences about our choice of weights to the manuscript if this is recommended.

General comments 5. In the simulation part, the only case used Smooth regularization is Smooth-3. What is the simulation results looks like for other noise level?

Good question! We did not analyze other smooth models than “smooth-3”, because when we saw the “smooth-3” and “sharp-3” results it convinced us that for the studied case the smooth model will always perform worse than the sharp model. This is because the geology of the synthetic system consists of “large-scale” structures of categorical fields with sharp transitions (like in a North-European or North-American glacial landscape). “Smooth inversion” cannot produce sharp transitions, so it is unlikely that a “smooth model” can do as good as a “sharp model”. We therefore only use the one “smooth-3” example to demonstrate ramifications of using smooth instead of sharp inervsion. We do not see value in performing the comparison for other noise levels. Furthermore, doing the remaining smooth simulations would be computationally expensive (approx. 2-3 weeks using 64 CPU′s).

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