Hydrol. Earth Syst. Sci. Discuss.,
doi:10.5194/hess-2016-245-AC1, 2016

Hydrology and Earth System Sciences Discussions
Open Access

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

N. K. Christensen et al.

nkc07@phys.au.dk

Received and published: 7 September 2016

We would like to thank Anonymous Referee #1 for his valuable and relevant comments. Our replies are found below.

General comments 1. In section 3.3, depth and direction dependent horizontal constraint factors were used for both smooth and sharp inversions, and the constraint factors assigned for the two inversion methods are different. However, in the results part, the author compared the impact of the two methods on the predictions of flow model, is the comparison fair?

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.

General comments 2. In section 3.4, the author weighted the river discharge observation more than hydraulic head observation when defined the objective function. Why the author think that the calibrated models have error in their simulation of hydraulic head but not in simulation of river discharge?

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 3. Figure 1 in this manuscript described the conceptual flowchart for the sequential hydrogeophysical inversion. The whole framework of the experiment process was clearly displayed by the flowchart, however the content and details of each experiment step are obscure. It is hard to understand that what kind of experiment was conducted exactly in this research without reading the text description, thus I suggest the author modify the flowchart to make it intelligible.

We don't understand? However, some references to the flowchart in the body text of the manuscript could be clearer and consist with the numbering!

Printer-friendly version

Discussion paper

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., doi:10.5194/hess-2016-245, 2016.

Printer-friendly version

Discussion paper