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Discussions

Interactive comment on "An automated method to build groundwater model hydrostratigraphy from airborne electromagnetic data and lithological borehole logs" *by* P. A. Marker et al.

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First, the authors would like to thank Referee#2 for the comments on the paper. Referee comments will be presented in quotes.

"However, I agree with referee 1 that there might be some confusion about the novelty of the paper. Especially, the title of the paper is misleading since the methodology to derive the hydrostratigraphy is the subject of the paper by Foged et al. (2014) and that the field case seems to be the same. The originality of the paper here is rather to compare several models with various complexity based on their hydrogeological output.

Some work should thus be done to focus the paper more on its original aspects and not on already published material (already in the introduction)."

See also our reply to Referee#1. We are aware of the overlap between this paper and Foged et al., 2014, HESS and will address this in the revision. We believe that the entire work flow, including CF-model inversion, clustering and calibration of these clusters in a hydrological model should be presented and discussed in the paper. We will revise the paper with respect to repetition, and consider a new title from which it is clear that the focus of the paper is hydrological applications.

"Another concern, from my point of view, is that the authors claim that they reduce the structural uncertainty of their model. For me, the methodology is not sufficient to do that. Indeed, once the number of cluster is chosen, a unique model is drawn. It is true that this model is now based on geophysical data, but a unique model cannot be used to assess the uncertainty. Structural uncertainty has two causes: (1) the conceptualization of the geology, using alternative geological scenarios for example and (2) spatial uncertainty inside the geological scenarios itself linked to the location of the different facies and their respective relationships. Assessing structural uncertainty would require to allow for more models. It is well-known that geophysical inversion is not unique and that several models may explain the same data. This should be included in a structural uncertainty analysis which is not the case here since geophysics and clay fraction distribution are taken as certain. Moreover, the proposed methodology has two inversion steps (AEM data and CF data). Consequently, the limitations of arising from the inversion are present two times, such as the varying resolution, the effect of regularization and the risk of artifcats of inversion. Those limitations should be clearly stated in the text since it makes the model quite deterministic."

The comments regarding structural uncertainty are appreciated, and we agree that our approach as presented in the paper is purely deterministic; one single hydrogeological cluster model is presented. We do not address model structural uncertainty in this paper. We believe that this comment on structural uncertainty arises from a misun-

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derstanding of the paragraph from page 1575 lines 27-28 to page 1576 lines 1-6. We will rephrase the paragraph to make our statement clear, to hopefully avoid misunderstandings.

“More details should be given on how the CF model is obtained. The petrophysical relationship is not shown. What are the two parameters linking clay fraction and resistivity? What are their respective ranges of variation? Are the obtained values physically plausible? With a locally dependent relationship, it is always possible to find a straightforward relationship which could subsequently affect the results. Few is said about the different scales for each data and model, borehole logs are fine-scale, geophysical data are representative of large volume and the hydrogeological model is a large scale model.”

For these details of the CF-model inversion we refer to Foged et al., 2014, HESS, which are not repeated in this manuscript to avoid excessive overlap.We will consider including details about the CF-model inversion where/if relevant in the results and discussion.

“I do not really see the usefulness of the reference model. It is not clear how many facies are defined for this reference. Maybe a comparison of the approach with a method based on the direct interpretation of resistivity in hydrofacies would be more suited, to see the advantage of adding the CF step.”

The reference model represents the hydrofacies as they were originally modelled in the integrated hydrological model. The reference model approach was chosen to have a baseline hydrological model performance for benchmarking. All four reviewers agree that the inclusion of the reference model has little added value and is partly confusing. We will therefore consider removing the discussion of the reference model from this paper.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 12, 1555, 2015.