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Hydrology and Earth System Sciences
Discussions

Interactive comment on "A framework for testing the use of electric and electromagnetic data to reduce the prediction error of groundwater models" *by* N. K. Christensen et al.

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

Overall Comments: "Some of the largest challenges with coupled or joint inversion are linking geophysical measurements to hydrological parameters of interest. In this manuscript, the authors almost entirely neglect this with the justification of demonstrating an example (resistivity is assumed to have a direct relationship to K, porosity is assumed to be known). How is it possible to know that the absence of reliable hydrologic output parameter prediction isn't due to the poor petrophysical relationship?

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In the example we imagine that a constant relationship exists, so for the entire catchment true resistivity gives true hydraulic conductivity when using the relationship. This is indeed naive compared to many real investigations, but it makes a case where EM measurements have the best possible chance to resolve change of lithology and change of hydraulic conductivity. This implies that a “poor” prediction is not due to the petrophysical relationship, but due to the limitations of the geophysical model and inversion approach, initial parameters of the inversion etc.

"After all, if a fully synthetic system is designed and then converted between hydrologic and geophysical properties using an empirical or semi-empirical petrophysical model the petrophysical model may be incorrect. How can the authors justify enforcing a link between hydraulic conductivity and resistivity, but not porosity (as Kozeny-Carman would require)?"

As we state at page 9619 line 16, porosity cannot be estimated from the hydrological and geophysical data available here. We therefore made the subjective choice not to include porosity as a part of the petrophysical relationship nor estimating this parameter during the different model calibration. As we state at page 9615 line 13-16, a more complicated, or less certain, relationship between electrical resistivity and hydraulic conductivity (and porosity) could have been chosen, but we made the simpler choice (with no influence from porosity) because it makes a case where EM measurements together with the hydrological data have the best possible chance to resolve change of lithology and change of hydraulic conductivity. We will emphasize this in the revised manuscript.

"A discussion section is absent." See our answer to the second last question.

"The authors helpfully identified previous simultaneous inversion examples “Linde et al. (2006), Herckenrath et al. (2013a) and Vilhelmsen et al. (2014),” and it would be helpful to relate these current results to the past examples. Alternatively, if the results

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of this investigation cannot be related to past experiments due to the highly synthetic nature of the study, then I question it's relevance to a hydrology journal and suggest consideration of an engineering journal may be more appropriate to document the development of the HYTEB computational environment."

We disagree. Our demonstration focus on hydrological predictions and whether they can be improved by using geophysical data in two different ways (SHI and JHI) to support parameter estimation of a groundwater model. In this respect our demonstration differs from Herckenrath et al. (2013a) and Vilhelmsen et al. (2014) who only focus on parameter estimation and not on hydrological predictions. So we find that this study is highly relevant to a hydrological journal, and even more than the mentioned previous studies.

"I suggest including a table of all symbols and definitions. There are many symbols used in this manuscript, and some of them are ambiguous (for example, small sigma may be used to refer to electrical conductivity or standard deviation, although I think it is always standard deviation in this manuscript.) "

We will consider including a table of all symbols and definitions if the editor finds it necessary.

"Line Comments: Replace all "worth" with "value." "

We are not finding good reason to change the current text, because the phrase "worth" is in line with related literature on the topic. See for example Doherty, J.E., Hunt, R.J., and Tonkin, M.J., 2010, Approaches to highly parameterized inversion: A guide to using PEST for model-parameter and predictive-uncertainty analysis: U.S. Geological Survey Scientific Investigations Report 2010–5211, 71 p.

"P9604, L:9: "play back" idiom. Consider replacing". The text will be modified and enhanced according to the suggestions made by referee #1.

P9604, L:9-10: The wording in this sentence is awkward. Suggest rephrasing. The text

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will be modified and enhanced according to the suggestions made by referee #1.

P9613, L. 26: “Fig. 2” The text on this figure is hard to read and in some cases overlapping. I suggest redrawing for clarity. We will enhance the layout off figure 2

P9616, L23: I understand that assuming a relationship between res and K is handy for simplicity, but it is also highly unrealistic. What will be the impact when a realistic relationship must be used when incorporating field data? How should that relationship be developed in order to work properly within this modeling framework?

The referee is asking all the right and relevant questions. One of the advantages of HYTEB is providing the framework for doing such experiments. However, estimating the relationship between resistivity and hydraulic conductivity for field data is beyond the scope of this manuscript. (This is actually studied by some of our partners in the HYGEM project.) However, here we intend to analyze under the most favorable system conditions (where there is a perfect relationship between hydraulic conductivity and resistivity), how well can a groundwater model make predictions when it is developed and calibrated from geophysical and hydrological data as it is done here. Any shortcomings of the data/model analysis would only be worse if there were uncertainties or nonuniqueness in the petrophysical relationship. As said to the other reviewer, the real limitation in using synthetic models is when favorable assumptions are made and then used to support/advance methods. Here we are not really successful even under favorable conditions, indicating that other (for example more dense) data or modeling/inversion approaches should be used for this type of case. We will emphasize this in the conclusion of the revised manuscript.

P9619, L16-17: This is a bit confusing –porosity is a key and critical parameter. How is it justified to assume it is known? Also, it seems like the Archies type relationship for porosity might be more reliable than estimating K from resistivity, so why is K the one calculated and porosity?

We have answered this question previously.

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P9619, L21-24: Since the numbers of layers in the geophysical model is linked to the number of layers in the synthetic geological model, does this mean it is required to know the number of geologic units in a real scenario a priori?

No, but again it is done to simplify as well as to make conditions favorable.

P9624, L10: How computationally intensive was it really? What kind of limitation might this pose for general users to HYTEB?

Making the calibration and predictions for 10 system realizations parallelized onto 24 CPU's took approx. 7 days for JHI-T, JHI-H, and JHI-G; 5 days for HI-T and HI-H; and approx. 2 days for SHI. This makes a total of approx. 2 weeks. We do not see this as a serious limitation. P9621, L18: It appears here that hydraulic conductivity is now represented as lowercase-k, rather than uppercase-K as in table 1. Is this significant? An error? What is the difference between these k's? The referee is correct. This is an error. We will change to use uppercase-K everywhere. As said in lines 18-19, K_joint is K inferred from geophysics, and K_mf is K inferred from hydrology (and used in the groundwater model).

P9627, L21: "Figure 6" the figures have a lot of overlapping points and numbers – hard to decipher overall. Suggest re-drawing for clarity.

We will remove the numbering on this figure. The idea behind this is to identify bias and scatter of the prediction around the unity line as suggested by Doherty and Christensen (2011).

P9627, L25: "Mean Error" Can the ME value reported on each panel of Figure 6 be interpreted as "Smaller is better"? In other words, would it be possible to interpret these results as "for each parameter, the model prediction with the smallest ME is the most well resolved"? If so, perhaps placing an identifying mark on each panel of this figure matrix would help the reader see more easily which is performing best and second best for each parameter? I think it would enhance clarity.

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On figure 7 we have summarized our findings from figure 6. Figure 7 highlights (with red) the predictions for which SHI or JHI reduce the prediction error compared to HI (hydraulic observations only).

P9628, L11-12: "the scatter around the identity line is larger for HI calibrated models than for JHI calibrated models" it is really hard to tell! For the head_1 prediction (and the other head predictions that are not shown in the figure) we find it visually fairly distinct that the points plot closer to the identity line for JHI and SHI than for HI. However, we will change wording in lines 13-14 to: "However, the scatter around the identity line appears to be larger for HI calibrated models than for JHI calibrated models."

P9632, L1: The purpose of the long summary text is unclear and conclusions are nearly absent. I suggest removing the summary text and instead focus on developing a clear, concise conclusions section. The referee has a good point. We will remove as much text as possible from the summary part and make the conclusions part more concise. As referee #1 mentions, this paper is already rather lengthy. We are therefore very reluctant to add a focused discussion section to the manuscript. The present "Summary and conclusion" section also has some discussion element in it. We will consider if we should keep it that way.

Table 1: The caption for the figure needs to be improved and the definition of each parameter needs to be included. I see the table referenced on p. 9614 line 6 for the first time, and no clear definitions of the symbols in the table are included there either in the immediate vicinity. K is clearly hydraulic conductivity, I presume "R" is resistivity given equation 1 on 9691, line 25, however in eq. 1, the Greek symbol rho is used. Typically R is "Resistance," not a physical property. I presume the last symbol is phi for porosity, but how is this calculated, or how does this value link with the K-to-resistivity transform? Clearly all three must be linked somehow (P9619, L16 would suggest that this is not the case – this should be expanded upon, justified, and rectified). The referee is right; we will explain the symbols in the caption. To clarify, in table 1 K is for hydraulic conductivity, R is for recharge to the groundwater model, and phi for porosity.

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 12, 9599, 2015.