

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.

Anonymous Referee #1

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Christensen et al. present a workflow to investigate to what extent electromagnetic data/models can improve groundwater model calibration. The authors ask a number of relevant questions concerning how to best integrate the electromagnetic data in the workflow and how different strategies impact the predictions of the calibrated groundwater model. A key topic in hydrogeophysics is to find adequate ways to decide on what data to acquire and how to best integrate disparate data for a specific setting and application type. This contribution is thus timely and of relevance to the HESS readership.

The topic that is investigated here is very important and that it is most challenging to

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obtain reliable conclusions from synthetic (or real-case studies) that extent to other settings. Algorithmic choices will influence the results a lot and it is impossible to vary all of these aspects (optimization or sampling method, petrophysical relationship, survey design, geological heterogeneity type, etc.). My main criticism is thus that I consider that the conclusions made are a bit too general and that they are not supported by the data. I don't criticize (just a bit) the synthetic case-study that is rather impressive, but I would just like to be sure that statements made are based on the evidence from the actual results presented.

My main points are listed below:

1. The authors have defined a framework that they call HYTEB and it is presented as a great tool to better understand the role of geophysics in hydrology and it is supposed to be very flexible. Based on the way that this work is presented I was expecting to see a statement by the authors that the HYTEB platform is available for download for any academic users that are interested. I don't find this, which means that I can't judge anything about what HYTEB can do (only a most generic flowchart is shown in Figure 1), I can only judge one specific synthetic study and the quality of the assumptions made and the validity of the findings. The impact of this manuscript would be much more important if this software would be available.
2. I find it frustrating to see a lot of general statements, such as, “Much of the lack of value of the geophysical data arises from a mistaken faith in the power of the petrophysical model ...”. This suggests that this is faith is somehow common to the “hydrogeophysics” community, while in fact I can only assume that this faith is the past faith of one of the authors. Indeed, this over reliance was prevalent until some ten years ago, but this has been solved. Most people that work in hydrogeophysics understand that a smoothness-constrained (Occam-style) geophysical inversion will lead (by construction) to a field that is smoother than reality. This implies that petrophysical relationships between geophysical and hydrogeological properties cannot be used to map a geophysical model into a hydrogeological model. Here are some of the papers

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from 10 years ago that clearly show this.

Accounting for spatially variable resolution in electrical resistivity tomography through field-scale rock-physics relations By: Singha, Kamini; Moysey, Stephen GEOPHYSICS Volume: 71 Issue: 4 Pages: A25-A28 Published: JUL-AUG 2006

A framework for inferring field-scale rock physics relationships through numerical simulation By: Moysey, S; Singha, K; Knight, R GEOPHYSICAL RESEARCH LETTERS Volume: 32 Issue: 8 Article Number: L08304 Published: APR 19 2005

Effects of spatially variable resolution on field-scale estimates of tracer concentration from electrical inversions using Archie's law By: Singha, Kamini; Gorelick, Steven M. GEOPHYSICS Volume: 71 Issue: 3 Pages: G83-G91 Published: MAY-JUN 2006

Inversion of tracer test data using tomographic constraints By: Linde, N; Finsterle, S; Hubbard, S WATER RESOURCES RESEARCH Volume: 42 Issue: 4 Article Number: W04410 Published: APR 18 2006

3. The authors present one case-study that relies on many strong assumptions and they are then surprised that the results of the inversions are biased. Of course they are. The reference field is an indicator field and the authors invert for a continuous field that is maximally smooth (while the true field has maximum entropy!). They reduce the true parameter field of 1.2 million pixels to some 500 parameters that are solved for using strong smoothness-constrained inversion. So, there is no chance for any of the approaches to lead to unbiased results if the small-scale matters and if sharp interfaces matters. This is specific to HYTEB and it has nothing to do with the value of geophysics. Also, the authors rely on a linearized gradient-based optimization method that is prone to be stuck in local minima. This is demonstrated by the different results obtained when using a homogeneous starting model or the true field. This is to be expected and it is due to limitations in the different aspects of the inversion workflow. My criticism is not that these choices are made (simplifications are needed), but I am against making sweeping generalized statements about the value (lack of value) of

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geophysics and the bias caused by geophysics. Data that are not properly handled will always lead to bias, but this is not the fault of the data. The findings presented here are valid in this specific synthetic case-study, for the chosen inversion framework, experimental design, and methods used. There are no findings here that permit the authors to make statements about the value of geophysics in general. Indeed, the authors use the geophysical models/parameters as proxies of hydraulic conductivity, which is clearly not the case in a real setting.

4. The authors state that the case-study is highly realistic and typical of Northern Europe (meaning Denmark). I would then like the authors to use all the data that they have access to in Denmark to show that a log-log resistivity/permeability relationship is valid in this type of settings and on these scales. Also, it is essential to clarify the correlation coefficient of this relationship and to use it in the inversion. The authors criticize the use of petrophysical relationships and then they use a relationship that is certainly not likely to be valid.

5. The title should be changed by removing electric data (only electromagnetic data are shown).

6. The authors should use data for data and models for models. It is very misleading to call the EM inversion results for data. I have highlighted some of this confusion below.

7. The abstract is somewhat convoluted and cryptic. I don't think it is easily understandable outside an expert group. I would suggest that the authors focus on the results of their study and avoid making more "philosophical statements" that are poorly motivated by the presented case-study. Overall, I don't see the need to include HYTEB (except if access is granted to the reader), why not only present the work as the synthetic case-study it is? There is nothing wrong with that and the results are interesting. It seems in the introduction that HYTEB is the answer to one of the most important questions in hydrogeophysics, but the reader is only presented by a simple workflow and a synthetic case-study (+ statements that HYTEB is flexible).

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8. There is no information at all about if the data used in the calibration is adequately fitted. For a meaningful comparison, all data should have a weighted RMS of 1. If the misfit varies due to the optimizer is getting stuck in local minima, then this will affect results, but these results will then be due to an inappropriate calibration. I request that WRMSE, chi-square or similar metrics are presented throughout. The study has little value (in my eyes) without this information.

9. The paper is rather lengthy and it could be shortened. Many sentences are repeated with small variations in various places in the text and I don't see the value of talking about HYTEB. The paper is not about HYTEB, it is about one synthetic case-study. A shorter paper would make the study more attractive to read.

Smaller comments: 9600, line 2: Add "that" before geophysics.

9600, line 3: I would write "data and models". In fact, the sequential approach integrates a geophysical model (not data).

9600, line 4: Adding "Therefore" can seem a bit too strong here (even more so in light of the actual content of the paper). I would suggest removing it.

9600, line 11: I would replace "and approaches to correlating" with "used to correlate".

9600, line 15: Perhaps state that the bedrock is clay. Personally I expected resistive igneous rocks.

9600, line 17: These "resistivity estimates" forms a model that here are assumed to be "data" with well-known consequences of this choice.

9600, line 25: "be minimized uniquely", what does this mean? I don't follow.

9601, line 2: Rephrase (here and elsewhere) statements about mistaken faith. This is a sweeping statement that makes little sense. Sure, garbage in equals garbage out. This is misleading because you the authors have studied the value of the geophysical model, not the actual value of the data. The data are not responsible for being misused!

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I don't think there is any value in discrediting geophysics in this way. Also, the sentence does not make sense as it mixes data and models throughout the sentence.

9601, line 20: Replace "ramifications" with "impact".

9602, line 5: This is obvious. All models are wrong and anyone involved in modeling and inversion should realize this. Remove "the model will be wrong and".

9602, line 16: Sensitivity to what?

9603, lines 6-7: Remove "the" and write "Methods". There are probably 100s of different AEM methods. The referencing in this section is very local and the authors could consider what has been done outside of Denmark and Northern Germany.

9603, line 25: This is not true, geophysical inversion is not required. Not the case in coupled inversion (say work by Mike Kowalsky) and indeed not the case for some interesting approaches, such as, this one:

Data-domain correlation approach for joint hydrogeologic inversion of time-lapse hydrogeologic and geophysical data

By: Johnson, Timothy C.; Versteeg, Roelof J.; Huang, Hai; et al. GEOPHYSICS Volume: 74 Issue: 6 Pages: F127-F140 Published: NOV-DEC 2009

9603, line 27: Also cite:

Accounting for spatially variable resolution in electrical resistivity tomography through field-scale rock-physics relations By: Singha, Kamini; Moysey, Stephen GEOPHYSICS Volume: 71 Issue: 4 Pages: A25-A28 Published: JUL-AUG 2006

A framework for inferring field-scale rock physics relationships through numerical simulation By: Moysey, S; Singha, K; Knight, R GEOPHYSICAL RESEARCH LETTERS Volume: 32 Issue: 8 Article Number: L08304 Published: APR 19 2005

Effects of spatially variable resolution on field-scale estimates of tracer concentration

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from electrical inversions using Archie's law By: Singha, Kamini; Gorelick, Steven M. GEOPHYSICS Volume: 71 Issue: 3 Pages: G83-G91 Published: MAY-JUN 2006

Inversion of tracer test data using tomographic constraints By: Linde, N; Finsterle, S; Hubbard, S WATER RESOURCES RESEARCH Volume: 42 Issue: 4 Article Number: W04410 Published: APR 18 2006

9604, line 1: Not correct, the SHI approach incorporates results from an inversion model, not from the data. The distinction is important and explains why the joint inversion approach works better.

9604, line 9: This is not the main reason while the sequential approach fails. Read papers suggested above.

9604, lines 11-12: Yes, but approaches exists to deal with this, say the paper by Moysey et al. cited above or the work by Lochbühler et al. (2014)

Conditioning of Multiple-Point Statistics Facies Simulations to Tomographic Images By: Lochbuehler, Tobias; Piro, Guillaume; Straubhaar, Julien; et al. MATHEMATICAL GEOSCIENCES Volume: 46 Issue: 5 Special Issue: SI Pages: 625-645 Published: JUL 2014

9604, lines 26-28: I doubt that the authors will find this type of relationships in a similar case-study in Denmark that is applied at this scale. There are better ways to do so. This is fine for a synthetic example, but it is most likely a futile approach in a real case-study.

9605, line 1: Paper of Linde et al. (2006) is on joint inversion of geophysical data, better cite Lochbühler et al. (2013)

Structure-coupled joint inversion of geophysical and hydrological data By: Lochbuehler, Tobias; Doetsch, Joseph; Brauchler, Ralf; et al. GEOPHYSICS Volume: 78 Issue: 3 Pages: ID1-ID14 Published: MAY-JUN 2013

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9605, line 19: Most of the geophysical methods are rather old, so I am unsure if this is the driving reason.

9606, line 2: I would remove "for making experiments".

9606, line 8: Supposed similarity". I don't think this case-study is overly realistic. Both in terms of the geostatistical model or in terms of the petrophysical relationship used. I don't suggest that it is not a good test model (it is rather good), but I wouldn't go so far that it is similar to reality. How do we know? The only thing we know is that we are always wrong.

9607, line 10: Replace "response models" with forward simulators or just simulators. Avoid having to many different meanings of "models" in the data. Petrophysical model, forward response model, inverse model, reference model, etc.

9607, line 11: not true, as there are significant modeling errors involved. Obviously for the 1-D EM modeling, but also for the hydrological model. However, the same simulator is used throughout (the so-called inverse crime). 9609, line 16: The paper by Günther is on inversion remove it. Indeed, no idea why the authors write about ERT here as it is not used. I would only focus on EM. 9609, lines 19-20: This is clearly leading to very large errors, so please don't make statements that the modeling is highly advanced and realistic. IF such statements are to be left, then I request a comparison of the 3-D forward simulation with the true field and the one by the 1-D integrated modeling.

9608, title 2.4: Please reconsider the name of the section.

9610, line 21: This statement makes little sense and it is at odds with the summary and conclusions where it is written that EM is excellent to derive the bedrock interface.

9611, line 3: Not true, HYTEB deals as stated with zones, pilot points and combinations. This is fine, but it is a very small part of all types of parameterizations that are and can be used. Please revise.

9614, line 7: Why using maximum entropy? It does not seem like a very geologically

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realistic choice.

9616, equation 1: State that this relationship is used here (what is the value of e , what is the resulting correlation coefficient, a value of 0.40-5 seems fair), but that this is unlikely to be valid in real settings throughout the whole model domain. It is not good to write in a hydrology context that this type of relationships is defensible, they are normally not, and there is ample literature that explains why.

9617, lines 18-27: Why not airborne as suggested in intro. Why 77 land-based, and not 5000 airborne? Probably due to computational issues with HYTEB.

9618: This is highly technical and an average hydrological reader (and many geophysicists will not follow). Please simplify or explain. What is the gate center time, what is dB/dt , what is a sign shift, what is an off-center configuration?

9619, lines 9-10: A very important assumption is made when going from 1,2 million pixels to 550 pilot points with kriging in-between, and then using a deterministic, gradient-based smoothness-constrained inversion. Many of the findings can be traced to these assumptions that are not general (they are specific to this study) and they have little to do with the data/models considered.

9620, equation 5: Here is an important source for some of the bias seen later.

9621, line 1: The target choice should be 552. Is it reached for all inversions? The data fit really needs to be presented for all cases and comparisons are only meaningful for the same data misfit.

9621, lines 13-14: This is not consistent from inverse theory and some more motivation is needed.

9626: Better results would have been obtained if inverting for the parameters in the petrophysical relationship (see paper by Linde et al. (2006) cited above).

9626, lines 26-29: This is expected and it is not a new finding.

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The content of the rest of the paper is fine, but the manuscript needs to be revised throughout to be in line with the comments made above. This is especially true for the Summary and conclusions section. It would also be good to shorten this section.

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

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