## Sequential and joint hydrogeophysical inversion using a field-scale groundwater model with ERT and TDEM data - Response to reviewer's comments

Dear editor/reviewers,

Please see our responses below (bold).

Best regards,

Daan Herckenrath

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**Based on the comments by reviewer T.P.A. Ferre and reviewer #2 we would like to acknowledge that we did not clearly describe CHI in relation to JHI. In the introduction of the paper we provided an overview of different hydrogeophyical inversion techniques, i.e. Sequential Hydrogeophysical Inversion (SHI), Joint Hydrogeophysical Inversion (JHI) and Coupled Hydrogeophysical Inversion (CHI). The way we presented these methods, suggests the methods can be substituted by one another. Although JHI and CHI can be substituted by a SHI, a CHI cannot be substituted by a JHI. Before pointing this out we provide a brief description of each method.**

- **SHI: An independent geophysical inversion is undertaken after which the inverted geophysical parameters are used to estimate hydrological model parameters. Subsequently, the inverted geophysical parameters can be used to constrain the input parameters of the hydrological model and/or by generating additional hydrological observations that can be compared with the simulations of the hydrological model.**

- **CHI: First a hydrological model is used for simulating a hydrologic system state. These hydrological simulations are converted to a geophysical parameter distribution based on which a geophysical instrument response can be simulated that can be compared with collected geophysical observations. Using this approach hydrological model parameters can be estimated by fitting the geophysical observations.**

- **JHI: The parameters of a geophysical and hydrological model are estimated simultaneously using coupling constraints that tie groundwater and geophysical parameters.**

**In both a CHI and JHI the geophysical inversion is (partly) dependent on the hydrological model. However, their coupling mechanisms are very different, as coupling in a CHI happens through simulated hydrological states, while in a JHI coupling happens through the input parameters of the hydrological model. Therefore CHI and JHI cannot be regarded as substitutes.**

**In a revised version of this manuscript we will add a section to clarify this.**

REVIEWER #1

T. P. A. Ferre (Referee)
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The authors provide an excellent, concise description of coupled (CHI), joint (JHI), and sequential (SHI) hydrogeophysical inversion. We have needed this in the literature for some time! For sequential and joint, the authors also provide a very useful mathematical definition.

I only disagree slightly with three points regarding coupled inversion. First, the author's state that hydrologic conceptual error is a severe limitation to CHI. This is true. But, I believe that it is at least as much of a limitation for JHI and SHI. The difference is that in JHI and SHI the geophysical inversion is allowed to have its own (likely equally flawed) conceptualization. But, this geophysical conceptual error is mapped onto the hydrologic conceptual error when the geophysical data are used to constrain the hydrologic model, which is the ultimate goal of the exercise. In my opinion, it is better to force a single, consistent conceptualization (or set of conceptualizations) on all of the models rather than deal with the unknown interactions of multiple inconsistent conceptualizations.

**Yes, ideally a single consistent conceptualisation should be used for both the geophysical and hydrological model. We wonder whether this is possible for JHI/CHI attempts for field-scale/regional groundwater models, ERT and EM data as the spatial resolution of regional groundwater models is much coarser than what is required to fit some geophysical data types. This especially applies for methods as ERT which have a spatial resolution in the order of 5-10 metres while regional groundwater models have cell sizes of 100 metres or more.**

The second point that I would add about CHI is related to enforcing a consistent conceptual model. That is, for methods that have variable spatial sensitivity depending upon the distribution of properties within their sample areas, it is particularly important to interpret the geophysics in the context of sub-sample-volume structure. This is particularly important for methods with relatively large sample volumes relative such as ERT and TDEM.

**This is a good point. Yes, in our JHI application coupled geophysical and groundwater parameters represent different volumes of the subsurface. We have not discussed this issue properly in our manuscript, which we will add to the revised version of this paper.**

**Enforcing consistent models that handle variable spatial sensitivity in a way mentioned by the reviewer will be difficult to apply for the types of models included in our paper. Geophysical and groundwater parameters in the synthetic and real-world example represent the same sub-volume in the vertical direction but not in the horizontal plane. The current paper applies a 3D groundwater model and 1D models for generating geophysical forward responses. The 1D geophysical models can be translated to a 2D (e.g. real-world case in the paper) or a 3D geophysical model when a set of these 1D models is projected on a grid and tied by additional constraints in the inversion process and/or by interpolation.**

**The groundwater and geophysical model are therefore not consistent as the 1D geophysical forward models are not able to explicitly model the horizontal variations in geophysical properties. In this case the spatial variable sensitivity of the geophysical models in the horizontal direction is more or less determined by the imposed regularisation and/or applied interpolation. This issue can be overcome by employing 3D geophysical forward models, but the computational burden would increase to levels that might be considered unrealistic for the type of models dealt with in this paper. Please see a paper about a 3D TDEM forward model [2], which indicates forward model calculation times in the order of hours (rough indication); for comparison, 1D TDEM forward response can be calculated in seconds.**

A third, perhaps most important, point regarding CHI is that it is most useful for interpreting time-lapse data. When transient conditions are observed, CHI also provides a natural basis for integrating measurements through time: SHI and JHI require simplifying assumptions and/or temporal smoothing approaches. Having said all of this, I duly note that this paper is focused on a comparison of JHI and SHI. I hope that the authors will extend their analyses to consider CHI in a future publication these can be considered pre-publication comments for that contribution!

**We will add this point to the discussion. The focus of this paper is to couple the static elements of a groundwater system that can potentially be described by geophysical models. We would like to make the reviewer aware of our recent publication [1] in which we present a CHI framework for seawater intrusion models and EM data.**

After the introduction, the paper focuses on two case studies. The synthetic case study considers steady state flow in a two layer subsurface. There are three hydrologic unknowns: the hydraulic conductivities of both layers and the thickness of the upper layer. Four head measurements and two flux measurements are calculated using a forward model and then corrupted with random noise. A single TDEM sounding is available, which can be used to infer the electrical conductivity of each layer and the thickness of the upper layer. The correlation between the geophysical and hydrologic parameters is also corrupted with random perturbations.

The key findings for the synthetic study are shown in Figure 3. The cyan lines show the results for SHI and the dark blue lines are for JHI. The x axis is the strength of the coupling coefficient used to 'join' the hydrologic and geophysical parameters. The examination of strength of coupling was very clever, allowing for a smooth transition from SHI to JHI. But, the main comparison can focus on the leftmost and rightmost dark blue results and the cyan results. The leftmost dark blue results represent independent interpretation. The rightmost dark blue results represent joint inversion as it would usually be applied (with strong confidence in the coupling relationship). The cyan lines do not vary with the coupling coefficient value. Viewed in this light, the results show great value for including geophysics for interpreting Kclay. Visually, there is little difference between SHI and JHI except for the presence of a few JHI outliers. Geophysics does not help to infer $K_{limestone}$ or $D_{clay}$. Again, the differences between SHI and JHI are minimal. The authors point conclude that the performance of SHI and JHI are comparable, but they point out that JHI faces a much higher computational burden, largely due to the higher computational demand of the groundwater flow model compared to that of the geophysical model.

The real world study considers steady state through a more complicated groundwater flow system. Six zonal hydraulic conductivities are estimated based on 34 head observations and four flux values. ERT data are used to infer one of these hydraulic conductivity values (shown to have the highest sensitivity with PEST) and the depth to a limestone layer at three locations. Unlike synthetic cases, it is more difficult to assess the performance of JHI and SHI in real cases. To provide a quantitative measure of performance, the authors compared the parameter estimates and their uncertainties for SHI, JHI, and independent inversion (Table 3). The inferred parameter values are essentially identical for all three approaches. Including geophysics (using SHI or JHI) reduced the parameter uncertainties; JHI led to marginally greater reduction than SHI.

I feel that the authors have done an excellent job of providing a roadmap for comparing SHI and JHI approaches (and, hopefully, CHI in the future). I do not disagree with any of their findings, which essentially note that JHI did not provide sufficient advantage over SHI to warrant the extra computational demand. However, I would caution against taking this as a general conclusion. Specifically, it seems to me that the real problem in this case was that the geophysical data only had limited information about the hydrologic system. In the synthetic case, estimates for two of the three parameters were not improved by adding geophysics; they remained poorly constrained. The third parameter was constrained more accurately using geophysics, but SHI was sufficient to extract the information. In the real world case the geophysical data only marginally improved the uncertainty of the estimates, but they did not affect the estimated parameter values. As a result, it would be excellent to see the authors' approach applied to a wide range of systems, including those for which geophysics is more informative. This would provide a more robust analysis of the relative merits of SHI and JHI.

Building on the preceding comments, I think that the authors could use the results of their study to make another point for hydrogeophysics. Specifically, I think that this points to a real need in hydrogeophysics to conduct the type of analyses that the authors have presented BEFORE data are collected. It would be a major contribution to the future success of geophysics in hydrology if we could identify opportunities for which geophysics is likely to be informative and to focus data collection there. This would reduce disappointment when geophysical data are incorporated with little improvement in models.

**Before commencing with the hydrogeophysical inversion, we should have performed a sensitivity analysis to see which parameters in the groundwater model govern the intended prediction and whether the ERT data can help constraining these parameters. A sensitivity analysis was done for the groundwater parameters (fig. 11) and geophysical parameters (fig. 10c). However this sensitivity analysis was done with respect to existing observation data, not with respect to intended predictions. Furthermore, the JHI in the real-world study coupled groundwater parameters with poorly resolved electrical resistivities. We will address this point in the revised manuscript to emphasize the importance of initial studies exploring which parameters need to be determined given a targeted groundwater model prediction and the importance of exploring whether parameter resolution in initial geophysical models provide opportunities to constrain crucial groundwater parameters.**

I look forward to reading future contributions by the authors on these topics!

Ty Ferre

P.S. Note that I chose to rate the paper as requiring technical corrections. All that I mean by this is that I would like to see a bit more discussion fleshed out on the topics that I have suggested above. My only other specific suggestion is that the plots with all of the lines showing the effect of the coupling strength may be a bit of a distraction. I think that you should reserve this approach and these plots for a future paper for which this can be more of the focus of the discussion. It really is a nice idea and deserves to be discussed in a more complete way in another paper.

**We will delete this figure in the revised version of this manuscript. The main message associated with these figures is the nature of parameter coupling. For a SHI, this can be based on either parameter resolution or correlation statistics from field data. To avoid overestimating the model coupling strength in a SHI (which can result in an underestimation of parameter uncertainty), weighting strategies should be based on that element (parameter resolution or field data) that has the largest error associated with it.**

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REVIEWER #2

Anonymous Referee #2 **-** Received and published: 3 June 2013
This paper considers different inversion methods for model data synthesis of hydrogeophysical observations and ERT and TDEM data. This is a subject that is relevant to the readership of HESS-D, and an important topic to improve characterization of the vadose zone. I think that the paper is unnecessarily difficult to follow. This will really diminish impact.

**The summary of referee #2 does not present the study entirely correctly, as our paper is about inversion strategies using groundwater/hydrogeological data and geophysical data to improve the modelling of saturated groundwater flow. Note the methods in this paper are primarily designed for groundwater flow models and highly parameterized geophysical models requiring relative large computational times. Vadose zone is not the focus of this paper.**

The description of the inversion methods is rather confusing, and reader might not be able to discern the differences between the three inversion methods that are summarized.

**We acknowledge the CHI component in the introduction needs improvement (also raised by reviewer #1; see responses). We will try to improve clarity of the paper regarding this point.**

(1) Abstract is difficult to follow. Results are not easy to understand for an average reader. Obviously, this is a choice of the authors – I do believe however that a paper can have significantly more impact if it is written in such a way that someone that is not a domain expert can still follow the text and main findings.

**We will make an effort to make the abstract easier to follow.**

(2) P:4658, Line 17 –> this work is not limited to single objective problems. For instance Huisman et al. (2010) shows how to use this methodology within a multiobjective framework,

**We believe this is a misunderstanding. This statement emphasizes that, unlike in SHI, where there are separate objective functions for geophysical and hydrogeological inverse problem, there is one single combined objective function in JHI.**

(3) I think that the paper would benefit tremendously from a Figure that conceptually explains that differences between the three different inversion methods (JHI, CHI, and SHI) considered in this paper. The explanation of the different methods is unnecessarily difficult which poses lots of questions later on.

**Figure 1 is the schematic that is referred to. A similar figure including CHI can be found in our recent publication [1]. Good schematic figures on CHI are given by Hinnell et al. (2010) and Huisman et al. (2003). CHI is not included in Figure 1, but will be in the revised manuscript.**

(4) P4662, L10; I suggest the authors to have a look at the work of Eric Laloy (WRR, 2012) and the group of Professor Linde that uses parallel MCMC methods to derive the two and three dimensional soil moisture distribution from geophysical data.

**We are aware of Laloy (2012) which describes a CHI. As mentioned before we will change the CHI section in this paper to emphasize the difference in coupling mechanisms between CHI and JHI, which makes them incomparable.**

**CHI: uses the output or state (e.g. simulated soil moisture) of a hydrological forward model to create the input parameters for a geophysical forward model after which simulated versus observed geophysical (and hydrological) data can be matched.**

**JHI: input parameters (instead of model outputs) of a hydrological model are coupled with geophysical input parameters after which geophysical and hydrological observation data are matched simultaneously.**

**The coupling mechanisms of a CHI and JHI cannot be substituted by one another, but these have the potential to complement each other. A comparison of CHI and JHI would not be inappropriate. We will focus our efforts on clarifying this important point in the revised manuscript.**

(5) P4663, L5: Remove "a" –> of ..

**We will change this.**

(6) Section 2.3. –> What about Coupled Hydrogeophysical Inversion (CHI). Why is this not used? Why not use all three different methods and then compare their results?

**See response comment (4) and the start of our response letter.**

Again, most readers will be stuck on the use of terminology. JHI versus CHI and SHI. What is their main difference? A simple schematic figure will really help to illustrate their differences. Always try to avoid describing differences mathematically. Most readers will not understand. Just use simple words, and ideally a nice schematic. I do not understand why CHI is not used in the comparative analysis. Maybe I am missing something (highly likely), but why not benchmark the work performed herein against that of Hinnell et al (2010) using CHI??? With appropriate discussion of the actual search method used, parameters considered. A less technical description will increase readability.

**We agree with the reviewer that the CHI section should be explained better and presented more clearly. We will include CHI in Figure 1 as well. See also comment (4).**

(7) The theoretical part of the paper that describes the different inversion methods is really difficult to follow. This introduces confusion.

**We will revise this section.**

(8) I wonder what search methodology is actually being used to solve the different inversion methods? Some more details on this would make the paper easier to follow. Equally important, what likelihood (objective) function is used? How is uncertainty being treated, and how does model error affect the results.

**The introduction (p. 8) already has a section describing the search methodology (gradient search algorithm) and its limitations. The objective function is to our knowledge clearly defined in equations 5, 16 and 24. Uncertainty was quantified using different parameter and observation realizations (synthetic case study) and through linear analysis based on the Jacobian of calibrated groundwater and geophysical models. Investigating how conceptual errors in the groundwater model propagate in the geophysical models is beyond the scope of this paper.**

(9) Section 3.5. –> Very difficult to follow. The paper might be presenting important findings but the methods and results are so difficult to follow (at least, according to my limited understanding) that this will really affect impact.

**We will revise paragraph 3.5.**

(10) P4682, L20: What does a std. of 10% mean? Rather strange unit. This depends on the choice of the prior distribution, actual search method used (classical linear intervals versus Bayesian (MCMC) intervals), so I would suggest providing more details about the uncertainty.

**We will rephrase this sentence. Regarding uncertainty and search-method see response to comment (8).**

**The work in this paper is done in a context of field-scale – regional groundwater models where computational times are in the order of hours. In combination with the large amount of estimable parameters (typically > 100), MCMC methods are generally not an option (yet), although advances through subspace reduction techniques, parallel computing and use of surrogate models is promising.**

**A final note regarding uncertainty. The JHI presented in this paper is not completely Bayesian, in a sense that parameter uncertainty is not only controlled by measurement error, but also affected by the strength of the parameter coupling constraints which statistical basis is difficult to determine. Ideally, its value should be based on statistical analyses of available site-data as borehole descriptions, pumping test results that are**

**subsequently correlated with the prior inversion results of the geophysical data. Based on such an analysis, a site-specific petrophysical relationship can be developed together with an estimate of its error. Note however, that this statistical analysis would include inverted geophysical parameters rather than measurement data only. Another element limiting the statistical basis of a parameter coupling constraint would be the differences in scale between geophysical model, groundwater model and groundwater observations.**

Coefficient of Variation (CV) is certainly not ideal (depends on scaling of mean), and thus why not just include a table with mean values of the parameters and their standard deviation. Note that the recent work done by Laloy et al, and Linde and co-workers on this topic explicitly confronts the issue of model parameter and soil moisture uncertainty using different dimensionality reduction methods.

**We will change Table 3 accordingly.**

I would recommend the authors to have a look at this more recent work. Altogether, I believe that a major revision is appropriate. With emphasis on rewording so that that a larger readership can understand what is done and why, and why CHI has not been included in the analysis (to my understanding).

**See comment (4).**

A detailed description of each method is warranted, including a Figure and some text that illustrates the main differences between the three different inversion approaches. Also it would be useful to have more information about the actual search method that is used to solve for the parameters.

**We will revise Figure 1 and rewrite the introduction to ensure the differences between CHI and JHI are communicated clearly.**

**References:**

**[1] Herckenrath, D., Odlum, N., Nenna, V., Knight, R., Auken, E., and Bauer-Gottwein, P.: Calibrating a salt water intrusion model with time-domain electromagnetic data, Ground Water, doi:10.1111/j.1745-6584.2012.00974.x, in press, 2012.**

**[2] Bauer-Gottwein, P., Gondwe, B. N., Christiansen, L., Herckenrath, D., Kgotlhang, L., and Zimmermann, S.: Hydrogeophysical exploration of three-dimensional salinity anomalies with the time-domain electromagnetic method (tdem), J. Hydrol., 380, 318–329, doi:10.1016/j.jhydrol.2009.11.007, 2010.**