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Hydrology and Earth System Sciences Discussions

Interactive comment on "Sequential and joint hydrogeophysical inversion using a field-scale groundwater model with ERT and TDEM data" *by* D. Herckenrath et al.

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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 authors 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. 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. 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!

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 Klimestone or Dclay. 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 advan-

tage 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.

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.

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