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Comment on hess-2022-12

Anonymous Referee #1

Referee comment on "In situ estimation of soil hydraulic and hydrodispersive properties by inversion of electromagnetic induction measurements and soil hydrological modeling" by Giovanna Dragonetti et al., Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2022-12-RC1, 2022

This manuscript presents an uncoupled inversion approach to estimate soil hydraulic and solute transport parameters from electromagnetic induction measurements made during an infiltration experiment. The consideration of solute transport in addition to water flow is relatively novel and should be of interest to the community.

After reading this manuscript, I am left with a range of concerns that are described in the general and specific comments below. Although I am generally supportive of this type of research, I can currently not recommend this manuscript for publication. To address my comments and concerns, additional analysis may be required and considerable rewriting would be necessary. I am unsure whether this still is in the realm of a major revision, but I decided to go with this recommendation.

GENERAL COMMENTS

- Considerable previous work is available discussing the use of geophysical data to parameterize hydrological models. It has been argued that the uncoupled inversion strategy used here may provide biased hydraulic parameters because errors as well as assumptions (e.g. smoothness) from the EMI inversion can propagate to the estimated water content and solute content distributions. In such cases, the use of coupled inversion has been advocated. I think that this approach may also be advantageous in your case. It should be made clear in the manuscript why the current inversion approach was selected, and why it is expected to not suffer from the problems that have led some researchers to prefer a coupled inversion approach when the aim is to parameterize a hydrological model with geophysical data. Given the used EMI-inversion approach with smoothing in space and time, I have difficult time to believe that uncoupled inversion does not lead to problems.
- A synthetic modelling experiment would help to support the presented results. It would

not only help to confirm that the uncoupled inversion approach is able to provide realistic parameter estimates, but it would also help to address other concerns addressed in the specific comments below, such as the information content of the measurements to reliably estimate 8 hydraulic parameters of two different layers from the limited number of available measurements, as well as the separation of the solute and the infiltration front.

- Although I also wish that EMI measurements could be treated as quantitative measurements, this is still not the case. As detailed in several studies, the application of EMI inversion requires the correction of the measured ECa data for expected shifts and offsets, as for example discussed in von Hebel et al. (2014) and some of his follow-up work. Such a calibration was not considered in this study, and I find this problematic. The authors argue that this only leads to problems with the estimated saturated water content, but I am not convinced by this.

SPECIFIC COMMENTS

Line 17. For most journals, a single-paragraph abstract is required. Please check.

Line 21. Consider reformulating this sentence to make clear that this is the aim of the study.

Line70. Although I understand where you are going, I think this sentence should be improved (e.g. "also" and "proper" do not really seem to fit here).

Line 86. The studies cited here are mostly focused on saturated systems and the estimation of the soil hydraulic conductivity. Perhaps it would be more appropriate to focus on studies that have attempted to estimate the full set of hydraulic parameters required to describe flow and transport in unsaturated soils.

Line 96. In my opinion, efficiency and number of electrodes are not such a good reason to discard the ERT method. If 1D models are assumed, the amount of electrodes could be substantially reduced. However, relatively large electrode separations would be required to obtain sensitivity at depth. The sensitivity distribution with depth is much more favorable in case of EMI, which enables a more compact experiment.

Line 105. Consider rewriting here. The apparent electrical conductivity DOES represent the electrical conductivity distribution with depth. However, there is no direct relation and there are many distributions that can provide the same apparent conductivity. Perhaps use "…does not directly provide information…".

Line 159. The text is confusing here. Two altitudes are provided. Consider rewriting.

Line 172. Perhaps it would be good to already mention the water content at the start of the experiment here.

Line 181. Please provide manufacturer of these sensors.

Line 182. How does this compare to the pore water electrical conductivity of the remaining water content? Both the initial water content and this information is essential to evaluate whether the first infiltration experiment can be evaluated solely in terms of water content variations.

Line 183. Can you be more precise about the measurement schedule? I guess you mean 1 hour irrigation and a break of 1 hour, but I am not sure.

Line 184. I guess it was 2000 dm3 on 16 m2? Perhaps already provide units in mm (or m) given that you will be using 1D modelling afterwards.

Line 210. This text confuses me. Were multiple experiments performed? Why average water volume? Please clarify.

Line 218. Is there an S in the equation, or is this a typo? If yes, please describe what it represents?

Line 229. To be able to reproduce the simulations, I think you should describe the layers that you assumed in the EMI inversion in more detail.

Line 230. This text is confusing because it suggests that models are constrained in space by neighbors. In my understanding, there is only 1 model with 7 layers here. Correct? Is there a constraint on the layer-by-layer variation? It is clearer later in the text, but please improve text here already.

Line 246. I wonder whether there is any support for the time-lapse inversion strategy implemented here. You are penalizing changes in space as strong as changes in time by using a single regularization parameter. Is this realistic? The literature on time-lapse ERT inversion is substantially larger. Has this approach been considered for ERT?

Line 248. At this point, it would be good to describe how the desired value of the regularization parameter was determined.

Line 284. It is not clear to me how the water content was obtained here. Did you use Eq. (1) and assumed a fixed pore water conductivity equal to the applied tap water? How was the permittivity converted to water content in this case?

Line 287. What kind of optimization procedure was used? Or do you mean here that the optimization implemented in Hydrus was used? In any case, it would be good to mention the optimization strategy.

Line 294. At this point, I am missing two important aspects. First, it would be good to discuss whether all fifteen hydraulic parameters were optimized. If yes, I would recommend reflecting on the identifiability of all these parameters. Is there sufficient information? This is particularly doubtful for the bedrock in case of the TDR measurements since it does not contain a sensor. Second, I think you need to clarify how the initial conditions were specified. Only with this information, it is possible to reproduce your model set-up.

Line 304. This seems to suggest that the same dispersivity was assumed for the three layers? A short justification would be appropriate.

Figure 3. Please emphasize that the modelling is related to the EMI inversion only in the caption. You have multiple inversions in your approach. I also think that more reflection is required on the relatively poor fit provided here. To what extent can this be related to the lack of calibration of the EMI measurements (see general comments).

Line 321. I assume that a set of EMI measurements before the infiltration is also available. I think it would be good to also include these measurements here and use them to reflect on the initial conditions.

Line 328. If the topsoil is saturated and the bedrock remains dry (i.e. no changes), I wonder where all the water applied after the third irrigation is going? Is it flowing laterally? This would be problematic because of the 1D model used to describe water flow.

Line 335. In case of TDR, I assume that this is the mean value from the four sensors? I propose to include error bars to reflect the spatial variability of the measured bulk conductivity obtained with TDR.

Line 347. I find it optimistic to state that a mean error of 16 mS m-1 is acceptable. The entire range of inverted bulk electrical conductivity is from 0 to 60. Would an accuracy of 0.15 cm3cm-3 (range of 0 – 0.45) be acceptable? I think some more critical reflection is required here.

Line 352. This is correct. Based on this observation, it was concluded that corrections are required before meaningful EMI inversion results can be obtained.

Line 377. It would be desirable to also show the fit to the tensiometer data.

Line 379. For the EMI-based inversion, I assume that the data presented in Figure 4 were converted to water content and used for the inversion? I think this should be emphasized more because one may obtain the impression that the two depths presented in Figure 6 were used only.

Line 380. I think this information should be provided in the methods and not in the results (see specific comment for Line 294).

Line 381. Units are missing for the hydraulic parameters. Also make sure that they are consistent with the parameters presented in Table 1. I assume that the units of hydraulic conductivity are not consistent, otherwise the bedrock would be the most conductive.

Line 384. Please also provide a simulated water content distribution like Figure 4. I am particularly interested in seeing the development in the bedrock layer.

Line 404. Make sure to indicate which parameters were fixed during optimization.

Figure 7. Consider changing the legend. I think you should use the horizon names and not the method names. This suggests that the two depths were inverted independently, which is hopefully not the case.

Figure 8. Would be good to also present measurements at t=0. How do the initial conditions of the second experiment compare to those of the first experiment?

Line 432. I wonder whether this can be interpreted as a separation of the infiltration front and the solute front. This is expected to happen, especially if the soil is relatively wet at the start of the experiment. Given that the background electrical conductivity at depth is much higher in Figure 9 than in Figure 4, this may be the case.

Line 454. This could be supported by using the error bars to represent the variability in the four measurements.

Line 463. This seems to suggest that only two depths were extracted from the EMI inversion. It is not clear to me why the information in Figure 9 was not used during the optimization. I would say that one of the key advantages of EMI is that we obtain more depth information compared to TDR, but this aspect does not seem to be considered here.