“In-situ estimation of soil hydraulic and hydrodispersive properties by inversion of Electromagnetic Induction measurements and soil hydrological modeling”

Dear Editor and Referees,

Your constructive comments on our manuscript are greatly appreciated. Our answers are listed below each of your comments, in italics.

Sincerely,
Mohammad Farzamian
on behalf of all authors

Reviewer #1

I have now evaluated the revised manuscript “In-situ estimation of soil hydraulic and hydrodispersive properties by inversion of electromagnetic induction measurements and soil hydrological modelling”. The authors have made extensive revisions based on the reviewer comments, and this has improved the clarity of the manuscript substantially. It is now clear what has been done and why, although I am not a big supporter of some of the (perhaps necessary) decisions that the authors have taken (see general comments below). However, this should not stand in the way of a publication of these results and insights. I have provided specific comments below that I would like to see addressed in a revised version. After these minor to moderate revisions have been adequately addressed, I recommend accepting this manuscript for publication.

GENERAL COMMENTS
The authors prefer to use uncalibrated EMI measurements to realize an approach that does not rely on additional measurements. I am quite skeptical about this, and I would strongly prefer the use of calibration to improve the ECa measurement before inversion. It is well established that different coil configurations and orientations may require different corrections. In this same context, I see the fit to the measured data shown in Figure 3. I do not agree with the authors that this is a fairly good fit. This would perhaps be clearer if plotted as a 1:1 line. For me, the solution presented in Figure 4 is a strongly smoothed fit to the data, and the derived water content profile can only be a rough approximation of the actual water content profile. However, I agree to disagree on this issue. I do not think that this a nice approach, and I would not like to invert measurements that I know are biased. However, the results presented in this manuscript show what can be achieved when doing this. It can be left to the reader to judge for themselves whether they find this useful.

The proposed approach optimizes the hydraulic parameters layer by layer. I do not think that this is a generally valid strategy, because the water content development in one layer is not independent of the hydraulic properties of the other layers when long-time evolution is considered. However, in the case of a single infiltration event as used here I think it should be possible to do invert the parameters sequentially. I think it would be good to emphasize this in the manuscript.

> *in line with the comment of the Reviewer, we added a sentence in the discussion at lines 438-441 of the new manuscript: “Despite the water content development in one layer is not independent on the hydraulic properties of the other layers when long-time evolution is considered, in the case of a relatively short infiltration event as used here, this approach makes parameter estimation of multi-layered profiles feasible.”*

SPECIFIC COMMENTS

Line 20. Check that all abbreviations are defined at first use. This also applies to the abstract (PTF, TDR).

We have defined all the abbreviations at first use.

Line 29. The order of the presented information is awkward here. Should the statements on the water content profile not come before the statements about the derived hydraulic parameters?

Agree. We have rewritten this paragraph.

Line 82-84. Improve statement. ERT has not been used to investigate solute transport in models – it has been used in the field.

We revised this sentence.

Line 88. Would be good to introduce limitations of ERT here before switching to EMI.

The main limitation of the ERT method in respect to the EMI concerns the need of installing electrodes. This limitation has been reported and better stressed.

Line 117. Not so useful to point forward in the text here. Consider deleting.

We deleted the sentence.

Line 121. Avoid colloquial writing. Use "does not" instead of "doesn't".

We modified the text here and in other two places in the text

Line 161. Check sentence. Two altitudes?

We revised this sentence.

Line 234. Please report which model was used to relate water content and dielectric permittivity.

We added the reference to the applied Topp (1980) equation.

Line 247. Is this the thickness of the layers, or the position of the layer boundaries. Please check carefully.

Sorry for the confusion. The values refer to the depths of the layer boundaries. We have now revised the text accordingly.

Line 256. I think parameter update would be a more appropriate terminology here.

Agree. We have substitute "corrections" with "update".

Line 310. Information about the assumed pore water electrical conductivity is missing here.

This information was already provided in Materials & Methods section, line 192 of the previous version of the manuscript.

Line 363. It is well known that all channels are shifted differently. Therefore, this statement is rather optimistic.

We revised this sentence and also deleted "one or more".

Line 373. Be careful how you argue here, because later the hydraulic parameters derived from TDR are used as a reference to evaluate the EMI inversion results. If the TDR measurements reflect local heterogeneity and not the bulk behavior, the estimated hydraulic parameters cannot be trusted either.

Many thanks for revealing this key point. We decided to delete this sentence.

Line 394. I would not mix arguments pertaining to apparent and bulk conductivity. I think that apparent bulk conductivity values need to be corrected before inversion, and this is exactly the point of von Hebel et al. (2014).

In order to avoid misunderstanding we deleted the following sentence "von Hebel et al. (2014) also found a similar behaviour when comparing their EMI results with ERT surveys. In that case, the σa values measured by EMI systematically underestimated the σa generated by applying EMI forward modelling to the σb distribution retrieved from the ERT surveys."

Line 441. Please carefully check the references. I found several papers that were cited in the main text, but not included in the references.

We have now revised both the main text and the references.

Line 451. Not sure that I understand the point that you are trying to make here. Can you improve your argumentation?

We have added a sentence specifying the relationship between the parallel behaviour of the water content and the shape of hydraulic properties. "This is a crucial point in this paper, as the parallel behaviour of the water content evolution will explain the similar shape of hydraulic properties we found for the TDR and EMI-based estimations (see Figure 8)."

Line 480. Please also comment on the higher Ks value for the Bw horizon. It is a bit surprising given the lower fitted n-value. Any other supporting information to confirm the plausibility of the estimated parameters obtained with inverse modelling?

We have added a comment explaining the why such behaviour could be expected. "Compared to the Ap horizon, higher Ks and lower n values were found for the Bw horizon. This may be explained by considering that tillage in the Ap horizon changes the geometry of the porous system, by reducing the structural pores, responsible of the lower Ks for Ap, and increasing the textural pores, explaining the higher n value for Ap.

Figure 8. I suggest to limit the x-axis to the range of pressure head values measured in the field. This provides a more realistic impression of the fitted results. All information for more negative pressure heads is just related to fixed parameters…

We do not agree to this request because, at this point in the paper, we are not looking at measured pressure heads anymore, but we are looking to the parameter's estimation obtained by the inversion technique and therefore on the whole shape of the hydraulic properties. The goal of the paper is to find the hydraulic properties parameters: a view of the whole curves, showing also the behaviour and shape of the curves where no measurements were available, is necessary.

Line 547. I would suggest to use the same procedure for TDR and EMI. Why should an estimate through Z be preferred over the use of the estimated TDR bulk conductivity? This just makes the results less transparent. If you keep this approach, please provide a reference to the used approach to estimate Cl- concentration from impedance directly.

We changed the text by removing any reference to the impedance Z, referring to the only σ_b*. In any case we want to clarify here that the* σ_b *estimated by TDR comes from a direct measurement of the impedance Z.*

Line 579. It would be nice to produce a plot such as Figure 10 using the simulated water content and solute concentration with the optimized parameters. This could show whether the high inverted concentration near the surface is well reproduced by the model. This would be important given that this region was not captured by the TDR sensors and the EMI layers at 20 and 40 cm depth. For consistency, this should then perhaps also be presented for Figure 4.

We disagree with this request of the Reviewer. While it makes sense, we believe that the manuscript is already long with many Figures and a 7-steps procedure that is not immediately easy to follow. New additions of figures and contents, in our opinion, will go to decrement of readability of the manuscript.

Line 628. There is evidence that the required corrections for ECa are relatively stable across different sites and conditions – this would remove the need for additional measurements.

We assume that – from the Premise and General Comment - this point by the Reviewer is just a comment without requiring modification in the manuscript. The stability of the instrumental shift in conductivity values is still to be fully solved, due to system miscalibration, the influence of surrounding conditions such as temperature, solar radiation, power supply conditions, the presence of the operator, zero-levelling procedures, cables close to the system and/or the field setup.

Reviewer #2

Many of my comments on the first version arose from a lack of clarity about the scales of interest and the objective. The authors have resolved these issues in this version.
The number of self-citations was reduced.
The procedures are better explained and the figures are more clear. The combination of field and lab work is explained better in the revised version.
The narrower focus in the paper, the more complete discussions of the strenghths and limitations of the proposed method, and the improved clarification of the combination of lab and field work significantly improved the effectiveness of the communication.
Overall I am now more convinced than I was originally that the paper makes a contribution that warrants publication in HESS. A few minor points are given in the annotated manuscript, but these do not require an additional review in my opinion.

SPECIFIC COMMENTS

The language used sometimes resembles spoken English more than written English, inlcuding abbreviations ('doesn't', 'let's') that are normally avoided in written texts.
This is difficult to fix for me, and I do not know the HESS standards regarding colloquial English.

We modified the abbreviations through the text.

Line 34: The relationship between K and the volumetric water content says otherwise. Please rephrase to clarify the point you wish to make.

As a result of this comment and another one from reviewer #1, we revised this paragraph of the abstract.

Line 369: You refer to this figure after referring to Figs. 5 and 6. Perhaps change toe order.

We doublechecked the order of the Figures. Figure 4 was referred at line 353 before we quote the Figure 5 and 6.

Line 626: Replace by 'here' or by 'in this paper' - I presume you are referring to this paper.

We have modified the term "our" with "this" or "the" in this line and throughout the paper.

Lines 655-656: See my remark in the abstract. I have the impression that what you are trying to say is not what this phrase actually says. But because I cannot surmise the point you want to make here, I cannot offer an alternative phrase.
Perhaps you could argue that if you replace the water content by the degree of saturation, ther effect of the inaccurate estimates of the water content is reduced because the saturated water content will be affected in a similr way as the unsaturated water contents - If this is the point you are trying to make.

Many thanks for your comments. The sentence was not clear. Therefore, we have modified the sentence here, in the abstract and in the discussion.