

**“In-situ estimation of soil hydraulic and hydrodispersive properties by inversion of Electromagnetic Induction measurements and soil hydrological modeling” - authors’ responses to suggestions and comments made in Public Discussion**

Dear Editor and Referees,

we sincerely appreciate your constructive comments on our manuscript. After carefully reviewing your comments and re-reading the paper, we realized that several parts of the paper may confuse readers, which requires a thorough harmonization of the paper. Thus, in case of a positive answer from the Editor, we will revise the manuscript according to your suggestions and comments.

In the following, our answers are written below each of your comments, reported in italic. Additionally, we have included our first responses to Referee #1 in this file, so that we can refer to this single letter for future communications.

Sincerely,

Mohammad Farzamian

on behalf of all authors

## Editor Comments

*Line 169: What does “nominal” mean in this context?*

The term “nominal” means that this value comes from the technical sheet of the manufacturer. We will delete it to avoid misunderstanding.

*Line 169: Per dripper, or per drip line?*

The flow rate of  $10 \text{ l h}^{-1}$  refers to the single dripper (see also the answer below about the lines 183-184)

*Line 172: Did the plastic not result in (approximate) hydrostatic equilibrium? In that case, the water content profile would not be uniform.*

We agree. The term uniform is wrong. The issue of the initial condition of the experiment was also raised by the Reviewer#1. See the answer below, we will provide details on the initial condition.

*Lines 183-184: One hour on, one hour off, for 21 hours, then 3 hours off?*

The plot was irrigated by 400 drippers on a  $0.2 \times 0.2 \text{ m}$  grid. They could thus deliver  $4000 \text{ l/h}$  on the whole plot. We applied eleven irrigation, each lasting about 3 minutes to deliver about  $180 \text{ l}$  on the whole  $16 \text{ m}^2$  plot for each irrigation (the volume was measured by a flowmeter). Irrigations were separated by a 1 hour shutoff. At each irrigation starting, due to the short inertia of the irrigation system just after its switching on, for some seconds drippers delivered less than  $10 \text{ l/h}$ . For each irrigation an average flow rate of about  $0.375 \text{ cm/min}$  was applied, which generated a small ponding at the soil surface for a short time.

We will eventually explain better the irrigation supply sequence in the revised version.

*Figure 2: Some elements of this figure are confusing. The map of the plot suggests that the TDR probes are installed vertically, but the cross-section shows they are installed horizontally, although we have to guess the depth.*

*The marker of cross-section A-A in the map has arrows that do not seem to have a purpose. Nor does the cross-section match with the map layout.*

*According to the map, the CMD is rotated horizontally, with the coils remaining vertical in both positions.*

*The arrows on the cylinder oriented top-to-bottom do not seem to have a purpose. 'HCP' and 'VCP' in the map appear to refer to the same position of the CMD.*

The TDR probes are installed horizontally at 20 and 40 cm depth. The experimental scheme will be simplified and made clearer.

*Line 231: What about the interface between the soil and the bedrock? Does that boundary not violate the smoothness requirement?*

This issue has been discussed in the conclusion #4 and is a source of uncertainty. However, the results of inversion shown in Figures 4 and 9 suggest that the interface was relatively well resolved. We will further discuss it in the revised version.

*Line 243: Perhaps this needs more elaboration.*

We will provide more details in the revised version.

*381 The rationale of the paper was to provide a large-scale non-invasive method for determining soil hydraulic and solute transport properties. Does the fact that you have to rely on bedrock parameters (presumably measured invasively and destructively) not negate that objective?*

This issue was also posed by the reviewer#2. There we maintained that the a-priori characterization of the bedrock layer is not really necessary and the proposed procedure holds independently on the presence of bedrock. Actually, we could have treated the bedrock layer as any other layer in the soil profile. The problem of this specific experiment was that inserting TDR probes and tensiometers into bedrock presents difficulties. So, we decided to fix the bedrock parameters to the values already available from independent measurements. Of course, this is only a special case due to the presence of a shallow bedrock. In different soils with either deeper or absent bedrock, we could have inserted TDR probes into any layers of the profile and validate the procedure for any of them.

Minor comments concerning the misspellings and the symbols will be considered and, accordingly, we will modify the text.

## Anonymous Referee #1

*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

- 1. 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.*

Thanks for the constructive and very relevant comment. Although in principle, we are perfectly aware of some of the points raised by Reviewer#1, we would like to give our reasons why we decided for using an uncoupled approach. In doing that, the response will unavoidably answer other important issues introduced by Reviewer#1, for example about the number of hydraulic parameters to be estimated by the inverse hydrological procedure. Let's briefly introduce the main steps of the coupled and uncoupled approaches, by simultaneously highlighting their strengths and weaknesses. In order to remain close to the case examined in the paper, we will refer to the case of EMI apparent electrical conductivity readings,  $ECa_{meas}$  (the geophysical measurements) during a 1D vertical water infiltration experiment.

### Coupled approach:

In the coupled approach, the hydrological model is the starting point of the procedure. Guess values of hydraulic parameters are initially fixed; thus, a hydrological simulation is carried out producing water content distributions along the soil profile, evolving over time. These water content distributions are converted to corresponding distributions of bulk electrical conductivity,  $\sigma_b$ , by using some empirical relationships. The  $\sigma_b$  distributions, in turn, are used as input in an EM forward model to produce estimations of apparent electrical conductivity,  $ECa_{est}$ . In the inversion, the objective function involves the residuals ( $ECa_{meas} - ECa_{est}$ ). This objective function is eventually minimised by optimising the hydraulic parameters in the hydrological model.

The main strength of this approach relies on the fact that no EMI inversion is required. It is well known that the inversion of geophysical measurements is an ill-posed problem. Ill-posedness is generally treated by regularising the inverse solution. However, different regularisation schemes and parameters can have a

significant impact on the results (e.g. Dragonetti et al., 2018; Zare et al., 2020); thus, inversion results of EMI data are always affected by uncertainties which can lead to artefacts, misinterpretations, and unphysical results (Camporese et al., 2015). These uncertainties can be minimised in case of prior information about the physical system under study. As discussed by Hinnell et al. (2010), the attractiveness of the coupled approach is that the hydrologic model may provide the physical context for a plausible interpretation of the geophysical measurements. And yet, this strength is counterbalanced by a couple of weaknesses:

Firstly, at least for the EMI measurements, an instrumental shift in ECa readings can be frequently observed due to several well-known reasons, which is not the case to discuss here. The issue has also been raised by Reviewer#1, who correctly cited the work by von Hebel et al. (2014) where the authors used ECa values coming from the ERT measurements to remove the observed instrumental shift and correct the measured conductivity values by linear regression. The issue has been deeply discussed in the paper by Dragonetti et al. (2018). Firstly, this means that EMI-based readings do not immediately provide correct electrical conductivity distributions. Related to this and much more important for this discussion, if the shift in EMI readings was not removed, the coupled approach would necessarily lead to wrong and probably physically implausible, hydraulic parameters, as the ECa coming from the hydrological model would be forced to correspond to incorrect measured ECa distributions. Thus, the coupled approach always requires an independent dataset, obtained by different sensors (ERT, TDR, sampling) to remove the shift in the EMI-based ECa readings. This would contradict the spirit of our paper, which mainly aims at minimising the sensors and the data necessary for in-situ soil hydraulic characterization. Furthermore, the use of another methodology does not provide the true ECa data (See for example, the detailed answers below for the use of ERT), which further limits the application of coupled model. Another related problem lies in the empiricism involved in removing the shift, which means renouncing to understand its physical effects on the final water content and, from this, on the hydraulic property estimations. This will be explained better in the case of uncoupled approach);

Second, with the coupled approach, there is a problem related to the number of hydraulic parameters to be simultaneously optimised. In the ideal case of a single soil layer to be characterised (but quite unrealistic for a natural soil profile), the hydraulic parameters to be optimised are relatively limited (say at least  $\theta_s$ ,  $\alpha$ ,  $n$  and  $K_0$ , for the case of the unimodal van Genuchten-Mualem hydraulic properties, taking  $\theta_r$  and  $\tau$  fixed). If more than one layer has to be characterised, the coupled approach requires that all the parameters have to be simultaneously optimised. This is the only way to have the ECa imaging of the whole soil profile be compared to ECa readings in the objective function. In this case, the number of parameters increases significantly to a not manageable number and may well produce problems of parameter correlation, uncertainty, and non-uniqueness of the solution. This is exactly the issue raised by the Reviewer#1 for our approach (later, we will explain how this problem may be minimised in the case of the uncoupled approach).

Uncoupled approach:

In the uncoupled approach, the geophysical model is the starting point of the procedure. As a result of geophysical inversion, the  $\theta_b$  distributions are derived, which are then converted to as many distributions of water content. Let's call them "measured water contents",  $\theta_{meas}$ . These, in turn, are used as input in the hydrological model. Starting from initial guess values of the hydraulic parameters, the latter produces estimated water contents,  $\theta_{est}$ . In the inversion, the objective function involves the residuals ( $\theta_{meas} - \theta_{est}$ ). This objective function is eventually minimised by optimising the hydraulic parameters in the hydrological model.

The main weakness of this approach corresponds to the strength of the coupled approach. The uncoupled approach requires geophysical inversion, involving all the problems discussed above about uncertainty coming from the problem's ill-posedness. However, it should be noted that the methodology we propose in our paper does not require preliminary removal of the (unknown) shift in the EMI readings. Conversely, the shift effect is implicitly kept in the ob distributions, from this in the “measured” water content distributions and finally included in the hydrological inversion. This allowed us to see the effects of the EMI shift (as well as the technical limitations of the EMI sensor itself) in the water content estimations and from this in the hydraulic properties’ estimation. In the first case, by comparing the EMI-based water contents to the water contents coming from TDR, it was possible to see that the shift in the EMI readings produced parallel water content evolutions, thus meaning that the EMI shift is rather stable with water content change.

Related to this, in terms of hydraulic properties, the shift simply results in scaled saturated water content. Reviewer#1 considers this finding quite implausible. And yet, it may well be explained physically by just considering that the parallel behaviour of the water contents over time, signifying similar water content changes over time. This is translated in similar hydraulic conductivities, which in the van Genuchten-Mualem model means similar  $\alpha$  and  $n$  parameters, and thus water retention curves are simply scaled by the saturated water content ratio. All this (physical) behaviour would have remained completely hidden if the EMI shift had been removed.

The uncoupled approach may also allow for reducing the problems of parameter correlation and uniqueness, as it allows for sequential (from the upper to the lower horizon) parameter estimation. In the methodology proposed in the paper, the parameters were determined separately for each horizon of the profile. First, the parameters for the topsoil were estimated on the basis of the water contents “measured” in the first layer. Then these parameters were treated as known and those for the second layer were estimated in a similar way on the basis of the water contents and pressure heads in the same layer. According to Abbaspour et al. (1999) and Coppola et al. (2004), this approach makes parameter estimation of multi-layered profiles more feasible and accurate, however, this approach cannot be done within a coupled model, as already discussed above.

In any case, we recognize now that the motivation of our choice in applying an uncoupled inversion approach without calibration of EMI data should be provided and better justified in the revised version of the manuscript.

- 2. 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.*

The addition of synthetic modelling will make the paper extremely long. If we did not have any other sources of data with which to compare our findings, that would be essential. The EMI-based findings were compared with those obtained from in situ TDR and tensiometer measurements as well as those obtained from laboratory analysis. Please see also our detailed answer to the previous comment as in our approach the hydraulic parameters were estimated separately for each horizon.

3. *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.*

We would like to point out some points about another major comment made by Reviewer #1 (Using ERT as a reference to calibrate ECa data prior to quantitative investigation). The authors have extensive experience with the ERT method and are aware of its pros and cons. Even though the ERT can be used to "evaluate" ECa data and correct the drift of the ECa measurements, calibration of ECa data using the ERT models has some limitations and does not necessarily provide corrected ECa data required for a couple approach:

- a) To obtain subsurface electrical conductivity distribution from ERT data, ERT data must be inverted. However, the inversion process is a highly nonlinear problem and the inverse solutions require regularisation procedures that impose additional constraints (e.g. smoothing). In the first general comment, Reviewer#1 correctly pointed out the same source of uncertainty. Other researchers that used the ERT method addressed this issue (e.g. Singha and Gorelick, 2005; Cassiani et al. 2012). The potential electrical static shift of apparent resistivity data or other sources of uncertainty (e.g. Ritz et al., 1999, Meju 2005) might need to be investigated in the case of ERT survey. These points obviously limit the use of ERT to calibrate other methods, especially in a "coupled" approach.
- b) In monitoring experiments, such approaches are difficult to implement because the ECa ranges will change significantly during the experiments, and ERT measurements prior to the infiltration experiment is likely not sufficient. In other words, the calibration equation developed prior to the infiltration experiment cannot be clearly applied due to the lower values of ECa (and also smaller ranges of ECa changes) than later to be measured during the infiltration (see for example figure 8).

#### SPECIFIC COMMENTS

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

We agree, we will revise the abstract and will shorten the text in the revised version.

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

We agree, we will revise the sentence in the revised version.

*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).*

We agree, we will revise the sentence in the revised version.

*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.*

We agree, we will revise this paragraph and include the relevant references.

*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.*

We may have used the terms "plot scale" and "field scale" incorrectly in the paper, which may have led to Reviewer#1's question. As Reviewer#2 clearly states, this mixing of terms causes confusion for the reader of this paper. Eventually, this issue will be resolved in the revised version of the paper. It is true that the ERT can be used for the 1D approach suggested in this paper, even if it is more physically demanding than EMI. In the revised version, we will clarify the advantages and limitations of each method in different scales of studies.

*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..."*

Thanks. We will revise the sentence in the revised version.

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

Thanks. You are right. We will rewrite this sentence.

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

Thanks. We will add sentence reporting the initial water content

*Line 181. Please provide manufacturer of these sensors.*

Yes. We will add this information in the revised version.

*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.*

From several experiments and measurements performed on the same soil, the EC of the soil water was around 0.7 dS/m.

*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.*

We will better specify the measurement schedule.

*Line 184. I guess it was 2000 dm<sup>3</sup> on 16 m<sup>2</sup>? Perhaps already provide units in mm (or m) given that you will be using 1D modelling afterwards.*

We will convert the units in order to be consistent throughout the paper.

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

You are right. The sentence is not very clear. Definitely the experiments are two (see fig 1 with the schematic diagram of the proposed approach). We will better clarify this part.

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

S is the sand content. We will specify in the text.

*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.*

We will include this information more clearly in the revised version.

*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.*

We will revise this text to avoid confusion. The number of layers and its thickness are equal, but the conductivity of each cell is constrained vertically (spatial constrain- as it is 1D inversion) and laterally (temporal constrain).

*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?*

We have performed synthetic tests (Generating ECa synthetic data for both experiments) and tested several different parameters. The results of these tests were satisfactory, convincing the authors that the algorithm would be a better choice compared to independent 1d inversion. We will describe this better in the revised version and discuss the points raised by the reviewer.

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

We tested several different parameters in our synthetic tests and obtained the best value. We will describe this better in the revised version.

*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?*

Yes, we used eq. 1 with the  $\sigma_w$  of the tap water. We used the Topp universal calibration to convert  $\epsilon_b$  to  $\theta$ , as the electrical conductivity is enough low to exclude any salinity effects on the  $\sigma_b$ - $\theta$  relationship

*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.*

Yes, we applied the inversion algorithm implemented in HYDRUS: in the revised version we will better explain the applied procedure.

*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.*

The parameters were determined separately for each horizon of the profile. Please see our detailed answer to the first general comment. There, we argued that this is the main advantage of the uncoupled model allowing to characterize each layer separately. Concerning the bedrock properties, they were known from previous characterizations. Thus, the bedrock hydraulic parameters were not estimated by the inverse procedure but were fixed to the known values. In any case, this is not strictly necessary, as these parameters may be estimated exactly as the parameters of the upper layers. This question was also raised by the reviewer#2.

As for the initial conditions, at lines 205-207 of the original manuscript we stated, “At the end of the 1<sup>st</sup> water infiltration experiment, the soil was allowed to dry again (by drainage and evaporation) to bring the distribution of water content along the profile similar to the initial one (observed before the water infiltration test).”

Anyway, the initial condition was set in terms of pressure heads measured by the tensiometers. The pressure head in the bedrock layer was assumed to be in hydrostatic equilibrium with that measured at 40 cm. We will give more details on the initial conditions in the text of the revised version.

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

After this comment and that below (about line 381) on the hydraulic conductivity value, we realized that the Bw label used in the table 1 may have been misleading. Actually, with A and Bw we are indicating the horizon including the 20 and 40 cm depth, respectively. So, in the table the Ap and Bw soil layers should be seen as 20 and 40 cm depth. For these two depths, the dispersivity values are different.

The bedrock parameters are not shown in table 1. We will report them in the new table 1 in the revised version of the paper. Labels in the table will be changed to make them consistent with the text.

*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).*

We will discuss it in the revised version. Agree, it could be due to the drift of 1 or more channels of the EMI measurements.

*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.*

It is a very good suggestion. We will use the EMI measurement we did at  $t=0$  to confirm the goodness of the initial condition we used for the bedrock.

*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.*

During the infiltration experiment, we supplied 2000 litres of water, which, given the area of the plot, corresponds to 125 mm. It should also be considered that the  $K_s$  is only 3.6 cm/h. Thus, it is expected this water will not be able to significantly change the water content at 80 cm, as shown in the figure 4. The lateral flow should be reasonably excluded. It is true that the TDR probes were installed in the corners of the plot but in any case, they were at 1m from the plot edge, so that any lateral boundary effects should be excluded. However, the fact that the TDR and EMI measurements showed similar changes of water content over time and that the EMI was placed in the middle of the plot, would confirm that no lateral fluxes occurred during the experiment.

*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.*

Sure, we will add this information.

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

We will revise this interpretation and add more critical discussions about the misfit and potential drift of ECa data.

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

Yes, we agree.

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

We will show the pressure head readings and model fitting

*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.*

Yes. We will clarify this in the text.

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

Thanks. We will move this information to M&M section

*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.*

After this comment and that above (about line 304) on the single dispersivity value, we realized that the Bw label used in the table 1 may have been misleading. Actually, with A and Bw we are indicating the horizons including the 20 and 40 cm depth, respectively. The bedrock parameters are not shown in table 1.

So, we will clarify this at the beginning of the M&M section, with a clearer and detailed description of the pedological profile. In the table 1 of the revised version we will add the parameters of the bedrock and will show that, actually, the bedrock has a high hydraulic conductivity as it is a fractured calcareous layer.

*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.*

We will add the water content evolution in the bedrock in the figure.

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

Similarly, to the answer given for the comment of line 287, we will better explain the procedure of inversion.

*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.*

For sure we can change the reference to the depth to the name of the horizon. However, we need to differentiate the hydraulic properties estimated by the inversion of the TDR data from that inverted by the EMI data. For the sake of comparison, this difference must be reported in the legend.

*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?*

Please see the answer to comment about line 294.

*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.*

This could have happened as the soil water content was in the range of 0.2-0.3 within the depth 0-50 cm. In any case, we recall that at the beginning of the two experiments the water content profile was very similar (as checked by the TDR probes and tensiometers readings). The higher background electrical conductivity observed at higher depths in the second experiment compared to first one has to be ascribed to the drainage water coming from the first experiment. We will eventually go a bit deeper in this issue in the revised version

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

Yes, as already discussed to answer to your question in the line 335, we will add the error bars.

*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.*

Please see our detailed answer to the first general comment. As uncoupled approach was used, it did not require to include information from deeper layers. On the other hand, the idea was to compare our findings with TDR and tensiometer in-situ measurements which were not available at deeper layers.

## Anonymous Referee #2

*The paper presents a way to interpret electromagnetic induction data (i.e., bulk soil electrical conductivities) estimate soil hydraulic properties in the field, at roughly the scale of the soil profile. The paper argues this is relevant for optimizing water use in irrigated agriculture. It reports on field experiments involving infiltration of solute-free water to monitor the wetting front, followed by infiltration with saline water to monitor the effect of the salt on the electrical conductivity. An attempt is made to derive plot-scale (not field-scale) soil hydraulic and solute transport parameters to assess the potential of non-invasive EM measurements for practical applications in irrigated agriculture without exploring this in detail.*

*"In the Introduction, the authors appear to tweak their interpretation of the literature to suit their needs, resulting in some claims that are debatable or even incorrect. I noticed numerous self-citations. In fact, 28 of 59 references (47%) are self-citations! I think this is a record in my multiple decades as a reviewer. It probably would not hurt to look at the works of others to have a more balanced overview of the state of knowledge."*

As for the problem of the references, we fully agree with Reviewer#2 that the paper Introduction includes an exaggerated number of self-citations. This happened as the paper has been written by "many hands", each tempted of citing his/her own work. None of the authors realized this before submitting the paper. We apologize for this inconvenience and in case of positive feedback from the Editor, in the new version of the manuscript we will follow Reviewer#2 suggestion of balancing the references about the state of knowledge. In the following, we will attempt to show, although disproportionate, why many of the references cited in the hydrological part of the paper are not superfluous. Actually, most of the issues raised by the Reviewer#2 about field and plot scale, observation window of the sensors, the relationship between the lab and in-situ measurements, local scale dispersivity, change of dispersivity with the scale, the role of heterogeneities at different scales on the transport, all have been dealt with deeply by the authors in the last twenty years in the papers included in the references.

Besides, to us the first statement of the Reviewer#2 "*the authors appear to tweak their interpretation of the literature to suit their needs, resulting in some claims that are debatable or even incorrect*" appears to be very dense and deserves a deep discussion that will provide responses to other issues raised by the Reviewer#2. This statement appears to us also a bit extreme and awkward. We may accept that some claims are *debatable*, not they are *incorrect*, as we will try to demonstrate in detail in the following answers. Most of the topics dealt with in the paper belong to a so uncertain world that, having too many certainties may be slippery. However, this is only a formal issue.

Let's discuss the issues raised by the Reviewer#2.

*"1. 51-58. Richards' equation (RE) applies at the Darcy scale. The smallest scale at which it can be applied is the scale of the representative elementary volume. The largest scale is not as well defined, but it is clear that at some point the proportionality between the water flux density and the hydraulic gradient will break down because of soil heterogeneity, different flow directions within the volume of interest, etc. The fact that RE is used at the field scale does not imply it is assumed that it is valid there, but that there is no alternative yet. This can be gleaned from the way RE is used at the field scale: the problem is used to one dimension, and it is hoped the properties of the soil and the vegetation (usually a crop) were chosen such that the results reflect the field scale, without actually modeling the entire field."*

Actually, in the paper, we stated: “Soil hydrological behavior is generally described by solving the Richards’ equation (RE) for water flow and the Advective-Dispersive equation (ADE) for solute transport, which is frequently assumed to apply at different spatial scales, from laboratory to field to larger scales (Sposito, 1998).” To us, this is only a semantic issue. We need to agree on the sense of the verb “assumed”. To us, this does not mean at all that is true or valid, it simply means that it is applied in practice and thus it is implicitly accepted. Even if this issue is not the core of the paper under review, we want here to discuss some more details of our thought about it:

In the incipit of a paper published some years ago, Coppola et al. (2009) (a paper not cited in the references of the manuscript under review) the authors stated: “... Though basically unproven, extrapolating the theory for the nonlinear unsaturated flow process to a larger-scale system is common practice and the “Darcian” scale governing equation for water flow in soils is assumed to be applicable at any scale (Kabat et al., 1997)”. At the Darcian scale, the applicability of the Richards equation is universally accepted. It is also well known that at larger scales (field, watershed and regional scales) it may lose its physical meaning due to the deviations from linearity of the relation between the water fluxes and soil water pressure gradients (Beven, 1994), so that different mathematical descriptions may be needed to describe the different physical processes which dominate at each scale level.

Looking for modelling water fluxes at scales larger than the Darcian scale, to be rigorous one should approach the modelling by a fully 3D RE numerical implementation with local scale discretization, which for watershed simulations would require a hugely high resolved grid-scale. This is why process-based watershed models (see for example Refsgaard and Storm, 1995) frequently apply the Richards equation at a grid-scale much larger than the local scale, thus implicitly accepting that the unsaturated-zone flow processes are scale-invariant. The issue has been deeply discussed for example by Harter and Hopmans (2004). Besides, in agricultural applications, models such as SWAP (Kroes et al., 2017), based on the Richards equation, are more and more applied at the scale of the agricultural fields. This is precisely what we mean with the word “assumed”.

By accepting this (which, as also argued by Reviewer#2, *remains an unproven convenient hypothesis*), the problem remains to find the soil hydraulic properties suitable to solve the Richards equation at the field scale or the watershed model grid-scale. As these properties are generally measured at local scale (either laboratory or in-situ methods), the further issue is how lab measurements are related to field measurements (see Basile et al., 2003; 2006) and how both are related to the properties effective at the scale of application of the Richards equation. This opens another research world (stochastic upscaling, MC analysis, etc.) (see Bresler and Dagan, 2003; Yeh et al., 1985; Coppola et al. 2009, among the many others).

Based on his/her comments, we think the reviewer knows very well how all these issues remain challenging and there are no certainties on what is really correct and what is not. We also believe that the reviewer is perfectly conscious of the fact that all the previous discussion is almost completely out of the scope of the paper under review. That’s why in the paper all these issues were limited to a couple of statements: “Richards’ equation (RE) for water flow... is frequently assumed to apply at different spatial scales, from laboratory to field to larger scales. ... RE requires the soil water retention and the soil hydraulic conductivity functions to be known at the scale of concern.” to implicitly say all we said above.

*“The author’s statement that the ADE is assumed to be valid at any scale is incorrect. There is a rich literature spanning several decades (especially in groundwater hydrology but also in soil physics) about the increase of the dispersion coefficient with solute travel distance, and concepts such as the fractional*

*advection-dispersion equation and Continuous Time Random Walk have been proposed to remedy the problem. The dilution theory proposes a different mixing process than diffusion. In soil physics, the solute spreading proportionality to the square root of time (for steady flow) consistent with the ADE has been challenged. Its alternative, stochastic-convective solute spreading, lets solute spread proportional to time. A google-scholar search with these terms will provide ample references.”*

Most of the discussion already done for the RE also applies here. ADE is the universally accepted local-scale mathematical model of transport. Again, it may not work at larger scales, characterized by increasing levels of heterogeneities, both in the vertically and horizontally directions. This would introduce a scale effect on the dispersion, which would tend to increase with the overall dimension of the region through which solute transport occurs, as also argued by Reviewer#2. And yet, may be again for convenience, ADE has been frequently applied for transport at larger scales, but with a scale dependent dispersion coefficient (see, Mishra and Parker, 1990; Gelhar et al., 1992; among the many others). Beven et al. (1993), argued that “ ... the ADE provides the mean transport and dispersive characteristics necessary to predict solute transport, provided that it is possible to estimate appropriate 'effective' values for the dispersion coefficient”. Pickens and Grisak (1981a) have analysed the relationships between travel distance and dispersivity in hydrogeologic systems. These relationships have been integrated into ADE for simulation and evaluated in both lab and field experiments (Gelhar et al., 1992; Neuman, 1990; Pang and Hunt, 2001; Vanderborght and Vereecken, 2007).

In most cases, under partially saturated conditions, ADE has been frequently used in the field and on larger scales in a stochastic formulation (Sposito et al., 1986; Severino et al., 2010). For example, in the stream tubes approach, solute may move vertically in independent columns with negligible lateral mixing among columns. Transport in single columns may be described deterministically by the ADE whereas the field scale transport is described by considering the column parameters as realizations of a stochastic process. In the literature, there is plenty of papers based on this approach after the early applications by Dagan and Bresler (1979) and Bresler and Dagan (1979).

In any case, in the literature there is plenty of contradictory results on the scale effect on dispersivity, especially under partially saturated conditions. Many examples show that the dispersivity may not change or even decrease with depth (Jury et al., 1982; Butters and Jury, 1989; Porro et al., 1993). Coppola et al. (2011) analysed the evolution of the dispersivity with depth at three different scales (local, transport and transect scales) and found that the dispersivity decreases with depth practically at all the scales. This was explained by the higher water contents with depth, which, from a physical point of view, produces less tortuous paths and thus decreasing dispersivity at higher water content. A similar behaviour was found at different scales by Maraqua et al. (1997) and Nützmann et al. (2002).

Without the need for the “*google-scholar search*” suggested by Reviewer#2, the authors know very well the different models that can be used to describe transport, based on the propagation of the travel time (t) moments. For example, they know that the ADE and the stochastic-convective (SC) are not the only modes of propagation of the t moments and found that the variance of  $\ln(t)$  may decrease (ADE), be constant (SC) or may increase with travel distance (scale-dependent process) (Coppola et al. 2011).

As discussed above there is no certainty as to what is correct or what is not, and it is still an open debate. The same Continuous Time Random Walk (Berkowitz et al., 2001) has for sure contributed to gain more insights into the (groundwater) transport processes. However, it seems quite challenging to have physical explanations for some parameters used in these and other non-local theories. Meanwhile, even if related to aquifers, a quite recent study by Fiori et al. (2017) on the predictive capabilities of ADE with the first order

approximation of macrodispersivity proved that ADE was accurate enough for the predictions of solute plume in heterogeneous porous media.

Like for the RE, as the paper under review does not deal with this topic, we limited all this discussion to a couple of statements: “the Advective-Dispersive equation (ADE) for solute transport. It is frequently assumed to apply at different spatial scales, from laboratory to field, to larger scales, and the dispersivity has to be known at the scale of concern”, thus meaning that the ADE may be accepted but a macrodispersivity is needed for scales larger than the local one. The term assumed is again used to mean “accepted” not because it is true but because is frequently used in practice.

Of course, if requested, we will add a part of this discussion to the manuscript, even if it would seem a bit redundant given the objective of the paper. For sure, we will add some relevant references to the statements about the assumption of using RE and ADE at different scales

*“l. 67-69. It is unclear to me why the authors believe that different properties of soil layers are more important for solute transport than they are for soil water flow or root water uptake (or density of the root network, for that matter).*

In the text, we simply stated that determining the solute transport properties on isolated layers of a layered soil profile (for example from columns collected in each of the soil horizons) may make it problematic to reconstruct the transport behaviour of the whole soil profile. Once a travel time pdf is derived for each horizon, the solute transport along the whole soil profile requires an additional hypothesis on the correlation among travel times in the different horizons. The issue has been discussed deeply by Hamlen and Kachanoski (1992). In the case the travel times of a solute particle in one soil horizon are uncorrelated to the travel times in the other soil horizons, the travel times along the entire soil profile can be described by summing up the first and second moments of pdfs in the different layers. In the case of correlated layers, the covariance of the travel times in different layers is also required (see also Bancheri et al., 2021).

*“l. 70-83. I agree (and so do others) that lab-based soil hydraulic properties often transfer poorly to the field. But field method often has a limited range, which creates a risk when the soil dries out. Also, soil layering, soil heterogeneity within layers, spatial variation of the infiltration rate during an experiment, preferential flow, etc. all end up indiscriminately in the soil hydraulic properties determined from field experiments. This paper focuses on field measurements and aims to obtain from those the soil hydraulic properties at a more relevant, but at this point in the paper still poorly defined, scale. With this in mind, the paper cannot not ignore these issues because they are highly relevant for it. They should therefore be thoroughly discussed, not ignored.”*

Thanks for the constructive comment and sorry for the confusion. By reading again the paper, we fully agree that the manuscript induces some confusions about plot scale, field scale, in situ measured properties and so on. Firstly, we want to clarify that the paper deals with a non-invasive in-situ method for determining hydraulic properties and dispersivity at plot scale. however, we believe that the approach we used here can be considered a proof of concept and given the potentiality of the EMI technique, appears to be promising to larger scales (field, for example) characterization. In the revised version we will definitely clarify and discuss these issues.

As for the laboratory vs in-situ methods issue, of course, lab-based methods may be carried out under more controlled conditions. Nevertheless, we should agree that, if the properties to be measured have to be used for simulating water and solute dynamics in the real field context, the in-situ methods are obviously more

representative than the lab ones, mostly for the reasons mentioned by the Reviewer#2. In presence of heterogeneities, a water flow process in the real field context would be affected by those heterogeneities. The hydraulic properties will be able to describe that process only provided that they contain the information on those heterogeneities. An in-situ method may allow this. A lab method not necessarily. Let's consider, for example, a fractured soil. In the in-situ conditions, this will show some matrix polygons delimited by fractures. By collecting soil columns to be analysed in the lab it is likely that the column will not sample all the heterogeneities (both the matrix and the fracture) (this would not necessarily happen only in the rare cases one collects very large soil columns). By contrast, a classical in situ method, for example the well-known instantaneous profile method (Watson et al., 1966), would be able to catch the hydraulic properties which are effective in describing the flow process observed in situ. Of course, this will also depend on the measurement scale (the size of the plot) and on the observation scale of the used sensors. These issues have been dealt with in details for example in Coppola et al. (2012; 2016) and in Dragonetti et al., (2019).

Similar reasoning may be done about the experimental boundary conditions used to carry out the hydraulic characterization in lab and in situ (see again Basile et al., 2003; 2006). The latter work was just looking for a systematic approach to convert the lab measured properties to the field ones to make them more suitable to describe in situ processes.

*"l. 107-112. Quite recently, HESS published a paper related to the subject discussed here (Kim Madsen van't Veen, Ty Paul Andrew Ferré, Bo Vangsø Iversen, and Christen Duus Børgesen, Hydrol. Earth Syst. Sci., 26, 55–70, <https://doi.org/10.5194/hess-26-55-2022>, 2022). It would be nice to discuss this paper as well. If I am not mistaken, that paper does not delve into the soil hydraulic properties, but they examine the details of the measurements and the data inversion in some detail."*

We agree. We will discuss this interesting paper in the revised version.

*"l. 122-130. There seems to be a contradiction here. Even if you are able to find field-scale soil hydraulic properties with non-invasive techniques, you propose to verify these with small-scale sensors (TDR probes and tensiometers). But to obtain field-scale data with those you will have to install them at many locations at multiple depths, which will disturb the soil layers and the flow paths. Only later in the paper we learn that you are actually only monitoring smaller plots, with sensors at two depths on four locations, away from the area in which you use the non-invasive techniques, without moving the CMD instrument over the field. Please bring this section of the text in agreement with the experimental setup."*

Many thanks for this valuable comment. In the revised version of the paper we will briefly discuss the issue of using sensors of different observation windows. Please see our detailed answer to the questions from the lines 70-83. As discussed, we are not proposing the simultaneous use of small-scale sensors for future studies. Our aim is to show that the EMI-based results are comparable with those obtained from TDR and tensiometer measurements as proof of concept.

As for the disturbance to the flow paths, we have established experimental plots for hydraulic characterization by installing sensors at different depth quite long before carrying out flow and transport experiments, so that natural and wetting cycles may reproduce the normal soil aggregation. However, we are conscious that this may not solve completely the problem. We tried to compensate for it by installing sensors on a number of verticals and working on the average measurements. Unfortunately, there are no many alternatives to this approach. As for the relative positions of TDR and EMI sensors, Figure 2 provides a schematic view of set up (to be improved, see the Editor comment). We stress again here that the EMI sensor

was only used in the middle of the plot and was not moved over the field. We have not used the EMI for large scale monitoring. We are simply following a plot scale infiltration process by placing the EMI sensor always in the same position in the plot. Evidently, according to the Editor decision, all this part will be clarified and made coherent throughout the manuscript text.

*"l. 167. This is the first time you mention the size of the plots. It appears to me that your frequent use of the term 'field scale' above was a bit misleading. You are working on the plot scale, not the field scale. I do not believe this invalidates the work, or that the experiments were performed on too small a scale, I just think the terminology you use is unfortunate"*

Sure. The terminology issue will be eventually resolved in the revised version (see the comments above).

*"l. 184. 2000 liters of water translates to 125 mm, is that correct? I am optimistic that the design of your experiment ensured a uniform infiltration over the plot area."*

Yes, it is correct. The irrigation grid and all the irrigation control system were designed to obtain a high irrigation uniformity (not lower than 95%), by using auto-compensating drippers. In the revised text, we will give some details on this.

*"l. 199-200. This makes sense, but are you sure that the digging required to install the sensors did not affect the water flow pattern and wetting front velocity? In other words: are your reference profiles representative of the profile under the CMD mini-Explorer? I admit I do not really know how to avoid this, except by digging up the entire plot. But perhaps you installed the invasive sensors some time before to let the soil settle, perhaps aided by some wetting-drying cycles? I cannot tell from the text."*

See the answer above

*"l. 207-211. How enthusiastic will farmers be if you propose to them to apply saline water to their irrigated plots if they have high-quality irrigation water available? And how well does your method perform in plots that are already salinized to some degree?"*

As we clarified above, the method (and the use of the calcium chloride for the experiment) is simply thought to characterize a soil profile at the plot scale. Also, the calcium chloride after the experiment may be easily leached by the rainfall or irrigation water. As for the soils already salinized, we remind that we used a solution highly concentrated at 15dS/m, just to make the salt propagation "visible" even under relatively high background salinity

*"l. 220. For a paper that argues against lab experiments, it is surprising to see that you need to determine some model parameters in the lab after all. From what I understand, these parameters are indispensable for any location where you want to apply your method, so in addition to the effort you reported here, these laboratory measurements need to be carried out as well, and probably for every soil layer. But your emphasis on transferability to the field implies you need to know the spatial variation of these parameters as well. All in all, how much additional time, money, and resources are necessary for this aspect of the work?"*

The lab experiment for such analysis is quite simple, fast, and standard procedure on reconstructed soil samples. Furthermore, soil sampling and laboratory analysis are always required when geophysical methods

are applied to hydrology (hydro-geophysics) or agriculture (agro-geophysics), since geophysical methods are indirect measurements, but the point is how these methods could reduce the laboratory work and related expenses. We'll clarify the required laboratory work in the revised version.

*"l. 222 'concentrations, Cl-, to sigma-w'. Unclear. Do you mean 'concentrations of Cl- to sigma-w'?"*

Yes, you are right, it is the concentrations of Cl- to sigma-w'

*"l. 331 (Fig. 4). Does the wedge at about 0.3 m depth in the first four hours of the experiment perhaps indicate that preferential flow rapidly carried water to this depth, wetting it faster than the top soil? It appears in Fig. 9 as well. The dispersivity of the top soil in Table 1 is very high, which points to the possibility of very non-uniform vertical flow rates consistent with preferential flow.*

Comparable values of dispersivity under partially saturated conditions were found in similar soil by Coppola et al (2011) who related the relatively high dispersivity values to occurrence of fingering flows.

*l. 336-339. And in addition you have the difference between disturbed and undisturbed soil in this case. Would it have been worthwhile to apply the EMI sensor above the TDRs, or would the metallic sensor have corrupted the measurements even if they had been temporarily shut off?*

An EMI sensor is not influenced by a metallic sensor only if it is switched on but simply because it is metallic

*"l. 361. The difference between the water contents is not slight, especially if it is used to time and optimize irrigations. See the comment on Fig. 6 below. "*

*"l. 370 (Fig. 6). If the differences between TDR and EMI are indicative of the error of the EMI, than the water availability in the root zone will be severely underestimated, so the use of such data in irrigation optimization will be very limited unless the farmer learns by experience to interpret the data correctly. But then, all this effort is unnecessary: even without all the modeling I suspect a farmer will figure this out after a few growing seasons. I find it difficult to reconcile this result with the rationale expressed in the Introduction."*

We already clarified this point above. We are looking for developing an in situ method based on the use of the EMI for determining hydraulic properties and dispersivity at plot scale. The EMI is used here only as a method to follow the evolution of the water content and solute concentrations along the soil profile in the plot to be characterised. In this sense, the paper does not deal at all with the issue of using EMI for either the irrigation or salinity monitoring.

This interpretation may come from some misleading statements about irrigation management we used in the paper, mainly in the introduction and conclusion sections. The EMI was used here only as a method to follow the evolution of the water content and solute concentrations along the soil profile in the plot to be characterised. Our simple thinking is that once the soil hydraulic and dispersive properties have been estimated, they can be used as inputs for simulating many management scenarios, including irrigation management. We agree that the text needs more clarifications, and we will, eventually revise the text to avoid confusion.

It is also important to highlight the fact that underestimation of the water content has been widely addressed in other studies during the last two decades in the application of geophysical methods to hydrogeology (e.g., Binley et al. 2002; Singha and Gorelick 2005; Deiana et al. 2008; Haarder et al. 2012; Cassiani et al. 2012).

This did not prevent hydrogeophysicists from applying these methods to hydrogeology studies, but instead encouraged them to find solutions to identify the error sources and taking them in consideration when a quantitative investigation is required.

*"l. 380. Fixing the residual water content at zero (or at any other value) affects the ability of the retention curve to adapt its sigmoid shape (Groenevelt, P. H. and Grant, C. D.: A new model for the soil-water retention curve that solves the problem of residual water contents, Eur. J. Soil Sci., 55, 479–485, <https://doi.org/10.1111/j.1365-2389.2004.00617.x>, 2004)"*

Because in the first attempts the residual water contents converged towards zero, we have decided to fix it to zero for reducing the correlation among the other parameters.

*"l. 381-382. So, apparently you need to know a priori the soil hydraulic properties of the deeper soil, presumably measured on soil cores in the lab. This is the second instance where considerable additional effort is needed for your field method to be operational. Should the conclusion therefore not be that field-only methods are not realistic and a substantial effort in the laboratory is needed as well? In addition, these extra requirements muddle the scales on which you purport to work, and negate your claim that you can work with non-invasive, fast techniques."*

It is not really necessary. We could have treated the bedrock layer as any other layer in the soil profile. We fixed them as they were already available. In any case, we would like to point out that any hydraulic characterization should be always preceded by a pedological study.

*"l. 384-385. On what basis can you claim the difference between the observations at 40 cm is acceptable and the EMI estimates at 20 cm were proper? As I argue above, the differences lead to large errors in the estimation of plant-available water in the root zone. Your statement in l. 401-403 about the different flows for EMI- and TDR-based properties illustrates my point."*

The method is not looking for estimating the actual water content in the soil profile. Please see our detailed answer to the questions from the lines 370 (Figure 6). In this sense, the statement at lines 384-385 may be confusing and is also not useful for the development of the proposed method. We will revise the text accordingly.

The ultimate goal of the method is determining the soil hydraulic and dispersive properties. The fact that the EMI detects lower water contents simply translates to wrong hydraulic properties, which can be converted to those ones would have measured by TDR by scaling them through the ratio of the saturated water contents.

*"l. 404 (Table 1). The values of n seem high for a silty loam, as does the saturated water content. In the A-horizon, there could be an effect of tillage, but in the B-horizon I am not sure what is going on."*

The values of "n" are not so high. On similar soils, we get comparable values obtained by direct measurement by a tension table apparatus. Also, by looking at for example at the ROSETTA Class Average Hydraulic Parameters(<https://www.ars.usda.gov/pacific-west-area/riverside-ca/agricultural-water-efficiency-and-salinity-research-unit/docs/model/rosetta-class-average-hydraulic-parameters>), average n values of about 1.65 may be found for silty loam textures. As for the  $\theta_s$  value, one should consider that we are speaking

about soils with relatively high limestone content, which may well induce a significant soil structural component and thus high soil porosity.

*“l. 489. I readily believe if you measure solute concentrations in an entire field you can find such high dispersivities because they represent the soil spatial variability. But how large are the columns you mention? Several square meters diameter perhaps, possibly with preferential flow paths?”*

We mentioned in the text that the values we found are consistent with those found by Vanderborght and Vereecken (2007) under saturated and partially saturated conditions and by Coppola et al., (2011) under partially unsaturated conditions. Both the papers report dispersivity values obtained at different scales. For “column scale (undisturbed soil monoliths with a length > 30 cm)”, Vanderborght and Vereecken found values in the order of 10 cm. The same values were found by Coppola et al. at both plot and transect scales.

*l. 518-525. Are these claims tenable if you need to have available the soil hydraulic properties of the deeper subsoil and calibrated parameters of your electrical conductivity model? Also, the discrepancy between the water contents is such that the calculated flows differ widely, as you state yourself.*

See the comments above

*l. 530-531. In solute transport studies in the unsaturated zone, the dispersivity is not that important because the flow dynamics determine most of the transport. In groundwater hydrology, with much less variable flows, the dispersivity is indeed important.*

With such dispersivity values, we believe that dispersivity may have an important role in the transport. Obviously, this depend on the Peclet number

*l. 554. I agree that you can cover a large area with EM methods. But your study did not use that advantage. Given the differences in the water contents, could one perhaps argue that repeated use of the same EM device by the same operator on the same field(s) could lead to an empirical ‘feel’ to time irrigations based on EM data alone, without a full-fledged monitoring and modelling effort behind it? An operational use of the instrument, so to speak.*

Please see our detailed answer to the questions from the lines 70-83. We agree that the text need to be thoroughly revised in this regard.

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