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Comment on hess-2022-12

Anonymous Referee #2

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-RC2>, 2022

Review of 'In-situ estimation of soil hydraulic and hydrodispersive properties by inversion of Electromagnetic Induction measurements and soil hydrological modeling' by Giovanna Dragonetti, Mohammad Farzamian, Antonio Coppola, Angelo Basile, and Fernando Monteiro Santos

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. A few examples are detailed below.

I. 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.

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.

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).

l. 70-83. I agree (and so do others) that lab-based soil hydraulic properties often transfer poorly to the field. But field methods often have 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 ignore these issues because they are highly relevant for it. They should therefore be thoroughly discussed, not ignored.

l. 96. 'a field scale' change to 'the field scale'

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.

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.

I. 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.

I. 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.

I. 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.

I. 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?

I. 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?

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

I. 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.

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?

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, then 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.

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)

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.

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.

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.

I. 470 (Fig. 11) Do you have an explanation for the dip in Cl concentrations after 2 hours for the EMI-based estimates?

I. 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?

I. 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.

I. 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.

I. 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.