The manuscript presents an interesting integration of EMI monitoring and hydrological modelling to interpret infiltration experiments. The experimental effort is substantial and the coupling between EMI inversion and Richards-based simulations is potentially valuable. However, several parts of the manuscript are difficult to follow, some technical statements are inaccurate or misleading, and some interpretations require better justification. I therefore recommend moderate revision before publication.

We sincerely thank Reviewer 2 for the careful and constructive evaluation of the manuscript. We especially appreciated the recognition of the value of the experimental dataset and of the potential of integrating EMI monitoring with Richards-based hydrological modelling.
All the comments helped us identify conceptual imprecision in the description of EMI measurements, as well as an incorrect interpretation of VCP and HCP sensitivity, which was also noted by the reviewer1.
Below, we provide a point-by-point responses and proposed manuscript changes. All the comments done by the reviewer are reported as italic text. The parts which have been included in the revised manuscript are reported between quotation marks

Specific comments

1. Title: the title is rather long and does not clearly convey the main contribution of the paper. It would benefit from being simplified and emphasizing the methodological novelty rather than the experimental setup.

New title: Soil hydrological modelling as a tool to scale EMI-estimations of soil water content to direct measurements

2. Introduction (lines ~55–60)

The sentence stating that "σa depends on σb" is conceptually imprecise.
Strictly speaking, apparent conductivity is the depth-integrated response of the true bulk conductivity distribution. Since σa is simply a measurement of σb weighted by the instrument sensitivity function, saying that one "depends" on the other is misleading. I suggest rewriting this section more rigorously.

In the revised manuscript we now wrote:
“Because σ_a is determined by the vertical distribution of σ_b,....”

3. Salt concentration discussion (lines 65–70)
The paragraph discussing high salt concentration is not completely clear.
Since the manuscript later assumes a linear θ–σb relationship, it would be useful to better explain under which assumptions the linear approximation remains valid and why salinity effects can safely be neglected in this experiment.

Yes, in principle we agree. Our statement would suggest that linearity is necessarily expected only under low-salinity conditions. This is not correct. Rather, we should have said that the low and relatively uniform salinity makes variations in σb predominantly dependent on water content, allowing an approximately linear empirical relationship over the observed range. The introduction may not be the appropriate place to discuss this point. We have therefore decided to simply remove the adjective "linear," deferring its discussion until Figure 5 is presented. There, in the revised manuscript, we wrote:

“A clear positive and linear relationship was found over the investigated water content range, with a coefficients of determination, R^2 = 0.84. The initially low and relatively constant soil salinity likely limited the influence of pore-water electrical conductivity on σb, so that its variations were mainly associated with changes in water contents. The fitted equation was therefore adopted as a site- (and experiment-) specific empirical calibration for converting TDR-based σb to θ”

4. Instrument height (around lines 90–95)

The statement that collecting measurements at different instrument heights provides different depths of investigation is misleading.
Changing the instrument height mainly modifies the sensitivity function and reduces the overall response amplitude; it does not simply increase the investigation depth. The wording should therefore be revised.

We rephrased the whole section of the manuscript:

“This is because each coil setup has a different spatial sensitivity function and samples a different, although partially overlapping soil volume. As an example, the well-known CMD Mini-Explorer EMI sensor (GF Instruments, Czech Republic) may take σ_a measurements over six different depth sensitivity ranges. Using more σ_a readings in an inversion procedure for the same profile enhances inversion reliability. Of course, a different spatial sensitivity function may also be obtained by using multiple sensors simultaneously or by collecting measurements with a single sensor at different heights above the ground. However, changing the instrument height also reduces the overall response amplitude and should not be interpreted as simply increasing the depth of investigation. In any case, a multi-coil sensor remains far more practical.”

5. Line ~97

The sentence "one invariably finds discrepancies..." is vague. It is unclear whether the discrepancies arise from inversion artefacts, support-volume differences, calibration, instrumental drift, or a combination of these. This point deserves a clearer explanation.

We tried to give a clearer description in the revised text. However, to us, the relative importance of the different sources of discrepancy depends on the specific case under study and on the sensor and measurement techniques used. Therefore, since the statement is intended to be general, it cannot identify a single predominant cause.
Nevertheless, the revised text now explicitly distinguishes among the main potential sources of discrepancy. The revised text now is:

“EMI inversion may introduce smoothing and is affected by non-uniqueness, because a limited number of σ_a data are used to reconstruct spatial distributions of σ_b. In addition, EMI, TDR, soil sampling and ERT have different spatial support volumes and sensitivity patterns and thus doo not necessarily represent the same observation soil volume. Additional discrepancies may result from instrument calibration and electronic stability, noise and positioning errors. Consequently, differences between EMI-based σ_b and direct measurement distributions should not be interpreted only as errors coming from the inversion"

6. Lines 180–190

I found this entire section difficult to follow. The discussion regarding the scaling of initial conditions and water retention curves is rather convoluted and should be rewritten in a more straightforward manner.

This point was also raised by the reviewer 1 (see also the answer given there). We completely rephrased the section. Now the revised text is:

" To us, this seems an interesting interpretative key and has important implications for understanding what an EMI sensor and point-scale measurements (TDR, soil sampling, ...) can really observe in a hydrological process, such as the propagation of a wetting front during infiltration. EMI and point- scale measurements (e.g., TDR and soil sampling) provide two apparently different representations of the infiltration process. This does not mean that one is more reliable than the other. Simply, they see different water content distributions because of their different observation volumes and spatial sensitivities. The key point is that a soil hydrological model may be able to reproduce both representations by simply using the specific initial water-content distributions observed by the two techniques and by scaling the corresponding water retention curves by the ratio between the technique-specific saturated water contents. If this result were confirmed, there would be no need to empirically adjust the EMI-based σ_b or θ time series to match the point-scale measurements at each time and depth. To the contrary, either representation could be converted into the other by scaling the corresponding water retention curve and initial water-content profile"

7. Instrument description (line ~285) - It is unclear whether the authors are referring to three receiver coils measured in two orientations (thus six apparent conductivity measurements), or a six-coil instrument. The CMD Mini Explorer should be described more explicitly to avoid confusion.

Done. The revised text now is:
"The propagation of the wetting front during the irrigation was tracked using a CMD mini-explorer EMI sensor (GF Instruments, Brno, Czech Republic). The sensor operates at 30 kHz and features three receiver coils located 0.32, 0.71, and 1.18 m from the transmitter coil, referred to as "ρ32," "ρ71," and "ρ118." The manufacturer specifies effective investigation depths of 0.5, 1.0, and 1.8 m in HCP mode, which decrease to 0.25, 0.5, and 0.9 m, respectively, when the instrument is operated in VCP mode. Thus, the combination of three coil spacings and two coil orientations provides six σ_a measurements characterized by different depth-sensitivity ranges.
Measurements started immediately after the irrigation stop. They were first taken in vertical coplanar (VCP; horizontal magnetic dipole) mode and then in horizontal coplanar (HCP; vertical magnetic dipole) mode by rotating the probe of 90°. As a result, six different σ_a values

were obtained (three coil offsets × two orientations), each corresponding to a specific depth sensitivity."

8. HCP versus VCP interpretation (lines 420–425)

The interpretation appears incorrect. The manuscript states that HCP indicates shallower conductive structures compared with VCP. However, for the CMD Mini Explorer, the VCP configuration is generally more sensitive to shallow conductivity variations, whereas HCP has a deeper sensitivity function. This interpretation should therefore be revised.

Yes, of course. The point was also raised by the reviewer 1. We report here the response already given to the reviewer 1
We recognize that our original interpretation of the σa evolution was misleading, partly because of a reversed interpretation of the VCP and HCP meaning. We also agree that the relatively modest temporal evolution of the apparent electrical conductivity deserves further explanation.
Accordingly, in the manuscript, we have revised the discussion of Figure 3 to account for all the discussion above:

"**Error! Reference source not found.** shows the spatial distribution of the EMI σ_a under the VCP configuration and HCP configuration for the three receiver coil spacings (ρ32, ρ71, and ρ118) measured at the beginning (t=0) and during the water infiltration experiment after the 3h (after 2nd irrigation), 5h (after 3rd irrigation) and 9h (after 5th irrigation). The σ_a values are relatively low, mostly ranging from 5 to 35 mS m^{-1}. Note that σ_a series tend to remain approximately in the initial range in the non-irrigated half transect. For the remaining half transect, a similar spatial pattern is observed in both VCP and HCP configurations.
An evident increase in σ_a occurs between t = 0 and t = 3 h in the VCP datasets and in the shallowest HCP dataset (HCP ρ32). A further increase is locally visible mainly in HCP ρ32 between t = 3 and t = 5 h, whereas the other datasets do not show a significant temporal increase. From t = 5 to t = 9 h, the σ_a patterns remain broadly stable. The limited temporal variation observed for HCP ρ71 and HCP ρ118 reflects a greater sensitivity to larger soil depths. During the infiltration experiment, water contents changes were limited to the propagation of the wetting front. As this was mostly confined in the upper part of the profile, it represented only a small fraction of the response volume of these deeper configurations. To the contrary, the contribution of the shallow (and conductive) water table may have dominated the HCP ρ71 and HCP ρ118 σ_a values, reducing their sensitivity to the wetting front propagation close to the soil surface. To the contrary, the clearer temporal evolution observed in the VCP datasets and in HCP ρ32 is consistent with their larger sensitivity to the water content changes induced by the irrigation at the soil surface.
For the same receiver-coil spacing, HCP generally shows higher σ_a values than VCP. Considering the greater depth sensitivity of the HCP configuration, this difference is consistent with an increasing contribution from the deeper conductive portion of the profile, including the relatively shallow water table.
However, the overall observed σ_a patterns may result from a combination of effects related to the integrated response of the EMI sensor and those related to the specific physical system under study. It is likely that a clear identification of the wetting front propagation may have been limited by the sandy texture of the soil, which favours rapid infiltration and

redistribution, as well as by the relatively low electrical conductivity of the irrigation water, which may have limited the contrast between the initial and irrigated conditions.”

9. Figure 3

Figure 3 shows relatively limited temporal evolution during irrigation. The authors should better explain why the observed changes are rather modest despite several hours of irrigation and discuss whether this reflects soil properties, irrigation intensity, limited penetration, or the averaging nature of EMI measurements.

See the response to the previous point

10. Line ~451

The interpretation given here would benefit from an appropriate literature reference supporting this behaviour.

Done. In the revised text we wrote:

“This behaviour is consistent with the attenuation of water content contrasts and smoothing of the wetting front frequently found in geophysical imaging of infiltration processes (Hinnell et al., 2010; Wehrer and Slater, 2015; Huang et al., 2016)”

11. Supplementary material

Some methodological details (additional inversion tests, sensitivity analyses, optimization of regularization parameters, etc.) would fit naturally into Supplementary Material rather than remaining only briefly discussed in the manuscript.

In principle, we agree that extensive inversion tests, sensitivity analyses, and systematic optimization of regularization parameters could be presented in a dedicated methodological study. However, such an exhaustive analysis is beyond the scope of the present work, whose main objective is to investigate the scaling between EMI-based and direct water-content observations through soil hydrological modelling, rather than to assess alternative inversion strategies.
In the revised manuscript, we have nevertheless expanded the description of the inversion procedure and clarified the rationale behind the adopted settings. Since these details are concise and directly relevant to the interpretation and reproducibility of the results, we prefer to retain them in the main text rather than introduce Supplementary Material containing only a limited amount of additional information.

12. Novelty- The manuscript repeatedly refers to Dragonetti et al. (2022), but it is not always obvious what is genuinely new in the present work. The authors should explicitly summarize the methodological advances over their previous study.

In the Conclusion section we rephrased all the first part. Now, the revised text is:

“The study builds on the approach proposed by Dragonetti et al. (2022) but extends it to a transect-scale infiltration experiment in sandy soil. The methodological contribution is in the use of hydrological modelling as a common process-based framework to compare the time evolution of water content distributions provided by point-scale measurements and EMI. The study focuses on the to the effects of the different measurement support volumes, the depth-dependent scaling factor of the initial conditions and the assumption of horizontally homogeneous SHP within each soil horizon. Specifically, it evaluates whether technique-specific initial conditions and water-retention curves differing mainly in their saturated water contents can reproduce the temporal evolution observed by both techniques, without requiring an empirical rescaling of the EMI-derived water contents at each time step. The main message of the paper is that integrating EMI measurements with hydrological modelling may provide a robust physically based framework for interpreting what an EMI sensor observes during a water infiltration experiment, as well as to estimate soil hydraulic properties noninvasively at the field scale.”

13. Language

Although generally understandable, the manuscript contains numerous awkward or overly long sentences. A careful English revision would improve readability substantially.

OK

14. Physical interpretation

The manuscript frequently interprets inversion results as direct evidence of physical processes. Since EMI inversion remains non-unique and strongly regularized, the discussion should better distinguish between observed measurements, inversion-derived models, hydrological interpretation.

We tried to do it by rephrasing several sentences about the comparison between EMI and direct measurements and their interpretation

15. Validation - The validation relies mainly on comparison with auger-derived water contents. A quantitative evaluation (RMSE, MAE, bias, correlation, etc.) would strengthen the conclusions compared to mostly qualitative comparisons.

The Root-Mean-Square Error, RMSE, between the auger-measured and the simulated water contents, and between EMI-derived water contents and the simulated water contents, were already presented in the original text. However, now we have included quantitative indices also in the map-based comparison.

16. Regularization

The authors adopt a "sharp" regularization because it appears more realistic. This choice is somewhat subjective. A short comparison between smooth and sharp regularization results (possibly in Supplementary Material) would help demonstrate the robustness of the conclusions.

The issue has been differently raised also by reviewer 1. This topic is very interesting but, to us, it is a bit beyond the scope of the paper. In any case, it has been dealt with in details in a paper that was already been cited in the original paper (**Deidda, G.P.**, Díaz de Alba, P., Rodriguez, G., Vignoli, G., (2020). Inversion of multiconfiguration complex EMI data with minimum gradient support regularization. A case study. Mathematical Geosciences, 52(7), 945-970.)

As for the supplementary material, see the response at the item 11 above