Responses to reviewer 1

Review of manuscript titled "An in-situ methodology to separate the contribution of soil water content and salinity to EMI-based soil electrical conductivity" by Autovino et al.

The manuscript is interesting, aimed to partition ECa surveyed data and untangle the influence of soil moisture and soil salinity on bulk EC. I believe the manuscript does not require significant revision. Nevertheless, the following issues should be addressed:

We thank the referee for their positive and constructive review of our manuscript.

Below we address each of the specific comments in detail.

L22-23: It seems contradicting having an assumption that field plots are homogeneous and then consider the spatial distribution of properties within.

Although the concepts of heterogeneity and spatial variability may appear contradictory, they are not. Heterogeneity refers to distinct or abrupt differences in soil classification or morphology within a given area. In contrast, spatial variability describes the continuous, often subtle variations in soil properties (e.g., pH, salinity, moisture content) that occur within or between otherwise homogeneous units. While spatial variability is intrinsic to all soils - even at fine spatial scales - our case study defines **homogeneity** as the overall similarity in soil type, classification, and horizon depth between saline and non-saline plots. To avoid potential confusion, we have adopted the term **pedological homogeneity** to emphasize this classification-based similarity.

L43: The most common field method…

We agree with the reviewer. In the revised version of the manuscript, we will modify the sentence to begin with "The most common field method to evaluate…

L52: Please consider the use of the word However at the beginning of the sentence. It seems misplaced there.

We agree with the reviewer and will make this change in the revised version.

L69: ECa (subscript a).

We agree with the reviewer and will make this change in the revised version.

L91: Please change meters to m. Also in L105-115. There is no reason for writing units in full.

We agree with the reviewer and will make this change in the revised version.

L91-93: Which year? I don't think you give it anywhere.

The experiment was conducted in 2018. This information will be added to the revised manuscript.

L97: Please understand that EC of 1.6 dS/m can hardly be referred to as non-saline. What was the source of this water?

The irrigation water with EC = 1.6 dS/m corresponds to the local well water typically used in the study area. We will reword the revised text to more clearly indicate that the "non-saline" plot was irrigated with low-salinity well water without the addition of additional salt.

L99-102: When? How frequently?

Leaf water potential was measured nine times during the growing season from June 11 to July 29. This information will be added to the methodology in the revised manuscript.

L118: Please add ring sizes and reasons for collecting undisturbed samples.

We thank the reviewer for noticing the typo . In the revised manuscript, we will clarify that disturbed soil was sampled at the TDR measurement points. These samples were used for laboratory determinations of soil solution salinity (σw) using the 1:2 extraction method.

L157: Above, there is no information about collection of disturbed samples, only undisturbed. As I believe the authors did not make the effort to collect soil cores only to destroy them later, some additional information on soil collection seems to be missing in section 2.1.

As reported in the previous point, we will indicate that disturbed soil samples were collected for soil solution extraction and salinity analysis.

L193: Zc (subscript c).

We agree with the reviewer and will make this change in the revised version.

L279: Please revise. Everything else seems to be given in dS/m.

We will revise the unit presentation in this section and ensure that all electrical conductivity values are uniformly reported in dS/m.

L438: Present.

We will revise the verb tenses in this section for consistency with the present tense style used elsewhere in the manuscript.

Responses to reviewer 2

This paper ("An in-situ methodology to separate the contribution of soil water content and salinity to EMI-based soil electrical conductivity") addresses a critical challenge in agricultural management: monitoring soil water content (θ) and solution salinity (σ_w) at the field scale. While Electromagnetic Induction (EMI) is a non-invasive geophysical method used to map soil spatial variability by measuring apparent soil electrical conductivity (ECa), the bulk electrical conductivity (σ_b) derived from EMI is influenced by both θ and σ_w. This dual dependency makes it difficult to quantify these two variables independently.

The study's primary objective is to develop and validate an EMI-based methodology capable of separating the respective contributions of θ and σ_w to σ_b. To achieve this, the authors conducted an experiment using two adjacent maize plots, one irrigated with saline water and the other with non-saline water. The proposed procedure involves measuring ECa with a CMD-MiniExplorer, inverting the data to obtain the spatial σ_b distribution, and then employing a site-specific calibrated Rhoades linear model, alongside an assumption of homogeneity in water content between the two plots, to estimate the spatial distribution of θ and σ_w in the saline plot. The results indicate that this integrated approach estimates θ and σ_w along the soil profile with reasonable accuracy, except at the immediate soil surface where EMI reliability is limited.

That said, I have several major concerns and questions regarding the methodology, assumptions, and interpretation of results:

- Which linear form of Topp's equation do you use to compare your results to in Fig. 3? It does not seem correct. This is the correct Topp equation: swc = -5.3*10**(-2) + 2.92*10**(-2)*eps - 5.5*10**(-4)*eps**2 + 4.3*10**(-6)*eps**3. The linear form of Ferre is: swc = 0.1181*√(eps)-0.1841. Based on my calculations, the curve (or line in case of linearization) should be around the Ap line on your plot. E.g., for √(eps) = 4, SWC = 0.29 cm3 cm-3 (see figure attached). Please verify, correct and clarify this.

  Thank you very much for pointing this out—you are absolutely correct. The error arose from cutting and pasting Topp's equation in Excel tabs, during which the minus sign of the first term (-5.3×10^{-2}) was inadvertently omitted. This mistake led to an overestimation of the water contents. When the correct value of Topp's equation is applied, the values are, on average, lower than the Ap values, as you noted.

  Fortunately, this error does not affect the rest of the calculations, since even with the correct formulation, a specific calibration is required for both horizons. In the revised manuscript, we will correct the graph in Figure 1 as well as the related comment.

- **Methodology of lab analyses**: both 2.2.1 and 2.2.2 use PVC cylinders with 15 cm height.
  - 2.2.1: Is it common to saturate the soil column from the bottom, hoping for a uniform wetting, instead of packing the soil with pre-homogenized soil? Similar question for the evaporation process, during which the measurements were taken: I would expect the risk of a vertical SWC gradient to be high?

The procedure we followed was as follows:

- The soil was air-dried for several days and then repacked into the cylinder to achieve a dry bulk density very close to that observed in the field (1.1 g/cm³). This target density could not have been obtained if the soil had been repacked at a higher initial moisture content.
- The soil was subsequently saturated slowly from below, which facilitated the release of entrapped air and allowed for complete saturation of the porous medium
- With regard to potential moisture gradients, it should be noted that the TDR integrates the signal along the entire length of the rods inserted into the soil, thus averaging over the surrounding soil volume. As a result, any vertical gradient in soil water content is integrated into the measurement and does not have an appreciable effect on the results.

We hope this clarifies the procedure, and we will amend the relevant section of M&M accordingly.

- 2.2.2: Here you state that each sample was wetted with 15 ml of a solution. How was this done? From the top or bottom? Or was the soil homogenized first and then put in the PVC cylinder? The latter one would be the optimal approach I think, since top or bottom wetting may result in non-uniform distribution of water and salinity.

Wetting with 15 ml volumes was achieved by applying the solution evenly across the sample surface from above. After each application, the sample was covered with film to prevent evaporation from the soil surface, after which it was left to equilibrate overnight to maximize capillary redistribution of water and solutes. This procedure was designed to minimize water and solute gradients. The choice to perform a preliminary homogenization of the soil at a certain moisture level would have posed greater challenges for the required analysis. It would certainly have been necessary to prepare as many soil columns as the moisture levels to be considered, and each column would have had to be prepared at the same dry bulk density as measured in the field. Of course, we are aware that no procedure can completely exclude the risk of moisture or solute gradients. However, as mentioned previously, the TDR's ability to integrate the response minimizes potential errors without substantially altering the results.

- **θ(σ_b) relation**

Fig. 4: the data range is very limited (0.3-0.45 cm3 cm-3); do you think these ranges are sufficient? Do you assume that you can extrapolate the linear relation?

- Why can you assume that this relation would be linear? I would expect a sigmoidal/concave upward curve, starting flat at low θ, rising sharply, then flattening again near saturation. You can see this also in your Eq. 1, if you write theta in function of σ_b, in principle you assume that the ratio of σ_s and σ_w is constant and independent of theta, which is not the case I think.

This range is in contrast with your experiment description in 2.2.2, where you mention SWCs of 0.06 to 0.46 cm^3 cm^{-3}.

In Fig. 7, you estimated SWC based on this relation, and here you get a SWC range of 0.19-0.25 cm^3 cm^{-3}; which was not in the range of the soil-specific θ(σ_b) calibration relation. How can you be sure this is correct? --> I would propose to redo the experiment to know the relation over a wider range of SWCs.

We fully agree that the question you raised is of central importance, and this is why it took us some time to formulate an adequate response.

We acknowledge that the assumption of linearity in the σb(θ) relationship, which is valid only for high θ values, may limit its applicability in our case, since the range of θ values observed in the field was lower. To address this issue, as suggested by the reviewer, we returned to the field, collected new soil samples, and repeated the calibration following the same procedure described in the original manuscript, but this time extending the measurements to lower θ values.

As expected, (see Figure 1 below), at lower water contents the relationship deviates from linearity. We found that the linear approximation holds reasonably well up to approximately 0.2 cm^3/cm^3, while deviate from this linearity for the first two σ_β–θ pairs. For this reason, the regression does not include these two values. Compared to the previous fit, the slope remains similar, while the intercept shows a slight downward shift.

When applying this extended linear relationship to the dataset, the resulting plot (Figure 2 below - Figure 7 in submitted manuscript) shows only minor differences from the original version. Therefore, the overall discussion and conclusions presented in the manuscript remain valid.

Unfortunately, we must note that the field from which the measurements and samples were collected belongs to a private farmer who carried out deep tillage in the experimental area. This intervention prevented us from performing differentiated tests between the Ap and Bw horizons; consequently, only a single σb(θ) relationship could be established. However, given the overall homogeneity of the soil profile—also confirmed by the similar linear relationships obtained for the two horizons in the original version of the manuscript—this limitation did not significantly affect the results.

All updated coefficients will be included in the new version of the manuscript, and the discussion will be revised accordingly once approval is received from the editor.

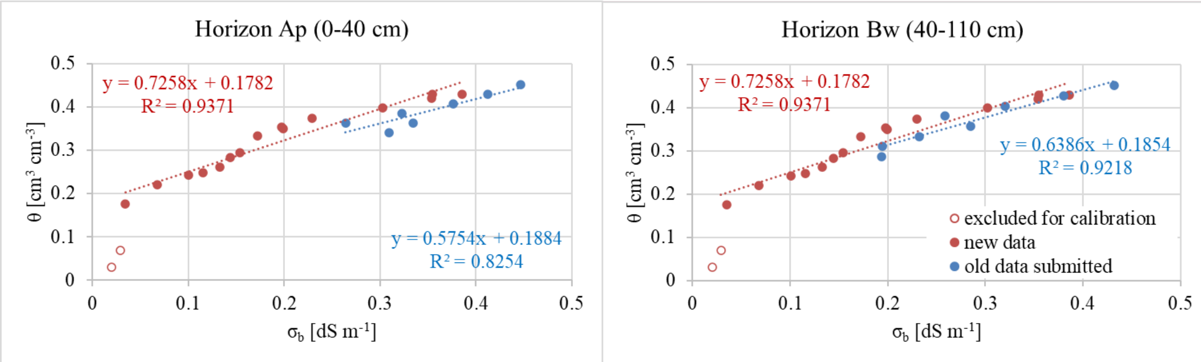

Figure 1. Calibration of the σ_β–θ relationship for two horizons: (a) Ap (0–40 cm) and (b) Bw (40–110 cm). Red points ("new") indicate θ-σ_β pairs from the new calibration; blue points ("submitted") and their regressions are the θ-σ_β pairs the original manuscript. Dotted lines represent the linear regressions (red = updated; blue = original; equations coefficients and R^2 shown). The red circles with white background represent the points excluded from the calibration of the linear relationship θ-σ_β.

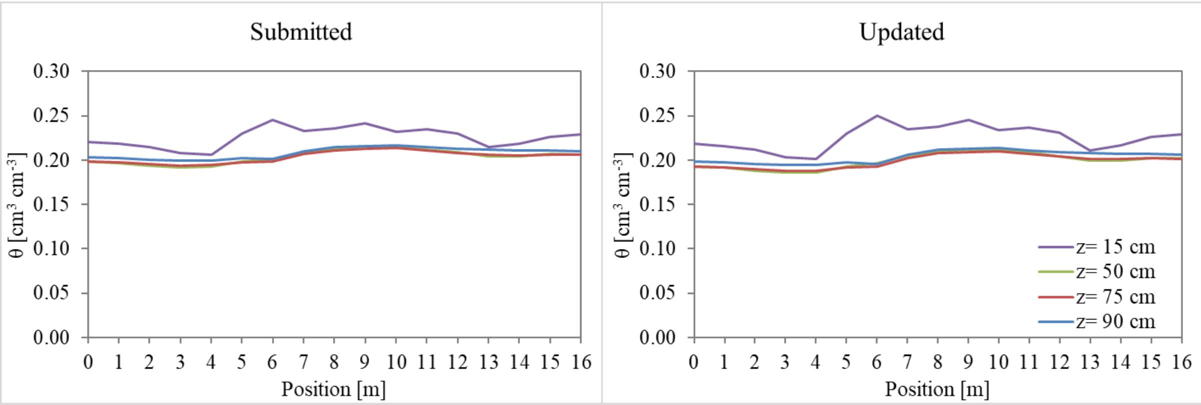

Figure 2. Effect of the updated σ_β–θ calibration on the estimated soil water content along the transect at four depths (z = 15, 50, 75, 90 cm). The left panel shows the values obtained with the calibration used in the submitted manuscript. The right panel shows the values obtained with the updated calibration.

Some smaller comments or questions that I have after reading the manuscript:

- L56: abbreviate TDR
  We agree with the reviewer and will make this change in the revised version.

- L75: over time and space
  We agree with the reviewer and will make this change in the revised version.

- L80-82: add comma or start new sentence
  We agree with the reviewer and will make this change in the revised version.

- L111: TDR with abbreviation
  We agree with the reviewer and will make this change in the revised version.

- You mention taking undisturbed soil samples in the saline plot – what happened with them? Later you also mention soil samples from the non-saline plot? Clarify this.

  We thank the reviewer for noticing the typo also noted by the other reviewer. In the revised manuscript, we will clarify that disturbed soil was sampled at the TDR measurement points. These samples were used for laboratory determinations of soil solution salinity (σ_w) using the 1:2 extraction method.

- Figure 1: Improve this figure.
  - Add subplot letters

    The subplot letters followed by their names will be added
  - Plant row is not visible

    A darker color will be chosen to better highlight the rows of plants
  - North arrow unclear what it belongs to. If it belongs to the map, improve the map (lat, lon) or don't show Italy at all. The Wikipedia reference is not optimal.

    We will orient the plots so that the orientation is consistent with the map of Italy
  - σ_w measurement?

    We will change the legend text to "TDR measurement and soil sampling"
  - TDR measurement are not on here?

    Yes, the TDR measurements were taken there. We will change the legend text to "TDR measurement and soil sampling"

- Why only measure 1-2 rows?

  We focused measurements along the crop inter-row space because all destructive and in-situ observations (trench, TDR profiles, and 1:2 soil-solution sampling) were co-located with the EMI transect along a 17-m line to minimize spatial bias across methods and to limit disturbance to the crop.
  Before choosing the trench location, a preliminary ECa map was used to select a homogeneous field transect. Since the variability across adjacent rows in that transect was small; within the lateral footprint of the EMI configurations, sampling additional rows would have yielded largely redundant information without improving the inverted σb profiles.
  Given the destructive/time-consuming nature of digging and repeated TDR insertions, restricting field surveys between 2 maize rows provided good validation while preserving plot integrity.
  We have added this rationale in Section 2.1, indicated the row layout and transect position in Figure 1, and noted in Section 3.6 that cross-row replication can be expanded where row-scale heterogeneity is expected.

- Why no σ_w or TDR measurements in non-saline plot?

  Our experimental design was concentrated in a destructive (trench and 1:2 extracts for σw,SS) and intensive in-situ profiling (multi-depth TDR) in the saline plot, where salinity contrasts are informative for validating σw,EMI.

The non-saline plot served a different role that is to provide the horizon-wise mean θ required by the Rhoades model. In that plot, irrigated with tape water and managed identically on the same dates, σw variability is expected to be low and contributes little to σb relative to θ.
Therefore, a destructive σw sampling or full TDR profiling there would have added limited value while increasing disturbance. Instead, as detailed in Step 4 of our workflow, we obtained the non-saline plot's horizon-wise θ non-destructively from EMI via the site-specific θ–σb calibration, and we scheduled EMI immediately after irrigation to minimize cross-plot θ differences.
We note, however, that a concurrent experiment at the same experimental farm collected a single vertical TDR profile in the non-saline plot on the survey date. Those θ values are consistent with our EMI-derived horizon-wise means, but because they represent only one location (limited spatial representativeness), we chose not to include them in the manuscript to keep the focus on our co-located EMI–trench validation in the saline plot.

- L184: Clarify that Tektronix = TDR
  We agree with the reviewer and will make this change in the revised version.

- The steps are not very clear and it's confusing that they are not in the same order in the results section. Also, step 6 is spread over 3 parts. Please improve structure.
  We will revise the manuscript to align the structure of the Results with the workflow

- It is unclear how you calibrated or fitted the parameters in Table 1: is this based on a least squares method? Also, the last paragraph of 2.2.3 is quite vague and is based on reading another paper ("In order to calibrate the model for deriving the soil-specific a, b and σs parameters, the procedure reported in Malicki and Walczak (1999) was applied, by using the same experiment reported therein at the point 2.2.2. Finally, the obtained θ - σb - σw data were fitted to the Rhoades model to finalize the calibration procedure.")
  We will revise this section and fully specify the calibration/fitting procedure.

- L239: Check sentence.
  We agree with the reviewer. This sentence contained a grammatical error ("compare" → "compares") and an orphaned fragment ("for the Ap and Bw horizons"). We also standardized the notation to σ_b. We will rewrite this sentence in the revised version.

- L242-243: R^2 is sensitive to the range of the data!
  We agree with the reviewer and will rewrite this sentence in the revised version

- Figure 6
  - Add subplot letters. C is not in the caption.
    We will add the subplots letter in the figure.
  - What are the lines in Fig. 6a-b? They don't seem to fit the data points – is this the result of a moving average filter? Clarify in the text and caption.
    Sorry for the lack of clarity in the figure captions and labels. We have revised both the figures and captions of this figure as well as figure 8 to specify that the points represent the measured EC_a, while the dashed lines show the calculated EC_a response from the inverted model.

Ideally, the calculated response of the inverted model should closely fit the measured data; however, several factors contribute to the observed discrepancies. These include the spatial variability of EC_a values (i.e., heterogeneity in conductivity along the transect), the smoothness constraint applied during the inversion to stabilize the solution (which reduces the ability to resolve sharp conductivity changes, resulting in larger misfit) and the choice of initial models, among others. In the revised version, we will include a discussion of this issue.

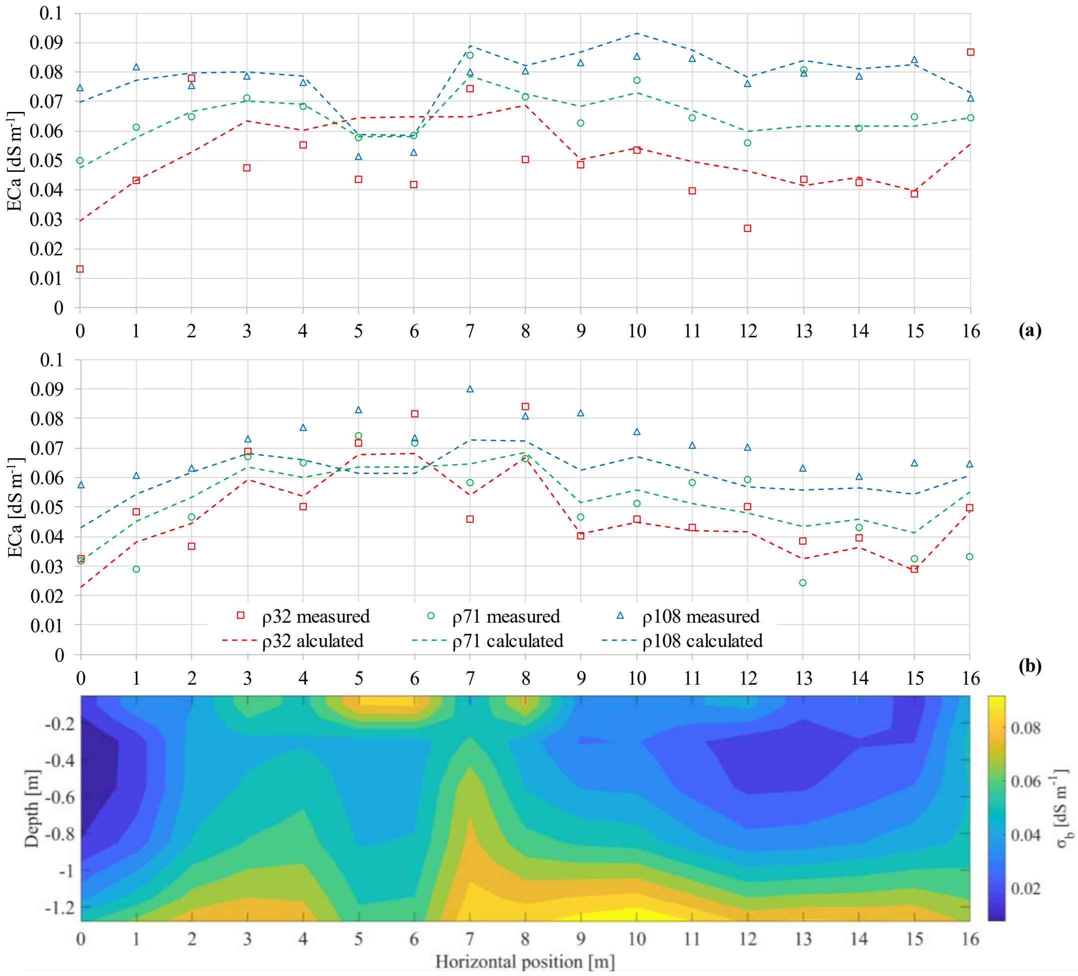

Figure 6. Apparent soil electrical conductivity (ECa) along the transect for the non-saline plot: (a) HCP mode; (b) VCP mode. Points indicate measured ECa, while dashed lines show the calculated ECa (forward response of the inversion). (c) Inversion results showing the bulk electrical conductivity (σ_b) distribution with depth.

- Fig. 6c: You have ECa measurements at 30 cm depth; how does the model extrapolate to the top 20 cm? You have ECa measurements at 1.2 m depth, why does the inverse model results only go up to 1.05 m depth?

  Apparent conductivity (EC_a) measured with an EMI sensor (including the CMD-Mini used here) does not represent the conductivity at a single physical depth but rather a weighted, cumulative response of the soil column beneath the sensor. The sensitivity of each measurement depends on the transmitter–receiver spacing and the operating frequency, which determine the effective depth range to which the instrument is most responsive.

For this reason, an inversion process is applied to estimate a layered conductivity model whose forward response (shown as dashed lines in Figures 6 and 8) reproduces the measured EC_a values (points in the same figures). We acknowledge that this aspect was not sufficiently explained in the manuscript and may have been misleading when we associated each EC_a measurement with a specific depth. We will revise the Materials and Methods and Results sections to clarify the effective depth range of each EC_a measurements.

Regarding the inversion depth, the model could theoretically extend slightly deeper; however, the resolution beyond 1m is limited, since only one measurement configuration has meaningful sensitivity at that depth. Moreover, this deeper interval lies beyond the investigation depth of interest in this study, where supporting data (e.g., from TDR) were available. For these reasons, we excluded the deeper layers. We will revise the manuscript to clarify this.

Consider using a different colormap; this one is not greyscale (printer) friendly.

We will replace the previous rainbow colormap with a perceptually uniform, greyscale-friendly palette (parula), so that the ordering of values is preserved when printed in black and white. We also checked all figures with Coblis (according to the guide for authors) and revised the color schemes accordingly.

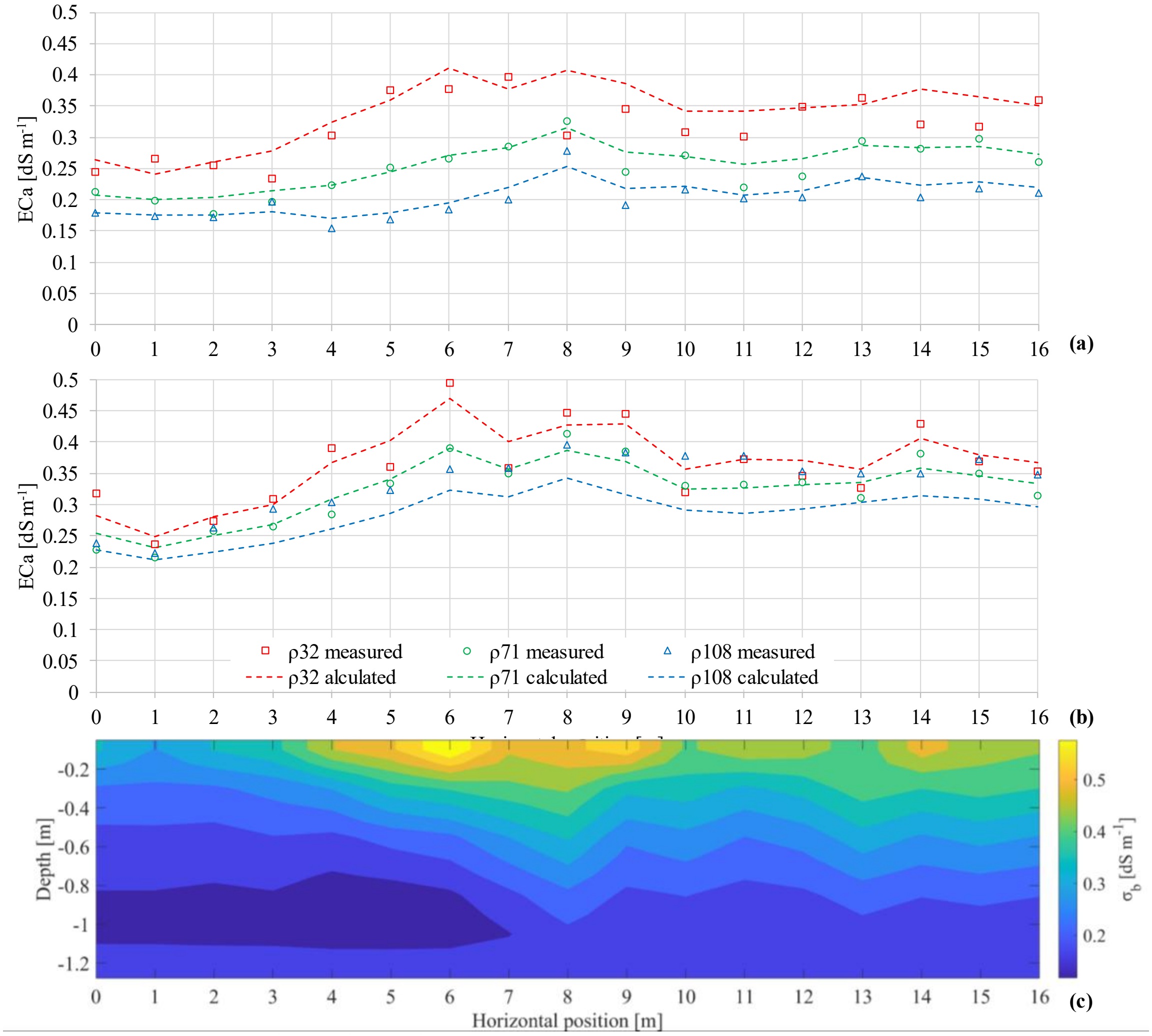

Figure 8. Apparent soil electrical conductivity (ECa) along the transect for the saline plot: (a) HCP mode; (b) VCP mode. Points indicate measured ECa, while dashed lines show the calculated ECa (forward response of the inversion). (c) Inversion results showing the bulk electrical conductivity (σ_b) distribution with depth.

- You say the 0-30 cm layer is influenced by surface drip irrigation, while also the 30-80 cm is 'directly wetted by drip irrigation'. At what depth was the drip line?

  The drip line was installed at the soil surface (0 cm), between adjacent crop rows. The 0–30 cm layer is directly influenced by the surface application, whereas the 30–80 cm layer is wetted by the downward percolation (wetting bulb) not by a subsurface emitter. To avoid ambiguity, we will also revise the wording in the Results section.

- Be consistent in the plots: horizontal position of 0-16 m (Fig. 6) or 1-17 m (Fig. 7)

  We'll modify the figures to make them consistent. The horizontal position will be 0-16 m.

- Fig. 8: Add subplot letters.

  We will add the letter subplots in the figure

- L347: Fig. 9, not 10

  we will correct this typo

- L349 & Fig. 9: You applied a moving average filter to the sigma_w-SS measurements; which window size did you use and why? Is this necessary? It feels like you might be artificially changing measurement data.

  We used a centered 3-point moving average on $\sigma_{w,SS}$. The rationale is to harmonize spatial support with the EMI-derived profiles, which are inherently smoother because of their lateral sensitivity and inversion regularization. The filtering makes the variance of the point samples comparable to that of the EMI estimates.

- L360: 0.23 is not in the figure? I read 0.10

  Thanks to the reviewer for noticing this typo. The correct value at 15 cm is r = 0.10. We will correct the text to align with the figure

- L376: Variance of wSS does not increase? It even slightly decreases with depth. (Table 2)

  We will correct the statement in the revised manuscript. Table 2 shows that the variance of $\sigma_{w,SS}$ is approximately constant to slightly decreasing with depth (1.60, 1.64, 1.41, 1.36 $dS^2\ m^{-2}$ at 15, 50, 75, 90 cm), whereas the variance of σw,EMI clearly decreases with depth (from 3.38 to 0.75 $dS^2\ m^{-2}$).

- Check consistency in symbols! (sigma_w,SS or sigma_w-SS, sigma_w^SS-f)

  We will standardize the notation throughout the manuscript (text, tables, figure and captions). The adopted conventions are:

  - Variable, methods (as subscripts after a comma):
  - $\sigma_{w,SS}$ = soil-solution EC from 1:2 extracts
  - $\sigma_{w,EMI}$ = σ_w estimated from EMI
  - $\theta_{,TDR}$, $\theta_{,EMI}$ for moisture by TDR and EMI, respectively

  Also Figure 9/10 legends and Table labels were updated to reflect this convention.

- Fig. 10: incomplete legend (thick solid line, thin solid line), unclear what are the two theta-EMI, the two X's, not all plots have both?

  we will review the legend and caption

- L402: Check sentence.
  We agree with the reviewer and will revise this sentence in the revised version.

- L467: Check sentence.

  We agree with the reviewer. There was a grammatical error ("presents is") and the phrasing was unclear. We will correct the sentence for grammar and clarity.

Overall, the manuscript addresses an important problem and proposes a creative approach, but at present the methodology and interpretation are not sufficiently clear or convincing. In particular, the assumptions about the θ–σb relation, the limited calibration range, and the lack of detail in sample preparation raise doubts about the robustness of the conclusions.

Furthermore, the practical applicability of the method seems constrained: it requires a non-saline plot immediately adjacent to a saline one, with identical soil properties and moisture conditions; a situation that is rarely feasible in practice.

Our approach was designed for secondary salinization contexts, which are common in irrigation systems, where within the same farm it is possible to have adjacent plots managed with water of different quality but with similar soil pedological characteristics. In this context, the assumption of comparable average water contents per horizon between the two plots is reasonable and operational. However, we recognize that this configuration is not universal. Therefore, in the revised version, we explicitly delimit the scope of use and indicate how the absence of a "twin" plot can be compensated for in a simple and practical way. In particular, the estimate of the average water content per horizon at the date of the EMI measurements can be obtained with a few field measurements (for example, using moisture probes or TDR profiles placed in homogeneous areas identified with preliminary ECa mapping) or with point sampling stratified by horizon or by unit of variability, and, if necessary, supported by a simple water balance model constraint calibrated with those same measurements. Furthermore, scheduling EMI surveys in time windows immediately following irrigation or rainfall helps reduce spatial differences in moisture, mitigating the uncertainty associated with the estimate of average θ. We will include these indications in Section 3.6 (Limitations and Conditions of Use) and will recall in the Abstract and Conclusions that applicability is optimal in irrigated systems with homogeneous soils and management, while in other contexts it is advisable to use the solutions described above to obtain the reference values of θ necessary for applying the procedure.

I encourage the authors to clarify the methodology, provide additional experimental data across a broader SWC range, and carefully reconsider the assumptions that are made. With these improvements, the study could make a valuable contribution to EMI-based soil monitoring.

We sincerely thank the reviewer for the careful and constructive assessment, which has already helped us refine the paper. In the revised manuscript we will clarify the methodology, we will add analyses and measurements. These changes markedly enhance clarity, robustness, and the practical relevance of the study.