Reply to Reviewer #2

The response to the individual comments of Reviewer #2 is given below. In the following, the original review is quoted in *italics*.

TDR conductivity measurements show larger conductivity close to the surface as it should be expected since it is closer to the source of conductive material (added saline water). For this aspect, it is acceptable to consider TDR conductivity measurements as providing more reliable information, which could serve to calibrate "less reliable conductivities" obtained with EMI measurements (or their inversion). I therefore agree with the overall approach. I find the paper clear and well-written. I have nevertheless few questions at the EMI inversion stage which, I think, should be explored, or at least discussed before acceptance. I also think that Authors should show the final results for the other transects in order to see if the calibration performs well for the different irrigation experiments. This being said, this manuscript is interesting as it addresses the problem of relating ground truth and EMI output with a pragmatic approach.

We agree with the Reviewer's remarks: in the revised version of the manuscript we will further elaborate on the role and importance of the assumptions made during the inversion stage and we will also show the results for the others transects.

Non-uniqueness of the conductivity model resulting from the inversion:
One particular choice of the present study (compared to other cited studies) is to calibrate the conductivity after inversion instead of EMI apparent conductivity data (Eca). However, the argument of non-uniqueness of the inverse problem, which is actually used by the authors in the introduction to question a calibration with ERT method, could be used here in the same way to question the presented method.

During the preparation of the new version, we will stress this aspect and we will make our point clearer: all the limitations connected with the ill-posedness are inherited by our approach that, indeed, is dealing merely with the absolute calibration of the model derived by the EMI data, based on the TDR measurements. For this reason, and accordingly to the Reviewer's remark, we will further elaborate on the fact that the output of our approach is still relying on the hypothesis made during the EMI data inversion.

This said, are you sure that the selected solution obtained using sharp regularization is the best solution to be compared with TDR? Actually, some of the smooth models (e.g. sounding numbers 5, 13 17 in Figure 2) show better vertical concordance with what should be expected and with the best misfit. Anyway, Figure 2 clearly highlights the non-uniqueness of the problem, because it is possible to obtain very different models at similar misfits. Why, for example, not putting some effort to stabilize the regularization of the smooth inversion in order to have sounding N5-like results all along the transect? I do see that, because of fixed interfaces and a very little number of layers, your parameterization does not allow enough model space flexibility to have stable smooth results along the transect. But there are many ways to fix such issues (like, for example, among others, increasing the number of layers or applying some lateral constraints). I would also like to point out that the smooth misfits are slightly higher than for the sharp method (of 1-2 %, except for the few soundings mentioned above. This feature supports my previous comments), which is not in favor of the smooth regularization in a context of fair comparison. For all this, I would not qualify the smooth inversions presented here as a "standard" approach as it is not used with an optimal way (you use few layers with fixed interfaces). For Figure 2, I *really recommend to plot the profiles of collected EMI data together with modeled (after inversion) data to see exactly how well all the channels were fitted. This would allow to properly evaluate and discuss the two methods of inversions. This is also important for a second aspect: in a cultivated area, I would expect some lateral anisotropy of the conductivity of the soil layer due the preferential orientation of the lines of the agricultural work. If this is true, inverting HCP and VCP together would not be an optimal choice as such geometries induce eddy current with different preferential directions. Did you try to consider HCP only, and/or VCP only? Maybe there would be a better qualitative and quantitative consistency between TDR and EMI inversion results?*

The Reviewer correctly highlighted that we used fix interfaces in our model parameterization. However, we have used a discretization with several tens of layers to be able to: (i) control the inversion results by acting only on the regularization parameters and (ii) remove the regularization effects possibly originated by the discretization choices (e.g., the number of layers, interfaces locations). In this way, we have been able to use an automatic strategy for the selection of the regularization parameters. For these reasons, we believe that our smooth result can be defined "standard".
Regarding the use of lateral constraints, it can definitely be a viable solution, but we showed that a satisfactory lateral consistency can be achieved by using an already existing 1D code (slightly modified to accommodate a sharp regularization) instead of implementing a (clearly more troublesome to code) pseudo-2D (e.g., laterally constrained) version of it.
Moreover, from our point of view, the fact that the data misfits are largely overlapping, confirms that the two inversion results are actually comparable. And, if the sharp inversion fits better the data, a simple explanation might be that the assumption of sharp interfaces is in a better agreement with the reality and that the (blocky) true model is difficult to be correctly retrieved when smooth constraints are applied.
We agree with the reviewer that the best way to assess the quality of the data fitting is to plot the collected data against the calculated ones. So, in the revised manuscript we will add a plot and the associated discussion about it.

To sum up, I would have interpreted the results shown in Figure 2 in a different manner.
After acceptation of the non-uniqueness of the EMI data inversion, I would have tried to find the right parameterization and regularization to get sounding N5-like results along the transect before starting the comparison with TDR. As a consequence, I believe that the calibration procedure presented in this study also corrects error due to the non-uniqueness inherent to the considered inverse problem (in addition to the spatial fractality problem already discussed in the text). And in the eventual case of lateral anisotropy, it maybe also correct for less realistic EMI results resulting from joint HCP and VCP inversion. However this two features means that the overall calibration procedure could be dependent on the initial method of inversion. In my opinion, this aspect should be explored in this study, or at least discussed in the text.

As it was mentioned above, we agree with the Reviewer and we will modify the paper taking into account his/her suggestions.

Application of the method to the four transects:
I think you should show the resulting sections (like Figure 8) for all the transects in order to check the consistency of the calibration procedure over larger areas (and implicitly for broader geological/irrigation settings) as it is claimed in the conclusion.

Also regarding this specific point, we see the rationale behind Reviewer's remark and, in the new version, we will show the results from other transects.

Some minor comments:
In the discussion about magnetic permeability (p7 line 239-241), you cite a nonpublished paper (Deidda et al, submitted). Here, it is necessary to also cite already published papers on this topic. There are a couple of recent studies dealing with the inversion of in-phase data for retrieving the magnetic permeability for the case of small EMI sensors.

In the revised version, we will include the references to additional relevant studies.

Figure 8 shows spectacularly that the TDR conductivity of the first layer is largely underestimated by the EMI sharp inversion results. Are we sure that this first layer is well constrained by the considered EMI vertical soundings? Maybe it would be good to show and analyse the a priori covariance on model parameter associated to the selected 4-heights/2-geometries data set.

In the new version of the manuscript, Figure 8 will be largely revised also following the observations of Reviewer #1. Accordingly, a new associated discussion will be added.

Summary of my recommendations:
Plot the measured data versus the modeled data in Figure 2. Explore or discuss the dependence of the overall procedure on the method of inversion. Show the final results for the four transects to confirm the robustness of the method on different irrigation contexts.
Sincerely

We thank the Reviewer for his/her useful and pertinent comments and suggestions. As mentioned above, in the new manuscript, we will show the measured vs calculated data, make our point clearer regarding the dependence on the adopted inversion strategy, present the results from other transects.