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Interactive comment on "Time-lapse monitoring of root water uptake using electrical resistivity tomography and Mise-à-la-Masse: a vineyard infiltration experiment" *by* Benjamin Mary et al.

Anonymous Referee #1

Received and published: 7 June 2019

We thank the Reviewer for his/her comments. In the ensuing text, we try and address all raised issues. The reviewer's comments are reported in black, our replies in *italic blue*. Please also find attached a version of the manuscript with all changes highlighted in red.

B. Mary et al.

The manuscript "Time-lapse monitoring of root water uptake using electrical resistivity tomography and Mise-à-la-Masse: a vineyard infiltration experiment" by authors Mary et al investigates root characterization using the electrical MALM approach in a time lapse setting.

As explained in the manuscript, new approaches to non-, or minimally invasive root characterization are urgently needed to increase our knowledge about the root zone, as well as to provide better data input for soil-plant-atmosphere (SPAC) modeling frameworks. As such I think the topic of the manuscript is relevant to the readership of SOIL and well worth investigating. However, in its current state I cannot support a publication of the text under review without major revisions. Some parts of the manuscript feel somewhat rushed, and perhaps some (or most?) of my issues can be solved by reformulations or some additional text?

—————– General Comments: —————–

My two major concerns are:

1) No validation data: The study uses neither independent information on the rooting depth or distribution, nor are soil information such as soil water content measurements, Archie-Parameters, or soil temperature data used to support the statements made (or used in the analysis of the data). The fact of missing validation data is also mentioned in the abstract, although not further discussed in the text.

Validation of results on the basis of independent data is particularly challenging in the case of root water activity, and even worse in the case of field (not laboratory data). In order to deal with the lack of validation data, we took two actions:

1. *We added the results of a 1D hydrological infiltration model. The simulation results are compared against the spatio-temporal changes of SWC obtained from ERT after petrophysical transformation using Archie's law. A new section was added in the methodology part (nb 2.4).*
2. *We discussed the limits of validation data in our case. See discussion section.*

In light of the novel, very promising, technical approach of MALM I don't think this should prevent the publication of the text, yet it should be actively discussed and conclusions should be limited to statements that can be made without validation data. Perhaps the text could be reworked to provide/develop recommendations for future experiments that deal with the problem of obtaining suitable validation data? As such, the direct predecessor paper, Mary et al 2018, and this study could be positioned as discussing the technical details of the approach, preparing future studies which focus on the validation aspect.

Thanks for the suggestion. We feel this is indeed the message that should be conveyed by the paper. We added an explicit objective in this direction (see L. 123) and added a relevant paragraph in the discussion. Furthermore, all along the manuscript we moderated each of our interpretations acknowledging our lack of direct validation.

Overlap with Mary et al 2018: As far as I can see Mary et al 2018 and this study were conducted on the same field site and same plants within a few months time. As such they should be considered as companion studies. As far as I can see their stated objectives overlap massively (as do the conclusions):

Thanks for the suggestion to link the two papers using the companion paper option. We will deal with it for the re-submission.

2) Aims of Mary et al 2018 (page 5429): "1. define a viable field protocol that uses jointly MALM and ERT to map active tree vine roots, 2. propose and analyze algorithms capable of identifying the location of active roots, and 3. test the algorithms above against real data from a French vineyard."; Aims of this study: "(a) define a non-invasive investigation protocol capable of "imaging" the root activity as well as the distribution of active roots, at least in terms of their continuum description mentioned above; (b) Integrate the geophysical results with mass fluxes measurements in/out of the soil-plant continuum system." My reading is that all aims of Mary et al 2018 are contained in aim a) of this study.

We agree with the reviewer comment, aims were reformulated to better highlight the specificities of this paper (see L. 119-123)

"

This study had the following goals:

- *define a non-invasive investigation protocol capable of "imaging" the root activity as well as the distribution of active roots, at least in terms of their continuum description mentioned above, under varying soil water content conditions;*
- *integrate the geophysical results with mass fluxes measurements in/out of the soil-plant continuum system using a simple 1D simulation reproducing the infiltration experiment.*
- *give recommendations for future experiments focusing on the method validation.*

"

Adding to this, I was not able to find any information on the stated "integration of geophysical results with mass fluxes" (aim b) in the text, leaving only the duplicate aim a).

See previous answer.

Also note that the time-lapse aspect is currently not discussed in detail (detailed below) in the text, the analysis of the time-lapse data does not take into account any dynamics such as daily evaporation information, and the conclusion mostly reflects the conclusions of Mary et al 2018, without significant conclusions regarding the application of MALM within a time-lapse context.

This concern has been also raised by reviewer 3. In this respect we took the following actions:

- *Plot the full time-lapse data for ERT, MALM and current density for both plants and discussed it in detail (while this presentation/discussion was very limited in the first version of the manuscript). For those situations with no significant variations, plots are put into the appendix.*
- *The time-lapse variations are now discussed in light of the hydrological model. This allowed to clearly identify the dynamics such as daily evaporation and/or RWU.*

—————————————- Specific comments/technical corrections: —————————————-

3) In addition, there are various (apparent?) inconsistencies between Mary et al 2018 and this text. However, this could be caused by the rather brief formulations in the text, not by actual errors. I suggest to rephrase.

We answered point by point hereafter.

3.1) Site description: Mary et al indicate sandy-clayey soil from 125-175cm, while this text puts this layer from 100-175m. I also wonder why the authors do not interpret the different information on root distribution given in Mary et al 2018. For example, Mary et al 2018 identifies the first soil layer as "with a first sandy horizon (0–40 cm depth), porous and soft." Looking at Figure 3 in this text, I wonder if the observed resistivity decrease in the upper 40 cm can be attributed to this porous layer, and correspondingly fast infiltration?

Simplifications in the text led to possible approximations but without any consequences on the data validity. At a depth larger than 100 cm the influence on our results is negligible, given the maximum depth of investigation of the electrode apparatus. We intended to describe the deepest layer only to highlight possible long-term water lift thanks to capillarity movements which could explain the shallow rooting depth hypothesized.

We rephrased the relevant sentences to make them consistent with the first article and we summarized the site description graphically in a new figure (as a support of the hydrological model)

The observed resistivity decrease in the upper 40 cm can be attributed to the porous layer, and correspondingly fast infiltration. We added a sentence to point it in the revised version of the manuscript L. 279

Note that in Mary et al 2018, the three layers were distinguished in term of root density and not in term of soil nature. Note that Figure 3 has been changed to highlight 3d feature as request by reviewer 3.

3.2) Mary et al 2018 state that active roots are located in the upper 0.3m (page 5436). This does not seem to be the case if the isosurfaces in Figure 6 (this text) are to be interpreted as root extension, although the same plants are measured. Note that this text also states, as one of the conclusions, that the MALM approach is relatively insensible to different water regimes, and thus I would expect similar results between both studies.

If this inconsistency is caused by slightly different interpretation of the term "active roots", I suggest to rephrase accordingly.

Obviously, the position of the active roots from one acquisition to the another (during different seasons) may vary. Sentence The sentence about 'active roots' has been rephrased to avoid misunderstanding.

First, in Mary et al. 2018, the value of 0.3 m has been validated using the target function F2, while the F1 plot is a spatial representation of the misfit (between measured data and individual source contribution). The misfit is normalized, but the threshold defined as the 25% percentile influencing the delineation of the "active" root zone is not directly comparable from one experiment to the other (see also answer for reviewer question nb 21).

Even though we think that function F1 might also be a good indicator for active roots, isosurfaces in Figure 6 of the manuscript mostly intend to show a straightforward comparison between soil and stem injection.

3.3) Mary et al 2018 introduces the "F2" inversion approach explicitly to improve upon the crude assumptions of the "F1" inversion (page 5432: "The F1 function can help guide the search for the region where the presence of active source is most likely to concentrate, but of course the use of F1 alone does not represent a realistic distribution of sources in the MALM inversion.")

I think the authors should thoroughly explain why the "F1" inversion is sufficient in this study, or even better, they should provide "F2" results. This would bring this text more in line with the other paper. Otherwise this could be interpreted as the authors downgrading their previous approach.

The reviewer is right, the F1 inversion is not sufficient per se to deduce root distribution but clearly convey the most interesting result of this article that is to say that the MALM results were corrected from variations of water content using ERT and thus are not sensitive to soil water content state which is the main problem for root detection using only ERT.

We took this comment into consideration and provide ALSO results from F2 inversion, and we took the opportunity to introduce some improvements as compared to the version used in the previous manuscript.

In the new version of the manuscript we adapted a robust algorithm to invert the data in 3D using the linearized form of the problem after Peruzzo et al. 2019 (Thesis.). The new features are (explained L. 246 to 260 of the revised version of the manuscript):

- *Regularisation using a L-curve analysis to control the regularisation weight.*
- *The code has been written for the 3D case and for an unstructured mesh.*

Most importantly all candidate sources are kept during the inversion of current density. Thus, there is no more a need to identify a threshold for which some sources are rejected. The misfit of F1 is transformed into a normalized initial model (m0) of current density via the inverse (1/F1) transformation. During the inversion of the current density, we adopted a relative smallness regularisation as a prior criterion for the inversion i.e. the algorithm minimizes $||m - m0||^2$, where m is the model parameter and m0 is a reference model to which we believe the physical property distribution should be close.

Result of F2 inversion are shown in the new version of the current manuscript.

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Line-specific comments:

4) line 39: what is the actual data that can be gained by the "new method"? In this regard, be more specific in lines 54/55: which data is required, which is extracted from MALM?

Sentence rephrased

In this study, we focus on new methods designed to image root systems and their macroscopic functioning, in order to help understand the complex mechanisms of these systems (the rhizosphere, e.g. York et al., 2016).

5) line 61: I think the "are" after "techniques" must be replaced by "is"

Ok done

6) line 106: "test" -> "tested"

Ok done

7) line 109: be more specific with regard to the field site and Mary et al 2018. I think it strengthens the study if the link to the previous paper is made more specific.

Ok done. Sentence added: "This paper is meant to be an extension of Mary et al. (2018) and to focus on an infiltration experiment"

8) line 114: as already stated, I'm missing this in the results/discussions

See response to comment 3.1 and 3.2 above.

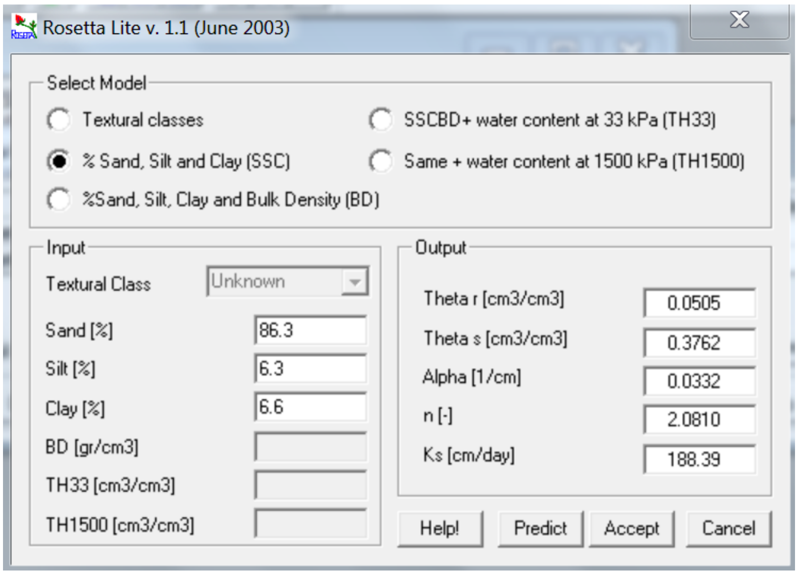
9) line 142: do you have any information on porosity/soil response to water content? Can you estimate an expected increase in resistivities due to the infiltration? Does the data fit any estimates?

These questions can now be answered thanks to the 1D hydrological modelling of the infiltration.

We here assumed that the retention and conductivity functions that describe the hydraulic properties of the soil can be represented by the Mualem-van Genuchten model (MVG, Mualem, 1976; van Genuchten, 1980.) Hydraulic properties of the soil were recovered directly from its grain size distribution – using Rosetta: see screen shot below.

Whilst this is a very simplified model, we were able to compare qualitatively the variations of resistivity with time with the infiltration simulation. To facilitate the comparison, electrical resistivities were transformed using Archie's law with the following parameters:

- *porosity, assumed to be equal to soil ϑs saturated water content;*
- *pore water electrical conductivity was assumed equal to the electrical conductivity of the water used for infiltration. The irrigation water had an electrical conductivity of 720µS/cm at 15°C. (0.072 S.m-1).*
- *m =1.3 (typical values notably described in Werban et al., 2008)*



10) line 165: can you be more specific on how you measure reciprocals for MALM? I would suspect totally different signal-to-noise environments, and correspondingly would deem the normal-reciprocal difference only as a weak proxy to data quality in this case, similar to nobody using normal-reciprocal differences for gradient or Schlumberger measurements.

Study of reciprocals were more thoroughly discussed in the first paper Mary et al. 2018. It has been shown that reciprocals were indeed not easy to correlate with data quality in MALM. This may be caused by non-linearities caused during current injection in the stem itself.

We rephrased the sentence to better state that reciprocals may not be the best solutions to estimate data quality in the MALM case.

11) section 2.4.1: can you provide error parameters used for the inversions? Is a target RMS of 1 reached for all time steps?

We added a sentence to explain this in the text (L. 212 and 215 to 218) and completed the table 1.

Table 1 shows the performance of the inverse model in absolute mode such as number of rejected measurements, and final root mean square (RMS) for an error level of 10 %. Most of the data converged after fewer than 5 iterations. Error level was assumed to be equal to the error used for the reciprocal analysis.

12) line 180: I suggest to rephrase and make clear that the voltage is measured with respect to the remote electrodes.

Sentence rephrased, see L225

13) equations 1 (and others): could you indicate vector/matrix entities? For example, in equation 2 it is not clear if the F1-value is the norm of all misfits for a given current injection, or only for the i-th value (I suspect it is the former, but in this case the notation must be corrected).

Equation 1:

Poisson equation was conserved in its general form without description of the vector/matrix entities.

Equation 2 and 3 were further described See L. 241 and 252 to 260

The reviewer is right, F1-value correspond only to the i-th value.

$F_1(D_m, D_{f,i}) = \|D_m - D_{f,i}\|_2$ *(2) changed to* $F_{1,i}(D_m, D_{f,i}) = \|D_m - D_{f,i}\|_2$

Equation 3:

$F_2 = \Phi_d + \lambda\Phi_m = W_\varepsilon \|d - f(m)\|_2^2 + \lambda(W_s \|m - m_0\|_2^2)$

14) line 186: I suggest to rephrase: the forward problem is unique, the inversion illposed. "relatively straightforward" is somewhat non-meaningful.

Sentence rephrased as (see L. 230 to 233):

"

Given a distribution of current sources, and once σ (x,y,z) is known from ERT inversion, the forward problem is uniquely defined and consists in the calculation of the resulting V field. Conversely, the identification of C(x,y,z) distribution given V(x,y,z) and σ (x,y,z) is an ill-posed problem, that requires regularization and/or a priori assumptions in order to deliver stable results.

"

section 2.4.2 (MALM) lacks quite some details to properly understand the approach and it took me a while to figure out that the details can be found in Mary at al 2018 (not only the discussion of different approaches). While you link to Mary et al 2018 in line 189, perhaps you could start the section with a sentence similar to: "The MALM analysis follows Mary et al 2018..." to indicate that this is just a short recap and the reader cannot expect a comprehensive explanation here.

The sentence has been changed according to suggestion (see L. 222)

Again, I wonder why you chose to only use the F1 approach. You state in lines 201ff: "While more advanced attempts could be made (such as the F2 approach also described by Mary et al., 2018) the simple F1 approach is capable of imaging the likely location of current sources in the ground, that in turn represent - according to our key assumptions – the locations where roots have an active contact with the soil." After reading through Mary et al 2018, I would expect a more detailed explanation, perhaps supported by numerical studies. Again, using only the F1 approach seems to contradict your findings in Mary et al 2018.

The reviewer's comment makes sense. We took two actions:

- *Plot the result of F2 inversion*
- *Rephrase the text to make it clear that F1 provides a feasible area of search to help F2 to converge (as explained in response to comments 3.2 and 3.3)*

15) line 193: I'm not sure if the term "likelihood" is suitable here, given that it usually is associated with stochastic/Bayesian problem descriptions. Why not call it a data misfit, or a current source RMS?

Ok changed accordingly: Likelihood has been replaced by misfit

16) section 3.1: Can you discuss more how you come to the conclusion that the intermediate depths are influenced by RWU (line 218-219)? Given the high irrigation rate of 115 l per hour (and ongoing infiltration), is the plant strong enough to take more water up than is infiltrated from above?

The question "is the plant strong enough to take more water up than is infiltrated from above?" finds a convincing answer (YES) in a previous work conducted also on Citrus trees (Cassiani et al.,2015) where this phenomenon is apparent.

Note that figure 3 has been changed to show ER absolute values and ER ratios for the full-time monitoring.

17) Are you sure that the high-conductive upper 40 cm in T1 are not cause by a somewhat higher porosity, compared to the layers below? (perhaps caused by the greater amount of roots reported in Mary et al 2018 for that layer?) Correspondingly, the anomalies in the "intermediate" region below could then be explained with a not-fully-saturated soil?

This is a speculation that is not supported by the data we have – although the presence of dense roots may increase soil porosity (at a larger scale than a typical soil sample, though).

18) Section 3.2/Fig 4: I'm wondering if showing measured resistances helps here, due to varying geometric factors. What about showing the ratio of measured voltage and the homogeneous solution (Fig. 4e)? Alternatively, just convert to apparent resistivities?

For a pole-pole acquisition and even more for a mise-a-la-masse acquisition, results as usually displayed using horizontal maps of normalized ***resistances – these are equivalent to voltages normalised on the injected current****. It has several advantages, in the perfect case, iso-potentials show directly the shape of the conductive body.*

19) line 250: "see Figure3Figure4" -> "see Figs. 3 and 4"?

Corrected.

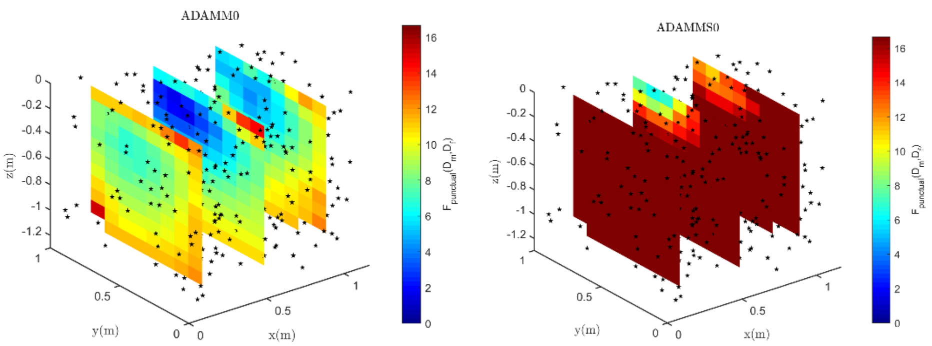
20) line 251: perhaps prepare an appendix with the corresponding results for plant B?

We have added an appendix for plant B

21) line 260: I'm wondering how the selection of the F1-threshold using the 25% percentile influences the delineation of the "active" root zone. In Mary et al 2018 their Figure 8 indicates possible threshold values of 35V (legend) or 17V (caption). Again, all for the same field site/plants. This seems inconsistent and should be discussed.

A value of the F1-threshold corresponding to the 25% percentile (here corresponding to 25% misfit) clearly influences the delineation of the "active" root zone as it can be appreciated from the misfit distribution below (time step T0 for the stem injection on the left, for the soil injection on the right).

See comment 23 for more details on the threshold determination.



22) In relation to 21, it would be nice if you could try to actually recover root information that could, theoretically, be used in the SPAC-modeling approaches, as motivated in the introduction. Can the MALM-approach provide information on root density, or only on root delineation? Can you extract an exemplary 2D rooting depth/rooting density map from your results?

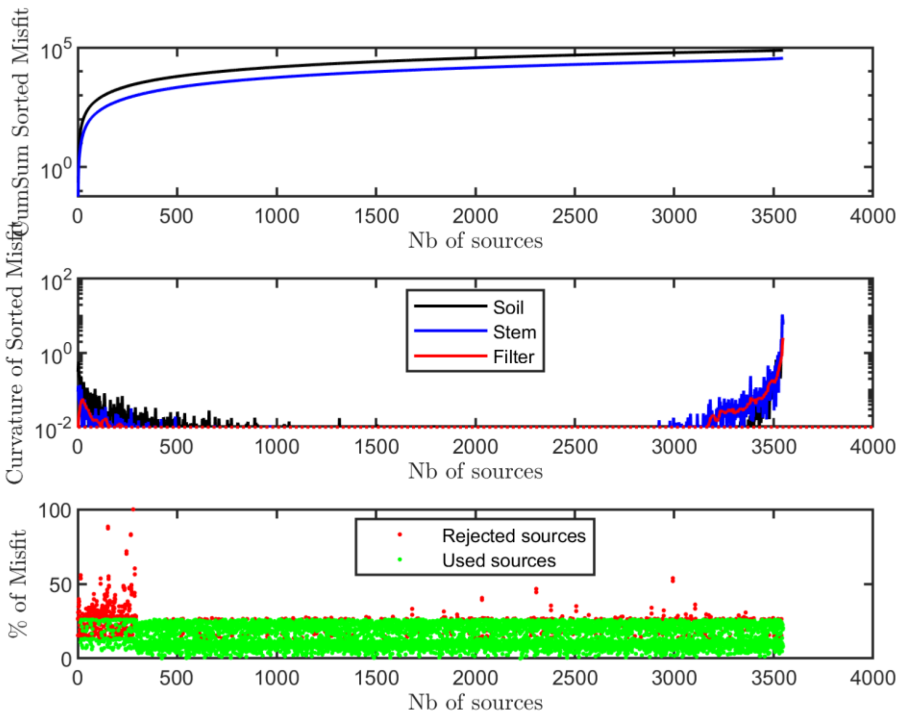
SPAC modelling approaches integrating root water uptake use empirical model describing the root system (Dupuy et al. 2010) and MALM could indeed provide relevant information in this respect. Our approach is adaptable to macroscopic RWU models that describe root water uptake as a sink, microscopic ones being too complex since they required knowledges in root hydraulic architecture. This has been more extensively considered and discussed in this revised version of the manuscript.

Using the average value of current density along horizontal planes we were able to plot the vertical profile which was tentatively used as rooting profile density for the 1D hydrological simulation.

We discussed the limitations of MALM and the uncertainties on delineating the root system considering that only the active root system may be highlighted. In some cases, the active root system is really local as compared to the entire root system extent and thus the picture we can retrieve from excavation. As far as enough data are available to describe time varying *active root location MALM can improve macroscopic RWU modelling. Recommendations to use destructive methods to complement MALM methods were also introduced.*

23) line 260: could you provide the curve of sorted F1-misfits in the appendix? This would ease the understanding of the F1-threshold.

We rephrased the sentence to make it clearer, but we did not overload the appendix with further figures. Below the curve of sorted misfit for T0 of plant B. See L. 320



24) line 268: I suggest to rephrase "absolute apparent"

Done

25) line 277: the F1-threshold of 7V is used to delineate root and soil zones?

Yes. We added a sentence to make it explicit. L. 345 to 348.

26) line 289: could you be more specific as to the resolution achieved here? My understanding is that the best resolution would be roughly the smallest electrode distance, which would be 10 cm here. Is this "high-resolution" in the context of root research?

Sentence rephrased

27) line 294: "...independent information... may help". Could you be more specific on how this additional information could be included into your workflow, apart from validating the results? Would it be possible for you to include any of this data in your inversion workflow, thereby minimizing uncertainties?

The first ERT-MALM coupling is <u>already integrated in the workflow</u> in the sense that we considered MALM as a non-soil water sensitive information to define the possible area of root. Separation of contributions of root zone and outer area on ER values extracted from ERT help distinguish between processes such as RWU and hydraulic redistribution (hydraulic lift in particular). This approach is more realistic compared to other approaches who used horizontally layered separation (Vanella et al.) to describe and separate the contributions.

The paragraph has been modified. See L. 384-393 of the new revised manuscript.

28) lines 296-302: I cannot follow your reasoning regarding the "second coupling" of ERT and MALM. By design MALM requires ERT results, and thus I'm having problems seeing this as a conclusion.

Sentence removed

29) line 303: I suggest to rephrase "successfully tested", as without validation we still do not know how reliable the results are.

Ok done

30) lines 310-310: "The soil injection leads practically to identifying the true single electrode location." I'm having trouble understanding the meaning of the sentence...could you elaborate or rephrase?

Sentence rephrased:

"

The soil injection leads practically to retrieve a current density close to a punctual injection (located at the true single electrode location)

"

31) line 310: I don't get the meaning of the first sentence, given that ERT and MALM solve completely different inversion problems, which guarantees that the results differ. Perhaps rephrase with regard to the emerging patterns of the results?

Here we described the situation where the stem electrode is replaced by one in the soil but close to the stem (we are not referring to ERT!).

The sentence was rephrased to make it clearer.

32) Conclusions: I fail to find much discussion of the time-lapse character of the present study, and much overlap with the aims/conclusions of Mary et al 2018. Perhaps you could focus the conclusions more on time-lapse-specific questions/answers? Does MALM provide more robust information in the light of varying water content regimes? Can MALM provide information on the actual RWU, perhaps by correlating to the estimated evaporation?

In the new version of the manuscript we try and correlate ER variations with estimated evapotranspiration and RWU. This also contributed to the second paper objectives which wasn't really fulfilled in the first version.

Questions "Does MALM provide more robust information in the light of varying water content regimes? Can MALM provide information on the actual RWU, perhaps by correlating to the estimated evaporation?" have been specifically addressed in the discussion section.

33) Will you provide all primary data and analysis results as a data repository, as required by SOIL?

Yes. We added a statement on "Data availability" section.

I sincerely hope these comments are not taken personally, but as constructive comments aimed a improving the message of the manuscript.

We are grateful to the reviewer, for its thorough reading of the manuscript and for all the very relevant comments.

Best regards

References:

Cassiani G., J. Boaga, D. Vanella, M. T. Perri, S. Consoli, 2015, Monitoring and modelling of soil-plant interactions: the joint use of ERT, sap flow and Eddy Covariance data to characterize the volume of an orange tree root zone, *Hydrol. Earth Syst. Sci.*, 19, 2213-2225, doi:10.5194/hess-19-2213-2015.

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