

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

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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 publica-

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tion 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.

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.

2) 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):

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."

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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.

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).

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.

————— 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.

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?

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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 insensitive 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.

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.

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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?

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

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

7) line 109: be more specific with regard to the field site and Mary et al 2018. I think it

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strengthens the study if the link to the previous paper is made more specific.

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

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?

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.

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?

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

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).

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

15) 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 et 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.

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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.

16) 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?

17) 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?

Are you sure that the high-conductive upper 40 cm in T1 are not caused 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?

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?

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

20) line 251: perhaps prepare an appendix with the corresponding results 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

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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.

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?

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

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

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

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?

27) line 294: "...independent information... may help". Could you be more specific on how these 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?

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.

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

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

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you elaborate or rephrase?

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?

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?

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

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

Best regards

References:

Mary, B., Peruzzo, L., Boaga, J., Schmutz, M., Wu, Y., Hubbard, S. S., and Cassiani, G.: Small-scale characterization of vine plant root water uptake via 3-D electrical resistivity tomography and mise-à-la-masse method, *Hydrol. Earth Syst. Sci.*, 22, 5427-5444, <https://doi.org/10.5194/hess-22-5427-2018>, 2018.

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