SOIL Discuss., https://doi.org/10.5194/soil-2019-28-RC2, 2019

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 #2

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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.

This manuscript deals with the application of geoelectrical methods for imaging and monitoring roots activity and water flow in the context of the soil-plant atmospheric continuum. The authors conducted time-lapse ERT and MLAM surveys around two grapevines plants differing in their age. The subsurface electrical resistivity was monitored before and after an infiltration experiment. The ERT data were inverted using a standard algorithm, and a simple algorithm for the imaging of the current source distribution was used. The work presented here is an extension of Mary et al (2018), but with the addition of an infiltration experiment, demonstrating the ability of the combined methods to monitor water content and RWU dynamics. Overall, the work presented is interesting for the reader of SOIL, the manuscript is well written, and the methods and data analysis are adequate.

The main pitfall is the lack of supplementary information that prevents a quantitative analysis of the (very interesting) dataset. Specifically, water content and water salinity were not measured or assessed. Differences between the transpiration of the two plants were not considered or measured (e.g., with sap flow meter). Nevertheless, even if this data is not available, the time lapse MALM provides qualitative information, at a high spatial resolution, on water content dynamics and RWU processes. In conclusion, I recommend publishing after some moderate revisions.

General comments:

1. The limitations in the interpretation of the results due to the lack of supplementary data (water content, salinity, formation factor) should be discussed in details.

We acknowledge the limited availability of supporting data. However, in order to strengthen the paper's conclusions we took two actions:

a) *We added a 1D hydrological modelling of the infiltration:*

Whilst this is a simplified model, we used a petrophysical transformations (Archie) on measured ER and thus recovered the spatial and time variations of soil water content.

b) *We discussed the limits of validation data in our case:*

We do agree that ancillary measurements are always welcome to support the geophysical information. Nevertheless, in this specific case they also have important limitations:

- *Validation through root excavation has numerous potential pitfalls. Among them, the destruction of fine roots during extraction that may prevent us from correlating root system architecture et geophysical observations. Discussed L417 to 423*
- *SWC can only be measured at few specific spatial locations. Discussed L. 425*

2. In my view, one of the most interesting parts is the maps in Figure 5, showing the time-lapses differences between the young and old plants. However, a discussion on this observation is missing. Figure 5 is not mentioned in the text at all (perhaps in L252). I would strongly suggest to give a detail explanation of those results and to link them to the expected behavior of the different plants.

The reviewer might have misunderstood the meaning of the Fig. 5. Maps in Figure 5 showed differences between the MALM stem injection and its companion soil injection (as explained in the section MALM acquisition 2.3). Also, reviewer 3 reported that the figure was redundant with Fig. 4 and was not sufficiently mentioned. We then decided to remove it and improve Fig. 4 instead to better convey the idea i.e. raw map of MALM potential contains time lapse information and show significant differences between stem and soil injection.

As for the comparison between plants A and B, this has not been neglected since in the initial version of the manuscript we discussed the differences between the two plants (see L. 265/266, 275/276, 314, 319, …).

Specific comments:

1. L123: I guess that the water holding capacity is related to the pore size distribution and not to the porosity.

Correct. We rephrased the sentence accordingly.

New sentence: "

Due to its larger particles and thus high porosity and smaller surface area, the sandy layer has a relatively poor water retention capacity.

" See L. 132 of the revised manuscript

2. L142: you report the EC of the irrigation water, but what is the EC of the pore water? Do you expect heterogeneity in the pore water salinity? This should be discussed.

For the hydrological model reproducing the infiltration test, pore water conductivity was assumed equal to electrical conductivity of the water used for infiltration.

We have good reason to think that this assumption can hold in our case since the infiltration was relatively intensive (> 100L/h) and the initial soil pores were then filled with irrigation water. We nevertheless agree with the reviewer comment that EC of the pore water is not necessarily equal to the EC of the irrigation. We did not consider specific rhizosphere processes such as root exudation, which could affect the water content estimates. We assumed, no salt accumulation taking place near the roots, i.e. passive solute uptake only with no active uptake, exclusion or exudation. Solute movement models exist to consider the different root processes that might affect the constant concentration in water pores. Significant solute gradients may arise around roots due to processes mentioned above. If they were to occur, the water content estimates could be impacted by such gradients. However, in this specific case the volume of irrigated water and the short residence time is likely to make these processes second order effects.

3. L223: Due to the lack of supplementary information, the arguments about the size and extent of the root systems are not solid enough. Is it the size of the root system or the total transpiration that differ?

If the reviewer is talking about the differences between the shape of the two plants, we do agree that a pure comparison of their root system is not realistic from our result. Nevertheless, this is clearly not the point of this study.

Nevertheless, the reviewer raised an interesting question that we tried to address in the discussion (see section 4.1 of the revised manuscript)

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