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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Inter-laboratory comparison of cryogenic water extraction systems for stable isotope analysis of soil water" *by* Natalie Orlowski et al.

D. Penna (Referee)

daniele.penna@unifi.it

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General comment:

This manuscript reports the results of an intercomparison exercise that aimed at testing the consistency of cryogenic water extractions for the analysis of stable isotope of hydrogen and oxygen in soil water among worldwide-distributed laboratories. In the last few years, the ecohydrological and soil science international communities have shown a strong and increasing interest in better understanding the functional interrelationships between soil and vegetation based on the use of stable water isotope data. The cryogenic water extraction technique has so become a golden standard for sampling water

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from the unsaturated zone. Very recent studies, often conducted by the first author of this manuscript and colleagues, already showed some potentials and limitations of this technique, and provided helpful information for users. However, a worldwide interlaboratory comparison among several cryogenic extraction facilities was still missing, thus this work it is certainly welcome. Indeed, I believe that this manuscript is timely and of great interest for the readers of this journal.

The manuscript is very well written, logically structured, nicely illustrated, and the conceptual steps can be followed very well. The working hypothesis and the specific objectives are well posed, following a substantial introduction, the statistical analysis is correct, and the results and interpretation are well supported by the data. The methodological approach leading to the comparison exercise was solidly defined and clearly presented.

As noted by the Authors themselves, the large differences in performance found among the labs included in the exercise are somehow worrisome and pose questions on the possible adoption of cryogenic water extraction as a standard method for soil water sampling. However, these results are very relevant to the scientific community because implicitly suggest cautions in comparing isotope soil water data extracted by different facilities, and indicate that much technical work is still needed to test possible further controls on these differences and develop new techniques able to return more consistent results.

I have only the following minor comments for the Authors to address.

Minor comments and technical corrections:

P4 L11. Here, and throughout the rest of the manuscript (e.g., P7 L16; P9 L12-14; P11 L15...), it is not immediately clear what “isotope results” are, and I suggest to replace this term with “values” or “data”.

P5 L14. The authors reported that the soil was homogenized before shipping. How-

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ever, as noted by the other reviewers, I wonder if possible heterogeneities in the analysed soil samples (especially for small volumes) could have been present and could have affected the results.

P5 L27. I find the definition of “water-water” cryogenic extraction a bit confusing. I suggest to use, throughout the manuscript, simply the terms “water extraction” vs. “soil water extraction”, or something similar.

P7 L1-2. Although intuitive, I suggest to add a short explanation about the choice of applying different extraction times for the silty sand and the clayey loam soils.

P8 L28, and P12 L6-20, and Figs. 5-6. It seems to me that these are not “true evaporation lines” describing the progressive isotopic enrichment of an individual source water (see Benettin et al., 2018 who reported the often misused term and concept of evaporation line). This might not be a critical point in the interpretation of the results and the overall meaning of the research. However, for the sake of accuracy, I suggest to check this and in case change the terminology (eg, simply calling them regression lines) and slightly re-interpret the results reported at P12 L6-20. Moreover, it’s not very clear to me why in the left panels of Figs. 5 and 6 (8% WC) one regression line (for 8% samples) is reported in addition to the GMWL whereas in right panels of Figs. 5 and 6 (20% WC) two regression lines are shown (both for 8% and 20% WC). Please, fix this or explain.

Benettin, P., et. al. Effects of climatic seasonality on the isotopic composition of evaporating soil waters, Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2018-40, in review, 2018.

P9 L23-24. Do the Authors have any idea about the reason for recovery rates higher than 100%? Could this somehow affect the results? Perhaps a sentence could be added here (trying to avoid the risk of speculation).

P11 L27. In addition to the statistical results, I wonder whether it might be appropri-

ate to show OA-ICOS and IRMS data as boxplots to graphically stress the difference between values returned by the two techniques.

P17 L23. The reference of “Orlowsky et al., 2018” is missing from the reference list.

Fig. 1. I suggest to increase the size of the axis labels.

Fig. 3 and Fig. 4. In the caption: was the mean computed among the three replicates? If so, I suggest to specify this for the sake of clarity.

Fig. 5 and Fig. 6. I suggest to add in the caption that the legend includes explanation for the symbols used for the 16 labs and the two extraction approaches.

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