

***Interactive comment on* “Technical note: an alternative water vapor sampling technique for stable isotope analysis” by César Dionisio Jiménez-Rodríguez et al.**

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We would like to thank the reviewer for his/her valuable comments on our manuscript. We appreciate that the reviewer acknowledges the interest and relevance of our study. Nonetheless, the reviewer indicated some issues which we will clarify point-by-point:

In my opinion, the two main important aspects not justified are:

1) The isotopic differences between the lab air sampled directly, and lab air sampled with the bags are significant. Table 1 presents larger differences between the lab air directly sampled (Laboratory) and sampled with the bags (sample D), than differences

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between bag sampled laboratory air (sample D) and forest air (sample A). These differences are not explained, nor justified in the manuscript.

Reply: Thank you for pointing out this issue. In fact, this interpretation is a misunderstanding from our methods where it was not clearly explained that the sampling date in the laboratory is not the same as when the samples were analyzed. Hence 'sample D' and 'laboratory' should not have a similar isotopic value per se. In method section 2.2.4, page 7, line 3 we omitted to mention that Sample D was collected one week before the measurement at the laboratory. We added the "laboratory" sample as a background validation during analysis to show the "Laboratory" as a proof of that no laboratory air was leaking and/or mixing within the inlet used to convey the air from the sampling bag to the Multiport Inlet Unit. We agreed that we haven't mentioned the time difference between the sample collection and sample analysis.

Consequently with the finding from the additional experiment, we decided to remove this section from the experiment to be replaced with the additional experiment (Supplemental Material).

2) The field experiment comparing the isotopic composition of the air sampled with the bags and the air sampling with the cold trap method gives important differences between methods. Then, authors conclude that differences are due to inappropriate results are given by the traditional cryogenic collection technique, compared to results given by the method proposed. However, there is no clear justification of this conclusion in the manuscript.

Reply: We agree with the reviewer that this comparison is not well explained. Our original aim of Experiment 3 was to 1) compare our method to the traditional cold trap method, and 2) test its application in a field setting. Unfortunately, Aim 1 was not well carried out since the cryogenic extraction was not well carried out. For the cryogenic trap method in the field, we used a pumping rate of 3 L min⁻¹. This high rate was necessary to convey the air from the tower towards the sampling point used during

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the sample collection (page 8, line 8-9: "... sucking the air at a rate of 3 L min⁻¹ for a travel time of less than 2 min from the sampling point to the collection point."). The problem of this high pumping rate is that it does not allow for a full condensation since the time in the cold bath is too short. Hence the large variability of the cold tap samples and the deviation between the cold trap samples and our method is likely due to this issue. However, after we carried out the additional experiment added as supplementary material to this reply we show the strong differences between the cold trap samples and the LDPE samples, MPE samples, PVF samples and the benchmark (direct measurements from the WVIA). Based on these results we decided to shift the scope of the paper and delete experiment 3 from the manuscript since the LDPE-samples and the cold trap were not reliable measurements.

Specific comments:

In addition, there are several statements that are not clearly justified. (for example: lines 7-8 in page 11; lines 17-19 in page 12; lines 4-5 in page 13; lines 7-10 in page 14). For that reasons, the manuscript cannot be recommended to be published in HESS.

Reply: Page 11, lines 7-8 were clarified on the first paragraph (point 1) of this reply.

Page 12, lines 17-19 says: "Sampling bag signatures depict an isotope signature slightly evaporated. This mixture of vapor could be originated from water evaporated from intercepted surfaces and transpired water from different sources than previous rain events." This sentence aims to provide a possible explanation about the source of the water. However, as the paper's objective is about the applicability of this method we propose to remove both sentences from the paper.

Page 14, lines 7-10 say "Thus, because of the homogeneous mixing within the sampling bags allowing isolating the sample from the surrounding air, preventing contamination and/or mixing during transport and analysis. The homogeneity reached by the air sample within the sampling bag allowed retrieving a better temporal variation than

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cold traps during experiment 3.”

The homogeneous mixing within the sampling bags is mentioned on page 11, lines 2-3: “Air samples collected with the 1.1 L bags were able to provide a stable signature after two minutes of analysis (Fig. 6)”. However, after the performance of the additional experiment added as a supplementary material to this reply we agree with the reviewer that the LDPE bags do not provide a full protection for contamination as a consequence of the bag’s diffusivity for water vapor as it is explained by Tock (1983) and later on mentioned by Herbstritt et al. (2014.).

References

Herbstritt, B., Limprecht, M., Gralher, B., Weiler, M.: Effects of soil properties on the apparent water-vapor isotope equilibrium fractionation: Implications for the headspace equilibrium method [poster presentation], UNI Freiburg. Available at: <http://www.hydro.uni-freiburg.de/publ/pubpics/post229>, 2014.

Tock, R. W.: Permeabilities and water vapor transmission rates for commercial polymer films. *Advances in Polymer Technology: Journal of the Polymer Processing Institute*, 3(3), 223-231. <https://doi.org/10.1002/adv.1983.060030304>, 1983.

Please also note the supplement to this comment:

<https://www.hydrol-earth-syst-sci-discuss.net/hess-2018-538/hess-2018-538-AC2-supplement.pdf>

Interactive comment on *Hydrol. Earth Syst. Sci. Discuss.*, <https://doi.org/10.5194/hess-2018-538>, 2018.

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