Reviewer 3 - Anonymous

The manuscript titled "OF-CEAS laser spectroscopy to measure water isotopes in dry environments: example of application in Antarctic" by Lauwers et al. is a much needed study on a promising technique to measure water vapor isotope composition at very low humidity. The manuscript includes all of the information one would hope for to employ their method and requires only minor revisions before publication. Indeed, the authors would go a long way towards publication readiness by clarifying the text and refining the grammar.

We thank the reviewer for his careful reading and recommendations. The answers are inserted in red and the citations from the manuscript are indicated in blue.

General comments for clarity:

- Make the figures and tables, including their captions, more independent of the main text. Telling a reader the same information twice (e.g., in the figure and in the main text) helps with clarity.

Thank you for this comment, we are adding all the information needed to understand the figures, both in the main text and in the figure captions.

Instrumentation description:

- I expect this study and instrumentation to be popular among water isotope laboratories and researchers. As such, please describe the specific model of AP2E used. Or, if all three are custom, please indicate so. Consider including a brief description of how these particular OF-CEAS instruments differ from those in Casado et al., 2016. Are they the same?

The two OF-CEAS instruments presented in this study correspond to the Proceas © model, while the first instrument deployed by Casado et al. in 2016 was a prototype assembled at the LIPhy laboratory (Grenoble). The technique compared to the laboratory prototype remains the same, but with improvements in terms of temperature regulation, pressure regulation and robustness (electronics, optomechanics). We added a paragraph in the manuscript at the beginning of section 1.1 to explain the specificity of this new model.

- Use of the description "V-shape" does not help the reader understand the instrument. Perhaps it would be more informative to say something about the lack of fiber optic cabling in favor of highly reflective mirrors?

The reason why the cavity has a V-shape is indeed not explained, because that would be beyond the scope of our paper and has been already been documented in the literature. We feel it necessary to be accurate, at least in the introduction, by stating that the OF-CEAS cavity uses a V-shaped cavity, as otherwise efficient optical feedback would not be possible. The V-shape cavity is essential to cancel direct reflection and only send back to the laser the resonating light, as the incident light is injected with an angle. Additionally, the V-shape enables it to reach twice the length of a normal linear cavity and increase the laser light/gas interaction length while keeping a compact size (40 cm long).

We added the reference to chapters 1 and 5 of Romanini and Morville 2014 in the introduction, which gives a detailed description of the OF-CEAS principle, including the principle of the V-shape cavity. In subsection 1.1, we added a paragraph to introduce the elements discussed here, and cited Clement Piel et al. 2024 who describe in detail the OF-CEAS set-up such as implemented by the AP2E company.

2.1.2 Long-term stability at Dumont d'Urville station:

- Do the authors expect the variable water vapor concentration requiring "An additional filtering has been applied to remove the points with a non- stable humidity, i.e. with a humidity value standing outside the 2-σ interval." is due to the LHLG unit or to the OF-CEAS instrument itself?

The non-stable humidity is due to the LHLG unit. The absolute humidity traces measured by both CRDS (Picarro) and OF-CEAS techniques appear to be nicely superposed. Moreover, the precision for the absolute humidity on these analysers allows us to clearly observe the LHLG humidity overshoots and instabilities, while we are looking at signals that are 2 to 3 orders of magnitude smaller when measuring isotopic ratios.

Figure 5:

- De-emphasize the green data by making it a lighter shade and placing them behind the OF-CEAS data.

- I suggest expressing the y-axes as residuals in the same way the authors have already done with Figs 3 and 6. The wide range that must necessarily be used when expressing the values in their absolute terms may hide drift or biases.

Thank you for this useful remark. For a better clarity of the figure, we centered the y-axis around the mean isotopic value, with the same scale between the left panel (AO1 standard) and right panel (FP5 standard). We removed the error bars which did not provide very useful information and overloaded the figure. We also added the standard deviation of the whole series. The numerical values are displayed in Table 1 to avoid overloading of figure 5.

- The caption sentence "Each point represents the average of the final minimum 5 minutes of 1000 ppm humidity plateaus and the error bars correspond to the associated standard deviation" is unclear. Does "minimum" imply that sometimes it is longer? Be specific.

Thank you for your comment. Ideally, we take a 10-minute window for the drift calibration value, which ensures a stable isotopic and humidity value. However, it can happen that the plateau is not completely stabilized over the last 10 minutes (or that the occurrence of an air bubble generated by the calibration instrument creates instabilities), so in this case we reduce the time window with a lower limit that we set at 5 min. The caption has been modified accordingly.

- Why would "plateaus" exist with this particular dataset? The caption currently reads as if all of these data were collected at 1000 ppm. Please clarify.

The value of 1000 ppm corresponds to the theoretical value delivered by the LHLG with the corresponding settings (water and air flux, pressure, etc.). In reality, the generated humidity is affected by instabilities of the water flux and varies from one calibration to another, leading to a typical standard deviation of ~ 30 ppm of the humidity over the presented calibration dataset in fig. 5. These small variations of the humidity are negligible in terms of impact on humidity dependency correction of the isotopic value. So the measured d18O and dD signals during the calibrations do not differ from what would have been obtained at exactly 1000 ppm. We are clarifying this aspect in section 2.1.2.

Table 1:

- If these variance estimates are from the data presented in Fig 5, I suggest deleting this table and stating these values in the Fig 5 caption text. If the authors choose to keep the table, state in the table caption that the variance estimates are from the data presented in Fig 5. If they are not from the data in Fig 5, please describe their origin.

Indeed, Table 1 gives the standard deviation calculated from the time series plotted on Figure 5. As indicated in a previous answer concerning Fig. 5 (referee 1), we decided to indicate the standard deviation in a graphical way to avoid overloading the figure, and specify the exact numerical value on this table. This will be stated in the caption.

Figure 6:

- Please describe in the caption what is meant by "ref" in the y-axis titles.

We added this sentence in the caption:

All curves are referenced to the isotopic composition measured at 500 ppm (left panel) and 1000 ppm (right panel), denoted “ref” on the y-axis.

Table 2:

- Does "lightly depleted" and "highly depleted" refer to standards shown in Fig 6? Please describe in the table caption what is meant, or better yet, remove the ambiguous terms in favor of the actual reference water names.

We added the actual standard names which we use as internal references, i.e. AO1 for the lightly depleted standard and FP5 for the highly depleted standard.

Figure 8:

- The caption is confusing. The authors are not plotting Allan deviation after 24 hours, they are plotting the predicted standard deviation after 24 hours of integration as predicted from an Allan variance analysis.

- The wording describing the dotted lines is confusing. It currently sounds as if they are the predicted range one would expect during the course of a 24 hour period. The main text, however, suggests these dotted lines are the authors desired "noise threshold".

- Is this threshold one standard deviation or perhaps a 95 % confidence interval? Please reword the caption.

- Make the right side axis title and tick labels red, in the same way the authors made the left side blue.

- Lastly, the font size of the stations on the map need to be much larger.

Thank you for these comments which will help make the figure more readable and less confusing. The figure 8 caption has been changed to correctly describe the Allan deviation and the threshold. The threshold corresponds to the upper limit for the standard deviation as predicted from the Allan deviation study.

Here is the new caption:

"On the left, histogram representing the year fraction (expressed in %) below a fixed humidity content for 4 sites situated along the transect. For each humidity, we plotted the associated standard deviation after 24 hours of integration σ(δi) as predicted by the Allan deviation study for $\delta^{18}O$ and δD. The dotted horizontal lines represent the $\delta^{18}O$ (red) and δD (dark red) upper thresholds for the standard deviation to confidently study the diurnal cycle. These thresholds are set at a value 10 times lower than the amplitude of the diurnal cycle, to ensure a correct signal resolution (see discussion in the text). On the right, map with the location of the 4 instrumented sites for the AWACA deployment."

The right side axis, title and tick labels are now in red, the size of the font for the stations is larger.

Section 3.1

- This section would be better positioned after the discussion since it is a proposed application of what the authors intend to do after having completed the current study.

Thank you for this comment. Indeed, the proposed structure for section 3 was in the original manuscript and seemed more obvious at first sight. But during the co-authors revisions, we found that a number of elements discussed in the current section 3.2 were not clearly introduced, which caused some repetitions in the manuscript and a loss of clarity. For example, in line 327 we use the result plotted in fig. 8 from the previous section. Again, in line 341, among the proposed solutions for improving the measurements, we discuss the calibration strategy, which was introduced in the previous section.

The discussion thus appeared more grounded when presenting first a concrete example of application and the logic of section 3 is the following: in subsection 3.1, the results of section 2 are discussed and summarized (Fig. 8) to propose a calibration scheme for an example of application in Antarctica (AWACA project). With this specific application in mind, the aim of subsection 3.2 is to give the limits and advantages of the OF-CEAS instrument compared to the state-of-art Picarro CRDS, and finally give some perspectives of improvement. In subsection 3.2, we propose for example a number of ways to improve the annual coverage of water vapor isotopes (shown on Fig. 8), such as increasing the calibration frequency. We also discuss in the last paragraph the advantage of using OF-CEAS instrumentation in the field for the specific case of autonomous operation in remote sites, such as in the AWACA project.

Figure 9:

- Similar to my comment for Fig 8, reword "The noise is obtained from short-term Allan deviations at τ = 2 min..." to something like "Standard deviation is predicted from an allan variance analysis for the 2 minute integration time...".

Absolutely, the caption has been changed.

Discussion Line 325:

- The sentence "It shows an optimum stability range of ~ 15 min, followed by a drift period in the hour range and finally a stabilisation of the signal in the day range" is confusing. Are the authors referring to the #1169 instrument at 500 ppm H2O and d18O? The blue series is the only series that does seem to be achieving stability in the day range. All others continue to drift or have insufficient data to make any conclusions about their trajectory (e.g., dD #1169).

Indeed, the system does not reach a clear stabilisation, but the AD curves clearly show that although the drift does not disappear in the day range, the slope of AD slows down after 10 hours and its value lies in a satisfying range. We are modifying the text accordingly.

Interesting features for field operation:

- This is an important section and seems to compare OF-CEAS to Picarro's CRDS. The glue, the inability to clean, lack of needed software. I suggest the authors state plainly that AP2E's design and software are more conducive to remote field deployment than Picarro's CRDS.

We believe indeed that for our specific application, the system designed by AP2E is better suited than Picarro, and we modified the paragraph to clearly mention Picarro’s CRDS. But this has to be put into perspective: in the case of water vapor isotope observatories at mid-latitudes where the humidity is not so low, there is an interest in Picarro systems which are particularly adapted: they are very robust, more “plug-and-play” and show higher performance that what would be obtained with AP2E OF-CEAS technique.