Review of “Characterising water vapour concentration dependence of commercial cavity ring-down spectrometers for continuous onsite atmospheric water vapour isotope measurements in the tropics” Komiya et al. AMT

Cavity ring-down spectrometers have been increasingly used in water cycle dynamics studies that employ stable water isotopes as tracers for moist processes in the atmosphere and at the land-atmosphere interface. This paper presents an interesting evaluation of the water vapour mixing ratio dependency of cavity ring-down laser spectrometers focusing on the high specific humidity range that is encountered in tropical regions such as the Amazon.

In general, I found this paper interesting and mostly well written, with some instances, where it was more difficult to follow in particular in Section 2.3 about the calibration strategy of the water vapour mixing ratio dependence as well as the results part. I very much liked the precise description of the self-made calibration unit and find the approach of the authors very nice. Maybe they could add a bit more information on the long-term stability of their calibration system as mentioned in my minor comments below.

I recommend publication of this manuscript after the following three major comments and several minor comments below have been adequately addressed.

Major comment:

1) The description of the calibration strategy in Section 2.3, especially with respect to Figures 2 and 3 was very difficult to follow. It would help if Figure 3 had panel numbers to which the captions could refer to. In general the figure captions could be improved to help the reader understand the Figures. Also the text should guide the reader better in understanding Figs. 2 and 3.
2) The results would benefit from a better structure within the sections. In particular, I would find it nice if the general findings valid for both instruments would be presented first and then the details about the L1102 and L2130.
3) The final recommendation of the paper to perform weekly or even more regular water vapour mixing ratio calibrations is surprising (e.g. P 16, L1-3), because an overwhelming majority of studies until now found that the water vapour mixing ratio dependencies of cavity ring-down systems from Picarro remain relatively constant in time and only occasional full water vapour mixing ratio dependency experiments were necessary, mainly for monitoring purposes. I therefore think the authors should discuss their recommendation and finding in this context of the existing literature. How much do you gain in permil uncertainty reduction for d18O and d2H (and maybe dexcess) with performing regular water vapour mixing ratio dependent calibrations compared to just applying a drift correction and a water vapour mixing ratio dependency correction that is constant in time? This is important because a lot of ambient measurement time is lost with the time consuming calibration scheme proposed by the authors.

Minor comments:

P 1, L 15: “including…” I would slightly rephrase to “which includes the correction of the H2O concentration dependence of isotope measurements”

P 1, L16: “past studies have assessed **the** [H2O]-dependence…”

P 1, L23: “… two pairs of a two point calibration with four different H2O concentration levels” sounds a bit obscure to me in an abstract. I think I see what you mean after having read the paper several times, but, maybe a more explicit formulation would be helpful

P 1, L29: Overall very clear and nice abstract. I would find it nice to finish it with a less technical and more general sentence on a scientific level. This could be for example that this study shows that measurements in the tropics are in principle possible also at very high humidity levels, which has promising implications for water cycle studies focusing on tropical regions. But maybe the authors have a better idea for such a final zoom out sentence.

P 1, L 35: Maybe Dansgaard 1964 and Craig and Gordon 1965 would be good studies to cite here in addition to the more recent review by Galewsky et al. 2016.

P 1, L36: Here maybe specific modelling studies would be good to cite: Risi et al. 2010, Werner et al. 2011, Pfahl and Wernli 2012 instead of a review paper. I think the statement “has also improved simulations of hydrometeorological fields” could potentially be misunderstood. Isotopes are implemented in different global and regional circulation models to improve our understanding of how stable water isotopes are transported in the atmosphere and affected by phase changes in clouds, below the clouds and how they behave in different situation of surface-atmosphere interactions. Of course, the idea is to learn more about these moist processes through these isotope modelling studies. But, stable water isotopes are implemented as *passive* tracers in these models and do not directly impact other hydrometeorological fields.

P 2, L6: Maybe here the polar regions, midlatitues and subtropics/tropics could be mentioned separately. There are many studies based on water vapour isotope measurements available in polar and midlatitude regions, some in the subtropics (e.g. Gonzalez et al. 2016, Bailey et al. 2013) but only very few in the tropics (e.g. Tremoy et al. 2012, Aemisegger et al. 2020). In particular studies over tropical continental regions are rare.

P 2, L16: Replace “confirmed” by “showed” because this was not shown before, and remove s in “an older model”.

P 2, L 26: This statement about the diel cycle comes a bit abruptly after the discussion about older studies of water vapour isotopes in different humidity ranges. Maybe a new paragraph would help and a smoother transition from mainly instrument technical aspects to previous observations in nature.

P 2, L 33: ATTO is a new abbreviation to me

P 2, L35: Old and new instead of older and newer, the latter would require a “with respect to what” statement

P 2, L42: “sufficiently detect…” sounds a bit awkward. Maybe something like “based on the uncertainty quantification presented, we discuss whether the CRDS analysers can detect natural signals of …”.

P 3, L16: To which water vapour mixing ratio level does this dew point correspond?

P 3, Section 2.1: In general: very nice self-made setup and description of it! I would be curious to see it in operation. A picture of the full setup next to the schematic in Fig. 1 would

be great! Also could the authors comment on the long-term stability of their system? Are there any failure-prone parts in this system? E.g. how about the Syrringe pump?
P 3, L 26: Could you indicate the total flow rate of the CRDS instruments? A recent study has shown that the water vapour mixing ratio dependency is actually depending on the instrument's flow rate (see supplement of Thurnherr et al. 2020)
P 3, L35: Could the authors shortly mention how this correction is implemented? Were the two CRDS instruments also connected to a dew point generator at different dew point temperatures to ensure that no bias affects their water vapour concentration?
P 4, L13: Also use humidity or water vapour mixing ratio level and not "moisture level"
P7: I had difficulties to understand Fig. 2. Is this actually a Figure or a Table? Maybe the caption could be a bit more detailed. And why does the DI1 strategy contain Cali. ID 1 to 3 and not only 1 and 3? Same for DI2, why is it not 2 & 4? And why do I see Cali IDs such as 43-47 at the 14h calibration interval. So in short, I think, I get the general idea, but I was confused by some details. The text and Figure/Table could also speak a bit more together to help the reader here.
P 9, L 20: At these water vapour mixing ratio levels, the instrument is measuring at its uppermost limits. So, I am not sure if the larger variability in H2O concentrations should be attributed to the calibration unit or a saturation effect within the cavity. Are the instruments used here optimized factory-wise (e.g. absorption peak scanning strategy) for high water vapour mixing ratio ranges? If yes, then it would be good to mention this in the methods section.
P 9, L24-25: I was confused here: variability in H2O concentration higher than what?
P 9, L27: The higher $\delta^{18}O$ and $\delta^{2}H$ precision at higher H2O levels is a bit surprising. I would have expected a saturation effect at some point.
P 9, L34: This is what I would have expected, maybe a slight rearrangement of paragraphs would be good here first discussion statements that are valid for both instruments and then discussing the individual instrument versions.
P 9, L37: I would make it clear that you mean the absorption peak fitting algorithm and you could add the "absorption peak scanning strategy".
P 11, L 9: Replace "checked" by "tested"
P 11, L 12: Replace "confirmed" by "showed"
P 12, L 6: Aren't the lower RMSE values for the L2130 compared to the L1102 mainly due to the higher precision of the L2130?
P 15, L 9: And in other tropical areas to make it more general?
P 15, L13: Also here I would rather use "water vapour mixing ratio level" instead of moisture (moisture is more general and could also for instance imply liquid water).

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