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Atmospheric Measurement Techniques Discussions

Interactive comment on "Airborne in situ vertical profiling of $HDO/H_2{}^{16}O$ in the subtropical troposphere during the MUSICA remote sensing validation campaign" *by* C. Dyroff et al.

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Review of manuscript AMTD 8, 121–155, 2015 "Airborne in situ vertical profiling of HDO/H216O in the subtropical troposphere during the MUSICA remote sensing validation campaign" by C. Dyroff et al.

The manuscript describes modifications made to the ISOWAT-I instrument for measurement of the H16OH, H18OH, and H16OD isotopologues during the MUSICA airplane campaign. Although the campaign aims at the validation of the satellite dD(H2O) data product through a coordination with ground based and airborne measurements, the

current manuscript focuses on the ISOWAT-II instrumental development and the data analysis and validation. A limited data set is shown and interpreted in terms of a simple Rayleigh model and air mass mixing. The manuscript is well written; the language is clear and concise. I have only a limited number of comments and questions:

In my opinion, presenting the very infrequent measurement of the airborne bubbler output as a calibration measurement is a bit of misnomer. After all, the data are not rescaled using frequent in-flight measurements of water vapor standards of different isotopic composition and different humidity levels. Rather, the in-flight measurement serves as a check on proper instrument operation and validation. The overall calibration of the data still relies heavily on the pre- and post flight calibration measurements. BTW: why is the headspace pressure of the on-board bubbler not regulated? The current configuration leads to the surprising condition of a higher humidity level of the reference at higher altitude, whereas the sampled air is normally becoming dryer with increasing altitude.

You extensively discuss sources of systematic errors, but I am still missing some:

- The large change in external pressure (450 – 1000 mbar) may very well induce mechanical distortion of the gas cell and/or displacement of optical elements. Do you observe fringe walking during (rapid) altitude changes? These may not well be accounted for by the in-flight calibration/verification measurement, as this measurement is carried out very infrequently, and most likely during level flight only.

- The air inlet appears to be different from the 2010 paper. Whereas the CARIBIC inlet collects total water, sampling both gas phase and particles, the present rear-facing inlet on the C-212 airplane is likely sampling the gas phase only. Has fractionation induced by this inlet been investigated, e.g., by flow modeling?

- Do you measure the gas temperature in the inlet line just before the MPC in order to be sure that the gas has completely thermalised?

- I presume that the measurement precision is not sufficiently high to see such effects, but have you verified that the different matrices used for in-flight validation (molecular sieve dried outside air, also depleted in CO2, but still containing Ar) and pre- and post flight calibration (synthetic air, no CO2, no Ar, possibly a different O2/N2 ratio) does not affect the results?

The fact that you make the fringe walk through temperature modulation of the bulk material of the f=1" focusing lens, suggests that the fringe is caused by internal reflection in the lens (otherwise, a simple translation of the lens would have been sufficient to reveal the fringe origin). Knowing the spacing between the lines in the spectrum (0.5 cm-1 between the H16OH and H18OH lines, over which range I count $\sim$4 fringes), I estimate the corresponding optical thickness (2L) equal to about 4 cm. That appears to be much more than the lens thickness (also after correction for n>1). Also, tilting the lens should reduce the fringe in this case. Can you clarify this?

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