Atmos. Meas. Tech. Discuss., 8, C582–C587, 2015
www.atmos-meas-tech-discuss.net/8/C582/2015/

Atmospheric Measurement Techniques Discussions
Open Access

AMTD
8, C582–C587, 2015

Interactive Comment

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.

C. Dyroff et al.

christoph.dyroff@kit.edu

Received and published: 7 April 2015

Comment: The manuscript "Airborne in situ vertical profiling of HDO/H216O in the subtropical troposphere during the MUSICA remote sensing validation campaign" present an improved version of the ISOWAT instrument for measuring water isotopologues in the troposphere. A detailed discussion of the instrument and calibration are presented as well as data from the MUSICA campaign with some interpretation of those data. Though a major objective given for the development of ISOWAT II is the validation of satellite retrievals during MUSICA of water vapor isotopologues, no data is shown on this comparison.

Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper

Reply: We did not want to show the results again. The comparison in discussed in detail in Schneider et al., 2014.

***

Comment: Minor Comments: page 125, lines 16-22: As you use the BD to account for residual water in the optical compartment, are the path lengths of the BD and the part of the SD that are in the optical compartment matched?

Reply: Yes. A sentence was added to the end of Par. 3 of Sec. 2.1.

***

Comment: Page 127, lines 18-22: What is the flush time of the detection cell? Also, have you measured the hysteresis time of residual water coming off walls in the flow system? Given the long times that you allocate for the calibration I wonder whether during ambient sampling you see a long time constant any time water vapor concentration or isotopic ratio changes?

Reply: The (theoretical) flush time of the cell is around 1.4 sec. We expect a somewhat longer time in practice due to the sticky nature of H2O. We have analyzed this (not shown in manuscript) based on the fastest changes of delta-D (and H2O) in ambient measurements, which were very well correlated with fast changes of ozone (O3) also measured in flight (also 1 Hz). In the analyzed cases, both instruments showed similar response to the fast changes in their respective target species. We therefore believe that the response was fast enough to sufficiently resolve changes in H2O and delta-D.

***

Comment: Section 2.5: I really appreciate this thorough discussion of optical affects that increase the uncertainty. I would like to see the numbers also translated into ppmv and delta-D uncertainties and compare to your overall uncertainty (presumably dominated by white noise and the uncertainty within your calibration system). These kinds of artifacts are not just constant multipliers to the data, which is typically how

AMTD

8, C582–C587, 2015

Interactive Comment

people view a statement of uncertainty such as accuracy within 5%. In these cases, the error (or bias) will change during the course of the flight as you state in section 2.5. This will not be corrected by periodic calibration and if large enough (perhaps on the order of instrument white noise) could be misinterpreted for atmospheric variability.

Reply: Sec. 2.5, Par. 2: The sentence “The fringe amplitude can be translated to an absorption signal equivalent to around 1 ppmv (strong H216O line).” was added to the end of this Par. Converting the fringe amplitude into delta-D uncertainties would generally depend on the absorption signals of both isotopologues. It also would depend on the (relative) phase of the fringe to the absorption line(s) considered. We therefore believe that the experimental determination of our instrument uncertainties is more robust and want to avoid potentially confusing theoretical examples.

***

Comment: Figure 3: I realize its hard to see based on the amount of data, but it looks like for the red points the data is biased high compared to the mean. I know you are trying to create a smooth fit through the transition region, but it seems like based on Figure 3 and 4 you would still have a jump in mixing ratio at the transition point.

Reply: In the transition between the two fit regimes we use the humidity-weighted average of the two fit results in order to avoid jumps in the final data product (end of Sec. 2.3).

***

Comment: Figure 9: This figure illustrates what I feel is the one knock on your statement that ISOWAT II has an in-flight calibration. It shows clearly that the ’calibration’ is not done over the range of measurements and in fact is done (perhaps coincidentally) at the lowest uncertainty point at least in delta-D (Figure 4). For the calibrations done at low altitudes, the ambient concentration is an order of magnitude greater and for the high altitude calibrations, the ambient concentration is an order of magnitude lower.

Reply (same as to first reviewer): In the introduction Par. 6 we have rephrased the according sentence to: “With the exception of Herman et al., 2014, these measurements were performed without what we believe is a critical component especially for a validation experiment: in-flight instrument-performance analysis by measuring a calibration-gas standard.”. Sec. 2.2 was rearranged, and it was highlighted that the in-flight measurements of the calibration gas were in fact for instrument-performance analysis. Sec. 2.2, last Par.: “calibration spectrum” was changed to “calibration-gas spectrum”. Sec. 2.4, Par. 3: First sentence was rephrased to “In addition we have used the in-flight calibration-gas measurements to verify that our uncertainty estimate was justified.”. Fig. 4: Caption rephrased to “In-flight calibration-gas measurements are depicted as the difference to the daily mean (absolute value) by the blue symbols.”. Fig. 9: Caption was rephrased to “Black symbols denote the respective quantities of the in-flight calibration-gas measurements.”.

***

Comment: Figure 11 and results section: In the instrument description you mention that the instrument also measures H218O and therefore delta-18O. However, you don’t provide any discussion of the calibration of this measurement. Perhaps there wasn’t room in the manuscript or this line did not provide useful data. However, if you do have the delta-18O, looking at the change in delta-18O compared with delta-D tells you a lot about the thermodynamics and kinetics under which ice may have condensed and fallen and I think would greatly improve your understanding of the different air-masses.

Reply: Yes indeed, the delta-18O data could provide additional information. The uncertainty of our delta-18O data was similar to that of delta-D, and thus too close to the expected natural variability. We therefore did not discuss the data in this paper.

***

Comment: page 124, line 1: Better to say “in situ AIRBORNE measurements have been performed with different...”

Reply: Done.

***

Comment: page 124, line 7: “... instrument uncertainty, ARE these measurements an adequate source...”

Reply: Done.

***

Comment: page 126, line 11: remove ’Thereby’.

Reply: Done.

***

Comment: Page 127, line 24: remove ’Thereby’.

Reply: Done.

***

Comment: page 128, line 26: affexted should be ’affected’

Reply: Done.

***

Comment: page 130, line 21: “They are differently effective” is not correct English. Try “Their importance varies ...” or “Their effect varies...”

Reply: Done.

***

Comment: page 133, line 20: remove ’was’ “The aircraft crew then waitED for ...”

Reply: Done.

AMTD
8, C582–C587, 2015

Interactive comment on Atmos. Meas. Tech. Discuss., 8, 121, 2015.