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Interactive comment on “Airborne in situ vertical profiling of HDO/H₂¹⁶O in the subtropical troposphere during the MUSICA remote sensing validation campaign” by C. Dyroff et al.

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Received and published: 10 March 2015

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.

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Overall the paper is well written and the authors give a detailed description of the improvements to the instrument including ground and flight calibrations. In the introduction, the authors present an argument that any instrument used to validate or calibrate satellite data should itself be traceable to known standards and that tractability needs to be verified in flight. I think the argument is well stated, and while the authors make an attempt to do that with this instrument, the tractability in flight needs to be over the whole range of measurements. The instrument measures over 2 orders of magnitude in water vapor mixing ratio and from -50 to -500 permil in delta-D. The in flight calibrations, however, are only done at 3000 and 5000 ppmv depending on altitude. While this is used as a check on the more extensive ground calibrations, in my opinion this does not constitute in-flight calibration of the instrument and doesn't live up to the metrics the authors present in the introduction. This is especially true as the instrument uses two different absorption lines to measure water vapor depending on the range, but the in-flight calibration only is in the range of one of those lines. I can't think of a reason why you don't vary the flow of your dry and wet source in flight as you do on the ground. Perhaps you are only using a fixed orifice to control flow in flight, but there shouldn't be a reason not to use a variable flow controller. This would allow you to calibrate over the full range in flight. Of course, this is mostly a complaint about overstating the in-flight calibration of the instrument. Overall I think the authors do an excellent job of representing the instrument and discussing issues and I appreciate the detail with which they explore the uncertainties associated with their instrument.

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?

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

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isotopic ratio changes?

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 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 mis-interpreted for atmospheric variability.

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.

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.

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.

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Minor Grammer/typo Comments: (all CAPS represent the recommended change) page 124, line 1: Better to say “in situ AIRBORNE measurements have been performed with different...”

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

page 126, line 11: remove 'Thereby'.

Page 127, line 24: remove 'Thereby'.

page 128, line 26: affexted should be 'affected'

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

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

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

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