

Interactive comment on “A framework for accurate, long-term, global and high resolution observations of tropospheric H₂O- δ D pairs – a MUSICA review” by M. Schneider et al.

Anonymous Referee #1

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The manuscript AMT-2015-330 is a review of the development of the MUSICA framework for observing H₂O- δ D pairs and a report of its current state. The authors describe very detailed their solutions of the lacks in the calibration and validation of the retrievals, partially published already in previous articles. The manuscript is within the scope of the journal AMT. A review of the evolution in this intricate research field merits publication. But, as this being a review article, I expect the content being presented in a more general context, and, maybe with less details of the own work, that has been published before.

General comments:

1. Introduction:

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A more general introduction into the research field of atmospheric water vapour isotopologues within the context of atmospheric dynamics should be added. The introduction currently reads more as an excuse for the authors work. A wider overview should introduce the reader into this topic and should set the authors work to less profile. This means also, the content of Appendix A should be shifted into the introduction, while Appendix A could be erased. There was previous work published (e.g. Worden et al., ACP, 2011). This should be cited correctly. The authors generally should set their work more into the context. Less noisy advertising would be more satisfactory for a review.

2. Remote sensing of H₂O and δ D:

The authors describe their retrievals in great detail, which is acceptable here. They quantify their obtained errors, but, a general discussion about error sources is missing. This should necessarily be added.

3. Accurate remote sensing products:

This section presents the main results of the development of MUSICA remote sensing products and claims their high quality and accuracy. For evaluating the results, correlation coefficients and slope-values should be added to the tables 2 and 3. A discussion about the limits of the described products is largely missing. Appendix C points out that coincidence of the reference measurements was limited. But the impact of short term and small scale variability of tropospheric humidity is not discussed or even quantified. To my feeling a detailed discussion about this is necessary for any evaluation based on reference data with a temporal and spatial mismatch. Quantitative analyses of tropospheric variability of water vapour have recently been published in ACP (Steinke et al., 2015 and Vogelmann et al. 2015). These studies should be considered and cited.

It is stated, that one error source is the unknown humidity above the ceiling altitude of the aircraft. For me, this seems not to be that clear. Above 7km I would expect definitely less than 5% of the total water vapour column and consequently variations being even

C2

significantly smaller which is in a certain contrast to the named errors. However, I would expect larger uncertainties caused by short term and small scale variability.

4. Validation of H₂O and delta_D pairs:

I agree with Referee #2. This section is a somewhat extensive and could be more a summary of the previous work (Wiegele 2014, Schneider 2015).

5. Consistent long-term observation with NDACC/FTIR:

The contour plots of Fig. 7 show, that there is a seasonality in H₂O-delta_D-pairs, but I would expect at least something like a long time series for different locations. Such a plot should have the time-scale on the x-axis. It might be better, to move the interpretation of measurements to section 7. In principle, I think (possibly different from Referee #2) that an extensive discussion about the interpretation of measurements should take place in a review. In particular, if defective interpretations are possible, this discussion is exactly where the authors can make an outstanding point by demonstrating the improvements obtained from their work.

6. Quasi global and high resolution observations with MetOp/IASI:

Please, make a statement about temporal coverage of IASI data, its availability and how to access IASI data. I do not agree with Referee #2, that Figs. 8 and 9 are redundant. It is of interest to see the seasonal change on the global map. But, I suggest to put Figs. 8 and 9 into one Figure with 4 panels. A discussion about the influence of small-scale variability (e.g., thermals) within a 12km large area is lacking.

7. Summary:

The authors summarize quite well their progress in H₂O-delta_D products. But, they should also point out the meaning for atmospheric research. What do we learn from the improvements in tracking water isotopologues? Maybe the (new) interpretations made in section 5 (criticized by Referee #2) are better discussed here in order to point out the gain for atmospheric research. The authors also should describe the benefit

C3

of the new quasi-global high-resolution product with more emphasis. "Conclusions" should be added to the sections' title.

Specific comments:

All contour plots: The Rayleigh-line should be plotted stronger (same thickness as black lines).

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P7 L18 - L25: Please, give an estimate of the impact of atmospheric variability to the significance of the remote sensing products. This is a major issue, just mentioning here is not enough. The discussion should take place here and not in the appendix C. Details of flight data, measurement schedules and so on, of course, should be left there.

P8 L3: ...the procedure...

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