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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Two decades of water vapor measurements with the FISH fluorescence hygrometer: a review" *by* J. Meyer et al.

Anonymous Referee #2

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Review of Meyer et al., 2015, ACPD, Two decades of FISH water vapor

This is an updated review, in a comprehensive and rigorous manner, of the performance, calibration, and measurement intercomparison of the FISH lyman-alpha fluorescence hygrometer. FISH is one of the world's leading water vapor measurements for the upper troposphere and lower stratosphere, and a thorough description of the calibration methods and update of "lessons learned" is well-suited for the community. Of further significance, this manuscript describes an update to FISH results at non-typical atmospheric conditions of AQUAVIT. For these reasons, I recommend publication in ACP. The authors may want to consider the following comments in preparing the final version/revision, most to encourage some further thought on their calibrations/measurements in the future.

My only real technical recommendation is for the authors to include an Allan deviation plot for a representative amount of UT/LS water vapor. The importance of this plot is that it shows to what extent and at what time scales does long term drift dominate over white noise properties. The longest timeseries of data shown in the manuscript at constant concentrations are on the order of an hour. Since most aircraft flights are probably 8 hours long, it would be helpful to see the performance of the measurement at these timescales. Because Allan plots are usually analyzed to about 1/10th of the duration of the actual timeseries, this would mean that a constant flow up to 80 hours would in theory be needed. Have the authors ever ran the system overnight or through the day at a constant concentration – and could these data be analyzed in an Allan plot? I'm not sure going out to Allan averaging times of 8 hours is fully necessary, but it would be helpful to measure out to an hour or two (i.e. day long time series or thereabout). Given the excellent flight-to-flight and campaign-to-campaign reproducibility, I don't anticipate any problems. In fact, I suspect any "drift" would be more related to the peculiarities of the water vapor dilution system than their measurement – but again, this would be a helpful piece of information for readers. Overall, I suspect the Allan plot will add one more piece of evidence to suggest the excellent stability of their system. See a recent paper by the late Peter Werle in APB vol 102, p313 (2011).

Minor typos/clarifications:

Abstract, line 2, use "measurements"

Abstract, line 5, replace "since" with "for"

p. 7741, first paragraph: a) how 'constant' is 'constant' – are you doing this with a flow meter? Critical orifice? Adding a sentence or two would be helpful, even if summarizing the earlier work. b) maybe use "flow ratio" or even just "ratio" instead of "mixing" ratio to avoid ambiguity with mixing ratios of water vapor

p. 7741, second paragraph: a) "the number. . ..has to be taken into account. . ." (instead of "have"); b) "measurement cycle" instead of "measuring cycle"

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p. 7744, paragraph starting line 14: How was 10 sLpm chosen? This seems a bit arbitrary. How much better is 10 sLpm versus 5 sLpm? Would it improve even more with 20 sLpm? Or is 10 sLpm chosen because that is consistent with the airborne system in-flight? Maybe add the in-flight flow rate in Section 2. p. 7744, same paragraph: awkward grammar, please revise: "...the effect can be accounted for including an additional calibration factor". Possibility: "...the effect can be addressed by including an ..."

p. 7747, line 5: "thus the data point will be ignored in the further discussion."

p. 7748, line 7: typo, "input"

p. 7748, lines 16-20: Why not use Murphy and Koop in the future, instead of using another formulation and then referring to its agreement with the Murphy and Koop parameterization for the ice vapor pressure?

p. 7748, line 13: depicts

p. 7750, line $\sim$ 7: second "term" instead of addend?

p. 7750, line 14: What is 6-10% accuracy before 2007 and 2001? Why not just state 6-10% accuracy before 2007?

p. 7754, intercomparisons in MACPEX: This is the only somewhat troubling aspect of the manuscript (the fact that in-flight intercomparisons still don't agree with one another, though improved), but I don't think much can be done about this except to quantify the agreement with other in-flight measurements. To this end, what was the agreement between NASA DLH and FISH? Please list.

p. 7756, near top: I agree that measurements below 1 ppmv in AIDA are not representative of the atmosphere (high pressure, low mixing ratio). However, I do think measurements at these levels provide some indication of a "zero". The fact that so many instruments disagree in this range is troubling, and it is this reviewer's opinion that many of the discrepancies between instruments may be related to not knowing

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the “zero” of each instrument. This is very challenging due to outgassing effects, etc., as the authors know. But I think more attention should be paid to the zero problem in future work. Yes, 0.5 ppmv will not be observed in the atmosphere – but a measurement of 3 ppmv is relevant, and not knowing a zero complicates such a measurement. Perhaps the authors can elaborate on this need in the summary section (and any other improvements that could help the calibration system – e.g. is it possible to add a standard addition of a known H2O flow to the inlet while in-flight?).

Section 5.3 – MLS/FISH intercomparisons: As the authors know, comparing a point, in-situ measurement with the volume of a satellite - taken at different times no less – is complicated (see Diao et al., JGR, 118, 6186, 2013). I’m not sure the (dis)agreement between campaigns really means much in either direction, given the spatiotemporal mismatch variability. Can the authors list the number of points for each campaign and mean time/space deviation for each campaign – perhaps the discrepancies are related to larger mismatches?

Section 6, Summary: Instead of just summarizing the key points, what about some forward looking statements on how to further build confidence in the measurement? FISH looks great but perhaps still has a bit of a dry bias compared to other instruments. It may be because the other instruments aren’t as rigorously calibrated and may be off themselves – but what further experiments could be done to build even more confidence in the FISH results? What about calibrating under representative UT/LS temperatures as well as pressures and mixing ratios? What is the temperature-dependence of the sensor (whether spectroscopic or electronic components)? Clearly, aircraft cabin temperatures change from the lower troposphere to lower stratosphere – could variations in these aspects be causing some discrepancies between on-ground calibrations and in-flight? Probably not much given the results presented in this manuscript but something to consider when trying to resolve the improved (but still nagging) discrepancies between instruments.

Overall, despite some nitpicks above that should be considered – either here or in fu-

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ture revisions of the calibration system/instrument – this is an excellent manuscript and sets a high bar for newly-developed (and existing) water vapor measurement systems. The work will be extremely valuable to the community, and the authors are commended for presenting such an in-depth and even-keeled analyses of FISH.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 7735, 2015.