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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.

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The authors thank Anonymous Referee #2 for the detailed review of the FISH paper and for the many fruitful comments which were helpful to improve the paper. All changes of the paper are highlighted in red color. Point by point answers to your comments are reported below.

Specific Comments/Questions

- *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).
This is indeed a very good point. We also thought about an Allan deviation analyses, but we haven't done it so far. Unfortunately, we have no run of FISH at a constant water vapor level for more than three hours and thus cannot include an Allan plot in this study. It would also be very difficult with our calibration bench to provide a high flow of a constant water vapor mixing ratio over such a long time. Another point is that our reference hygrometer (DP30) could show small deviations on such a long time which would also be adverse for an Allen plot. Our future plan is to go with FISH to the national primary standard where constant water vapor mixing ratios are guaranteed. With this setup we can than make an Allan plot on a more reliable basis.

Minor typos/clarifications:

- *Abstract, line 2, use "measurements"*
  Is changed.

- *Abstract, line 5, replace "since" with "for"*
  We revised the first sentence in the abstract.

- *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*
  We use a flow controller to maintain the constant flow of 2nml/min +- 0.05 nml/min of the ArH mixture. The argon and hydrogen is already mixed in a gas-bottle. We included the the numbers in the text, but we don't want to put too many details in the text to keep it clear. We changed the "mixing ratio" to "ratio" to avoid any ambiguity as you suggested. (see page 7 l 110)

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

- *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.*
  The 10 sLpm where chosen because we have similar flow rates during aircraft operation in the UT/LS. The second point is that at 10 slm the "normal" calibration (without considering the outgassing) results in the same calibration factors as considering the outgassing effect. This is caused by the minor fraction of water from outgassing compared to the total flow through the cell. We added the in-flight flow rate in Sec. 2. (see page 6 l 93)

- *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 : : :"*
  Is changed.

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

- *p. 7748, line 7: typo, "input"*
  Is changed.

- *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?*
  You are right, the best parameterization for ice vapor pressure is Murphy and Koop. The first point for using the Sonntag eq. is that the DP30 frost point mirror uses the Sonntag eq. internally for calculating the water vapor concentration. The second point is that the deviation of Murphy and Koop (-1.33% at the 3 ppmv level) and Sonntag (-1.44%) are quite similar and therefore wouldn't not improve it significantly. For that reason, we would like to leave it as it is.

- *p. 7748, line 13: depicts*
  Changed to: "illustrates"

- *p. 7750, line 7: second "term" instead of addend?*
  Changed.

- *p. 7750, line 14: What is 6-10% accuracy before 2007 and 2001? Why not just state 6-10% accuracy before 2007?*
  We changed parts in the FISH and in the calibration bench in 2007 and 2001. But you are right, it is more clear to state only 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.*
  We agree with your point and stated also the agreement of DLH and FISH. (see page 20 l 459)

- *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 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?).*
  This is a very important point. With our new calibration equation we tried to quantify our "zero" in a better way. But for sure this is a difficult task, because outgassing and other effects are pressure dependent and maybe hard to scale from the AIDA conditions (0.5 ppmv, high pressure) to UT/LS conditions 3ppmv at low pressures. We added this point to our summary and state that in future we will try to better quantify our zero. We thought already about an in-flight calibration for the FISH, but discarded it due to the need of a high flow through the FISH which is hard to provide aboard an aircraft.

- *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?*
  The average distance values for the profile matches are 615 km Troccinox (25 profiles), 717 km for Scout (23 profiles), 517 km AMMA (16 profiles), 372 km Reconcile (29 profiles), and 609 km for MACPEX (5 profiles). Figure 13 shows all the points for each mission, and as we note, there is no significant difference between the FISH and MLS measurements for all of the missions except Reconcile. Actually Reconcile has the closest average matches in distance, and the largest number of matching profiles, but has the largest average difference. We state in the text the matching criteria (within 12 hours, 5 deg latitude and 2 degree longitude). In addition, to provide more information we change the figure caption for Figure 13 to: Comparison of FISH with MLS (Microwave Limb Sounder on the Aura satellite) for different aircraft campaigns. Mean deviation with respective SD for each campaign is given in the upper right. There are 25 matches for Troccinex, 23 for Scout, 16 for Amma, 29 for Reconcile and 5 for MACPEX, and matches are within 12 hours, 5 degrees of latitude and 2 degrees of longitude.

- *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 ra-*

tios? 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.

We have already done a lot to characterize the FISH and so far unknown effects. We already calibrated FISH, for instance, in a pressure chamber to see if we have any so far unknown pressure dependencies of the sensor or the electronic system. But this is fortunately not the case and water vapor mixing ratio stays very constant even if we changed the outer pressure from 800 hPa to 100 hPa and back. As we already stated to your first point, we would do an Allan deviation analysis in future and try to quantify better our zero measurement. This two points are stated now in the conclusion. (see page 24 l 591-597)

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

  Thank you.