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Atmospheric Measurement Techniques
Discussions

Interactive comment on "Intercomparison of in situ water vapor balloon-borne measurements from Pico-SDLA H₂O and FLASH-B in the tropical UTLS" *by* M. Ghysels et al.

Anonymous Referee #3

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The study presented in the manuscript deals with comparisons of in situ observations of water vapor in the tropical UTLS by two balloon-borne instruments using very different techniques. Water vapor is known to be of very high importance in the radiative balance of the atmosphere and in stratospheric ozone loss processes. This is however tricky to measure with great precision/accuracy and intercomparisons within the atmosphere are highly supported and encouraged by the international scientific community to properly derive biases of instruments operated to study vertical profiles or trends (see SPARC Water Vapour Assessment, Kley et al. 2000). They are particularly missing in the tropical regions. The manuscript is well-written, both concise and well-detailed with appropriate associated references. A nice effort has been done to

convincingly describe the instruments and a relevant discussion about the consistency of the results obtained is provided. For all these reasons, I would recommend the publication of this paper in AMT once the comments given below have been clarified. In particular, caution must be put within the text about the description of the water vapour vertical structures with respect to altitude because there is some confusion about balloon ascent/descent attribution of these structures (figures 4 and 5). This will be, I think, easily solved by the authors.

Comments:

P13698 lines 18-26: The examples about comparisons between various hygrometers should provide some altitude ranges (or at least clarify if observations are compared in the upper troposphere or lower stratosphere) because the differences are dependent on the considered levels and are most of the time stronger at lower levels (i.e. closer to the tropopause) in the given references (Vömel et al. 2007 and Jensen et al. 2008), especially for aircraft measurements.

P13698 line 20: NOAA/CSD and not CFD.

P13698 line 1: I would suppress the "thoroughly" statement.

P13698 lines 16-17: you only mention here small stratospheric balloons but what about rubber weather balloons (mentioned P13697 line 16)? Were they used to launch a 9 kg instrument? If not why do you cite this specific balloon in the introduction?

P13698 line 22: What do you mean by "tuning the laser current at fixed temperature"? Is there a temperature control of the diode? Please give short explanation at this stage of the description.

P13699 lines 15-20: you do not clearly mention how possible pressure effects on the line shape broadening are taken into account.

P13699 line 20: here you give an example of atmospheric spectra but are they removed from baseline structures mentioned in P13700 line 14? I guess yes because you overplot the fit to the measurement in figure 2. However you seem to distinguish between the term “atmospheric spectra” and “absorption spectra” (P13700 line 14) but the fact that you mention “Atmospheric spectra” in Figure 2‘s caption is somewhat confusing.

P13699 line 29: fitted instead of fit.

P13701 line 2: remove “temperature” in “pressure sensor temperature” because the sentence is confusing.

P13701 line 20: I have found the subsection numbering somewhat weird. There is a long text describing the Pico-SDLA instrument associated with number 2.1 and subsection 2.1.1 appears 3 pages after. Why not providing a 2.1.1 subsection named as something like “Description” on P13698?

P13702 line 17: for which altitude range?

P13702 line 22: remove the brackets for the cited references

P13702 line 25: Because the time is important factor towards temperature biases you should provide somewhere in this subsection the UTC time of Pico-SDLA and RS-92 comparisons. This information is really missing.

P13703 line 3: please provide the number of radiosonde launches used for your statistics.

P13703 lines 3-19: this discussion about the statistical significance of the various radiosonde data obtained throughout the campaign is interesting but I am wondering whether it is useful for the scope of the paper dealing with comparisons of two independent water vapour profiles.

P13704 line 5: this contamination in the descent phase from balloon outgassing 3 km below float altitude may be surprising. Could it be due to (small) altitude excursions or rebounds of the balloon making the instrument cross the balloon’s wake?

P13704 line 18: you may choose between 300 or 400 hPa for the lower altitude limit.

I would write “lower than about 400 hPa”. P13704 line 18: I do not understand well why the instrument can be used only at night. Is it because the analyzed air volume can be affected by scattered sunlight from below (where there is strong lyman-alpha absorption) as a result of the open optical layout?

P13705 line 13: when you say “above 70 hPa” you mean in term of altitude (i.e. the 70 hPa level) and not 70 hPa in term of pressure value?

P13705 line 17: what do you call “undisturbed air”? No wind velocity gradients or no turbulence, no rebounds of the balloon? Also, the effect of water carried on the instrument surfaces has no effect during the descent?

P13706 line 17: “mass” instead of “mas”.

P13706 line 19: remove “the” in “the Flash-B”.

P13706 lines 12-22: Please add some references (if any) for the other instruments: Pico-SDLA CH_4, COBALD, LOAC

P13708 line 7 onwards: please provide pressure level values associated with altitudes for consistency with P13705 line 13.

P13708 lines 25-26: remove “especially for water vapor” due to redundancy.

P13708 line 26: you speak about the importance of knowing the CPT during the slow ascent but the CPT levels represented on figure 4 result from the balloon descents. This is not quite consistent but may be a simple mistake.

P13709 lines 11-12: There is some confusion here. Well, the structure at 17.2 km is much less pronounced in the Flash-B descent profile and might be attributed to noise. I suggest at least to mention the fact the weaker amplitude of this peak in Flash-B data. Line 12 the sentence contradicts the previous one when you state that no structure at 17.2 km is visible during the descent. If we consider this detection as realistic the structure is visible and wider (in term of vertical extent) in the ascent profile. Please clarify.

P13709 line 15: I do not see any structure at 18 km in the Flash-B ascent profile. Here, the structure is visible (though difficult to spot on the overlapping profiles of figure 4) and shifted in the descent profile. Please clarify.

P13709 lines 16-17: “Because. . .visible.” Both sentences are confusing. Do you refer to the ascent or the descent profile of Flash-B? We already know that the ascent profile has to be cut unless you speak about the descent profile of Flash-B. In this case the reader can be surprised that you discuss about outgassing effects at this stage of the manuscript while this should discussed earlier in the text on P13708. How can we be sure that the differences between both instrument descent profiles can be attributed to small outgassing effects rather than to precision or accuracy issues or geographical/time shift?

P13709 line 20: I may have missed something but once again, there is some confusion between ascent and descent for the Flash-B profile. The ascent is truncated at $\sim$18 km on figure 4. Maybe the difference value provided by the authors (0.13 ppmv) corresponds to a non-truncated Flash-B ascent profile but they should provide the difference value for the 15-18 km range.

P13709 lines 20 onwards: this part of the manuscript is essential and is the core of the study. That is why I suggest to make a Table summarizing the results for the different altitude ranges detailed here and for both flights. As mentioned by the authors the TTL is typically characterized by fluctuating water vapour amounts influenced by convection. This does seem to be dramatically the case here (below CPT) but this is tempting (and perhaps preferable) to specify somewhere in this paragraph that the best conditions to estimate the differences between both instruments is above TTL, i.e. above 19 km. I suggest to provide also the difference value for stratospheric conditions only.

P13710 lines 19-25: This is an important issue when comparing high resolution in situ data. I am coming back here to the February comparisons. Did you investigate the shift

induced by the different types of altitude information provided by the GPS and RS-92 for the 10-11 February flights? You should have to face the same problem and apply the adequate correction to get the same vertical axis for the Pico SDLA and Flash-B profiles. Please add this information (or specify in P13708 that the correction has been applied and no shift remains, as you seem to conclude on P13711 line 4, for the February flights). Is it possible that position differences of the structures in figure 4 are partly due to GPS lags?

P13711 line 12: I do not see identical features at 17.4 km in figure 6.

P13713 line 19: Same comment for the structure at 17.4 km which is not that clear.

P13722 Figure 2: The red and black lines are difficultly distinguished. Please try to better highlight the fit vs the recorded spectra. Also I guess you mean “descent of the balloon” rather than “descent of the flight”.

Interactive comment on Atmos. Meas. Tech. Discuss., 8, 13693, 2015.