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Atmospheric Measurement Techniques
Discussions

Interactive comment on "A lightweight balloon-borne mid-infrared hygrometer to probe the middle atmosphere: Pico-Light H_2O. Comparison with Aura-MLS v4 and v5 satellite measurements" *by* Mélanie Ghysels et al.

Anonymous Referee #3

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This manuscript describes a new development based on the established Pico-SDLA laser hygrometer. The authors describe some of the significant change from the original instrument, and provide a few validating measurements based on Aura MLS satellite and radiosonde measurements.

The manuscript is an important contribution describing the next development in the line of these laser hygrometers. However, it has weaknesses that need to be addressed before publication. In particular, the description of the measurement, processing, and their associated uncertainties lacks a lot of detail. In addition, the manuscript will re-

quire significant language editing. I highlight my detailed concerns below and point out language issues, where they negatively affect the understanding.

I would recommend publication only after some major revisions.

Major comments

The validation observations, in particular the stratospheric validation does require some improvement and more instrumental discussion. Using Aura MLS is appropriate, but having just two profiles is not sufficient to provide a strong quantitative validation. Although the precision of the measurements is significantly low, atmospheric variability in the comparison of remote and in situ observation is the dominating source of uncertainty here and limits what can be deduced. There is a reasonable agreement with MLS roughly in the range of 100 hPa – 50 hPa and a significant wet bias at lower pressures. The upper tropospheric dry bias of MLS is still visible and limits what can be said about Pico-Light. Without more soundings, which apparently are scheduled, this cannot be remedied. The manuscript would gain tremendously, if the comparisons that are planned in a few months could be included here. However, even though the benefit seems obvious, I do not this it is required to include additional sounding data. What is essential is a more detailed discussion of the error sources and their vertical dependencies. I give more detailed suggestions below.

The discussion of the atmospheric filament is of marginal interest here and does not really contribute to a better understanding of the instrument. I find it a little distracting and would propose to move it from this to a different manuscript, which can focus more on the atmospheric aspects of that observation.

The discussion comparing versions 4 and 5 of MLS water vapor is also a little distracting. Pico-Light cannot contribute to evaluate which version is better. All that can be said is that the two versions agree reasonably well with the Pico-Light measurements. I would propose reducing this discussion in favor of an expanded instrumental discussion.

Detailed comments

Lines 17-18: I believe the authors refer to the difference of the two versions provided by the JPL MLS team. I do not believe that they mean that the authors applied a dry bias correction for their manuscript as this sentence implies. I would suggest just deleting it.

Line 38: A better reference for the proposed radiative feedback of stratospheric water vapor on surface temperatures is Forster and Shine (GRL 1999).

Lines 56 to 60: These two sentences require references. Which HALOE observations indicate that the Boulder trend may be an overestimate? Which study suggests that ECMWF derived tropopause temperature trends are inconsistent with stratospheric water vapor trends from the NOAA observations?

Lines 71-72: Please give references and describe where these differences have been observed to provide proper context.

Line 73: The stratospheric water vapor trend to date does not remain "undetermined". Quite the opposite. Much is known about the stratospheric water vapor trend from a multitude of instruments. However, there is still some uncertainty to the exact magnitude of the trend. It is this level of uncertainty that drives the requirements of instrumental uncertainty. The authors should spend more text here to describe where observational and analytical limits are and what these imply for the requirements on their instrument.

Lines 115 ff: The authors state here that no higher order line shape effects were observed for stratospheric pressure and state that this is their region of interest. However, there is a significant amount of discussion of lower to upper tropospheric measurements. Therefore, a discussion of higher line shape effects seems necessary.

Line 121: Do the authors mean "burst altitude"? Or is the balloon indeed floating for some time?

Lines 140ff: It is not clear to me, what the ramp or reference signal is that is subtracted from the measured spectrum. A figure could illustrate what is meant if this is done in real time. Or is there a laboratory calibration step involved to obtain the gain? Overall, I am missing a more detailed explanation of how the measurements are processed and a final mixing ratio is calculated.

Lines 149ff: One single spectrum takes 20 ms. Does that mean that there are 50 spectra measured per second potentially allowing that temporal resolution? Or is there an averaging scheme involved, that also includes the ambient pressure and temperature measurements? I would like to see, how sensitive the spectral analysis is on temperature and pressure. Would it be necessary to measure pressure and temperature and the same higher frequency to improve the temporal resolution or would it be sufficient to interpolate pressure and temperature onto the higher resolution spectral measurements?

Lines 151 ff: I assume that measurements are taken on parachute descent. Depending on the parachute size the fall velocity will vary strongly with density, most likely more than a factor 2.5. Please state what size parachute you used and show a fall velocity profile, which combined with a proper temporal resolution will give the vertical resolution.

Lines 162 ff: What temperature corrections exactly are described here? How do they contribute to the measurement uncertainty?

Line 178: I assume the authors mean that "no remote control" is necessary, which implies that the system relies on one-way telemetry only.

Could you explain whether all data needed to generate a profile are sent by telemetry or whether instrument recovery is needed to obtain high rate data?

Line 192: Is the Imet-4 just flown piggyback to get a pressure, temperature, and humidity profile from this radiosonde, or is it used to send instrument data to the ground?

Later in the manuscript you mention that a Sippican Mark-II sonde was flown as well. Was that one maybe used as radiosonde or data carrier? Can you better describe the setup of the payload?

Line 193: It would be better to give the absolute precision of the Honeywell pressure sensor. The precision is normally provided relative to the full measurement range, which was not provided. Therefore, 0.03% is not very meaningful.

Line 206: The response time of the Honeywell pressure sensor is much faster than 1 s. Maybe the authors mean sampling time? Or is there a more elaborate measurement scheme used?

Lines 206ff: Could you be more specific on the averaging? In the previous section you mentioned that the spectra a measured only for 200 ms, followed by temperature and pressure measurement and processing. If that is the case, then the uncertainty over 200 ms should be the same as over 1 s.

Lines 210ff: Please expand the discussion of the uncertainties and provide a table. You state that this estimate applies for the UTLS. What are the uncertainties above and below the UTLS? Since you show the data and the capabilities of the instrument up to 20 hPa, it is important to understand the uncertainties over the entire range.

Table 2: The precision of the measurements is not sufficient to evaluate the difference between the MLS and Pico-Light. For Pico-Light, an estimate of the possible systematic errors is important. Since there are only two profiles and no averaging can be done, the random uncertainty may be dominating, but this is not at all clear. Although the authors do not mention it, I suspect that there may be a calibration somewhere in the operation of their instrument. If not, this could be pointed out.

Line 249: See above. A parachute descent rate depends on altitude.

Lines 262: How does MODIS contribute to the analysis? This requires more explanation about the product that is used and how it is used.

Section 6.1: I am a little concerned about the temperature profiles. Are they measured by the Imet-4 temperature sensor? Are they measured on ascent or descent? Radiosondes are designed to measure during ascent and descent measurements may have reduced reliability. Or was a different sensor used, e.g. the Sippican thermistors? If so, which, how where they exposed and coated? Was there a proper radiation correction applied?

Lines 296ff: The comparison between Cosmic and the Sippican Mark-II sondes is only relevant, if these sondes were flown exactly the same as operational sondes. If they were flown piggyback as part of the larger payload with a shorter unwinder, and possibly measuring on descent, then this comparison does not apply and more discussion of exposure, ventilation, and processing is needed.

This entire paragraph does not appear to apply to the payloads that were flown and should be deleted.

Lines 322: Please explain first what the MLS quality criteria are, where they come from and what they mean, before stating the thresholds that were used.

Lines 329: What pressure is meant? 215 hPa or 21 hPa? Or something else?

Line 343 and other places in the manuscript: When referring to “levels” it would be good to specify either “pressures” or “altitude levels”. For example, here, it is not clear whether you mean “for altitudes below 100 hPa” or “for pressures below 100 hPa”.

Line 347: I believe Sunilkumar used the CFH, not the NOAA frostpoint hygrometer.

Lines 354f: It is hard to see the exact bias of Pico-Light in the stratosphere, but it seems to be in the range of 20 % above MLS. This is larger than the wet bias of MLS against the frostpoint instruments or the Lyman alpha instruments in that altitude range.

Line 375: delete “globally”

Lines 381 ff: This statement is not correct. The quality of radiosonde humidity measurements depends on the manufacturer and no blanket statement is possible. The Vaisala sondes are generally much better than the other manufacturers. The Imet-4 is not well validated.

I don't think there is any value including the Vaisala data here for the reason the authors provide. The collocation issue is so large that no real conclusion can be drawn. I would remove mention of that sonde.

Line 400: Something is missing in this line.

Line 430 and 433: Again: Do you mean the balloon floats before bursting?

Line 432: Replace "pollution" with "contamination"

Lines 430ff: In the conclusions, please provide an overarching discussion of the uncertainties and how they compare with the current state of technology and measurement requirements.

The reference to Jensen et al. is not a valid link. A better reference for the comparison of MLS and NOAA frostpoint is Hurst et al. (2016).

Figure 3: Are the spectra shown single spectra, or have they been averaged over a period?

Figure 5: the right panel is the same as in Figure 1. Please combine these panels?

Figure 5: Open path laser hygrometers often suffer from unwanted absorption between the laser and the exit window or fiber coupling. The same issue may be the case here. You have a Styrofoam insulation box, which most likely generates relative large water vapor concentrations inside. It is not clear where the laser light exits the laser, but I would assume that it may have to cross a window and thereby the large water vapor concentration inside the box. Is this considered and if so, how? Please spend some text describing this potential issue.

Figure 8: The legend interferes with the data. Here, as in Figure 9, I cannot make out

the black open squares.

Figure 9: Why is there a data gap just above the 200 hPa altitude? This gap also shows signs of a 5 point running mean. Please explain why this running average is needed and how it contributes to the uncertainty/vertical resolution.

Figure 10: Please remove the Vaisala RS41 data. They are more confusing than they help. Please use different symbols for the Pico-Light and Imet humidity data such that the vertical coverage of each can be seen.

Figure 11: This Figure shows two regions shaded as filament and profiles from two very different days. The upper left axis also refers to ozone, which is not shown and not mentioned in the manuscript. Highlighting the region of a possible filament has no meaning here, since the two profiles are from very different seasons. I would suggest removing Figures 11-13 from this manuscript and to expand that discussion in a separate manuscript with more care and detail.

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