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Atmospheric Measurement Techniques
Discussions

Interactive comment on "Accuracy assessment of water vapour measurements from in-situ and remote sensing techniques during the DEMEVAP 2011 campaign at OHP" *by* O. Bock et al.

Anonymous Referee #1

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The manuscript presents results from a field campaign which took place in Fall 2011.The campaign involved multiple instruments (2 lidars, 5 GPS, 3 radiosonde types, etc.) using multiple techniques (lidar, capacity sensors, frost-point, GPS, spectrometers). The most significant outcomes are on the behavior of the new radiosonde M10, the Snow-White FP hygrometer, and the range/accuracy of the IGN lidar. Unfortunately, very little could be learned from the low-elevation sensing capability of this scanning lidar (technical issues), from the OHP WV lidar (technical failure), and from the spectrometers (large biases). These results should be published after minor revisions. The list of suggested minor revisions is provided below.

Page 3453, line 3:

Clarify sentence starting with "However, this effect has usually…" Specify the effects, and be more specific on the "usually".

Page 3457, line 20 and any alter occurrence thereafter:

To avoid confusion or misinterpretation, I suggest to use the syntax "%RH" (respectively "%") throughout the manuscript when discussing quantitative RH absolute (respectively relative to measured value) differences.

Page 3459, line 15:

It would be more appropriate to use the wording "filtering increases" or "resolution degrades" instead of "resolution increases". The latter expression is subject to be misinterpreted into "resolution gets better".

Page 3460, lines 10-11, "because no upper air temperature…":

I do not think this is a legitimate argument. The temperature dependence of the cross-section is only slowly varying with temperature. If a temperature correction is needed throughout the troposphere due to a decrease in temperature of roughly 100 K between bottom and top, a nearby radiosonde profile, ECMWF profile, or even empirical climatology (may be seasonally varying) is enough for a first order correction. Please modify this sentence to specify that there are options available, but the ultimate choice is to not correct, which automatically introduces a "baseline" accuracy of the OHP lidar measurement of 2% (per [Sherlock, 1999b] estimates).

Page 3460, lines 19-20, "it is standard to adjust…":

I do not think it is "standard" to do such adjustment. The way most existing instruments are calibrated today is to normalize (typically LS fitting) the ratio of the H2O to N2 channels to the a priori source. Therefore there is no use of intermediary constant f, and no interplay between the Ca priori and f as described in the manuscript. The adjustment procedure as described is subject to potentially large errors because one rarely knows where the fluctuations in the overall calibration constant come from: is it from an a priori measurement issue? Is it fluctuations in the aerosol extinction? Is it a fluctuation in the lidar receiver transmission? Quantitatively, it is very difficult to differentiate between each of these uncertainty sources, so I do not see where we have something to gain by “adjusting”. Please clarify.

Page 3461, lines 5-6: “each slant lidar profile (integrated 5 min). . .”:

It is a shame that the lidar slant path integration time and GPS averaging time do not coincide (5 min vs. 10 min). Maybe there are strong measurement constraints on both sides (lidar and GPS) that keep this schedule quite rigid, but I think in occasions such as a large field campaign, measurement planning should be done to optimize things like coincidence times. One can reply that the time difference do not affect much the measurement, however that would go against other rationales such as to save data points every 30 s or 1 min to potentially capture very short atmospheric fluctuations. Maybe this can be used as a learning lesson that prior brainstorming on measurement scheduling might be a good idea for future campaign(s).

Page 3468, lines 23-27„ starting with “NDACC-OHP lidar measurements. . .”:

In order to fully understand the differences between OHP/NDACC lidar and the RS and IGN lidar, the OHP data should be averaged over the same time window. It looks like they have measurements for over 2 hours, so it should be possible to analyze their data for the time window that best coincides with the other measurements. No conclusion can really be drawn if only one, non-coincident profile is used for the comparisons.

Page 3469, lines 3-5, “the comparison with M2K2DC. . .”:

I find this paragraph quite confusing. Is it only the M2K2DC showing a large moist bias. What about the IGN lidar in the 3 km dry layer? Please reword the paragraph, maybe highlighting the moist bias in the upper troposphere, but also being more specific about

the differences seen at 3 km.

Page 3469, lines 11-15, “This is one would expect since SOPHIE. . .”:

Again I think this paragraph needs some clarification, especially on the actual effect of the two different RP1 and RP2 cal methods (how different they are and why do they yield these differences).

Page 3472, lines 15-19, “Compared to the SW and Raman lidar. . .”:

Are there any attempted explanations in Yoneyama et al., 2008? If the answer is yes, please report them in the present manuscript.

Figure 15:

The caption mentions “a 3 kg.m-2 bias has been removed from SAOZ and SOPHIE in this figure”. I am not comfortable with the way these measurements are presented:

1) If there is such a bias, then the non-corrected measurements should be plotted to highlight the bias. This is the very purpose of validation papers. Readers must be clearly aware of the bias.

2) On the other hand, the corrected measurements may be plotted, but in this case the legend must clearly mention “corrected SAOZ” and “corrected SOPHIE”, and the caption must refer to a discussion in the main text on this bias and its correction

Figure 16:

It is not clear whether the corrected or uncorrected SAOZ and SOPHIE measurements are used. Just like in my previous point, this should be clearly explained, and it would be preferable to show the non-corrected dataset, or whichever dataset is publicly available, together with the proper references.

Interactive comment on Atmos. Meas. Tech. Discuss., 6, 3439, 2013.