Atmos. Meas. Tech. Discuss., 8, C5075–C5082, 2016
www.atmos-meas-tech-discuss.net/8/C5075/2016/

Atmospheric Measurement Techniques Discussions

Interactive comment on "Relevance of a kite-based calibration for a water vapour Raman lidar" *by* J. Totems and P. Chazette

J. Totems and P. Chazette

julien.totems@lsce.ipsl.fr

Received and published: 22 January 2016

We thank the reviewer for these detailed and helpful comments. Our responses below :

MAJOR POINTS: 1) Introduction: add more context on the significance of water vapour and describe better the Hymex and Charmex campaigns. The text added after the first review seems insufficient to clarify the context to a reader. The advice of the previous review has been followed only partially.

We now detail the rationale and method behind Hymex and Charmex in the third paragraph of the introduction: "HyMeX is dedicated to the study of the water cycle, and the role of water vapour and aerosols in cloud formation leading specifically to the intense

precipitation in the western basin during the fall season, using multiple instruments in synergy and coupling their measurements to meso-scale models. Its overarching goal is to improve forecasts of extreme precipitation events on the orography of the western basin. " "ChArMEx aims to assess the present and future state of the atmosphere (gaseous reactive species and aerosol particles) in the Mediterranean basin, while its ADRIMed segment focuses on how the Mediterranean climate is impacted by direct aerosol forcing and may evolve in the next 50 years due to climate change and growing anthropic pressure. It involved ground and airborne instruments to study among others water vapour and aerosol coupling during summertime dust and pollution events in the western basin." We hope this is sufficient, as we feel the introduction is now becoming too dense to adequately lead the reader to the question at hand, which is rather technical than scientific. This is also why after trying to rewrite the first paragraph on the significance of water vapor, we thought that the previous version was better because it was more synthetic, and kept all the relevant information. We then just added a couple sentences and references for missing ideas. If this is not acceptable, we would ask the reviewer more specifically what is missing for the reader to better understand the importance of water vapour.

2) P. 10587, L. 9. Please clarify in the article text if the uncertainty is instrumental (e.g. computed from propagation of measurement uncertainties) or an atmospheric variability during the 40 min of the sampling (e.g. a standard deviation). Actually, both contributions are relevant to this comparison, and they could both be plotted.

It is indeed the instrumental uncertainty deduced from the standard deviations of the short term standard deviation signals over just a few profiles as detailed in the given reference Chazette et al 2014. This has been added in the text. The problem to extract the atmospheric variability, which has been tried, is that signal-to-noise ratio seems too low to see a substantial/reliable excess of std over the 40 minutes, which would correspond to what we are looking for.

3) P. 10587,L. 19-20. It would be worth trying to use the instantaneous data. For

example, shorten the lidar integration time (e.g. 5 min) and use, for each altitude, the measurement corresponding to the time for the radiosonde at that altitude. This could give insight on temporal variability (i.e. averaging over 40 min might not be the best way to do this).

Same as just above, the signal-to-noise ratio is unfortunately too low over 5 minutes to be exploitable. Our minimum integration time for water vapour is 15 to 30 minutes.

4) P. 10589, L. 6. This excess RMSE is about 1-1.5 g/kg as per figure. This is comparable to the stated uncertainties. Please state this and comment. Also, in Figure 5, add the lines of RMSE +/- the measurement error (as per comment below, RMSE is a difference, not an error).

It is indeed interesting and it would have been a valuable addition. However, we do not know the measurement errors due to the sondes used at Palma, and more importantly, following the comment of reviewer #1 who deemed this part was not informative, we deleted it.

5) The conclusions are a little weak. They need to be strengthened (as already highlighted in the previous review).

The conclusions were indeed already expanded in the previous version. In the absence of a specific suggestion on what to develop, we added a paragraph on the results obtained by the calibrated WALI and its scientific prospects: “This calibration method may be leveraged in the context of medium-term prospects such as the use of H2O Raman lidar systems for monitoring water vapour content in the low and middle troposphere. Such monitoring will provide the necessary constraints to forecast extreme precipitation events, like those encountered during the fall on the reliefs of the western Mediterranean basin. Indeed, during the upstream phase of HyMeX, a good coherence was evaluated between the water vapour mixing ratio profiles measured by WALI and those estimated by mesoscale models such as AROME-WMED and WRF (Chazette et al. 2015a; 2015b).”

6) Please have a general check on the English language throughout the paper.

We have had the article corrected by a native professional translator.

MINOR POINTS:

7) Abstract, line 13: "relative mean" –> "relative difference". 8) Abstract, lines 13 and 15: add the percent sign after 5 and 3. 9) Introduction: spell acronyms (MISTRALS, IHOP, COPS, BASIL, IGN).

This has been corrected, except for BASIL and IGN which are both named after their host organizations (University of Basilicata and Institut national de l’Information Géographique et Forestière) and are not actual acronyms.

10) P. 10580, L. 2. Indicate which body recommends the 0.4 g/kg uncertainty (e.g. WMO or other)

It is NCAR and NOAA as stated in the title of reference Weckwerth et al., 1999. It has been added in the parenthesis.

11) P. 10581, L. 11. Add ’lidar’ before ’signal’.

This has been corrected.

12) Section 2. Add the ground-based humidity probe mentioned on P. 10590, L. 18

We added “Finally, a Vaisala PTU-300 on a 6-m mast yields WVMR measurements at ground level, with an accuracy of 0.3 g kg-1”.

13) Section 2.2. You mention 4 channels for the lidar. The first channel is at 354.7, the second one at 387, and the third one at 407. The fourth channel is never described. The advice of the previous review to clarify this has not been followed.

Indeed we had not understood the reviewer’s question after his last review. Sorry about that. The system is composed of three receivers. The four channels are co- and cross-polarized, N2 Raman and H2O Raman. Section 2.2 has been modified in several

sentences to be clearer on that point.

14) P. 10582, L. 13: "system" –> "receiver".

Because we have added “three receivers”, we “kept system”.

15) P. 10582, L. 18: add "range" after "dynamic". 16) P. 10583, L. 4: "N2O" –> "H2O".

This has been corrected.

17) P. 10583, L. 8: indicate if this previous calibration had been obtained in daytime or nighttime.

As detailed in the reference, it had been done first during daytime and checked afterwards in France during nighttime. The results were identical within the error bars so we kept the first calibration coefficient, since we did not reflect on precision at that time. We chose not to add daytime or night-time and not to explain all this so as not to confuse the reader.

18) P. 10583, L. 11: add "due to SNR" after "daytime".

This has been corrected.

19) Section 2.3. Discuss the probe on the kite BEFORE the radiosondes at Sant-Luis. Discuss also the radiosondes at Palma de Maiorca (mentioned at the top of P. 10587)

The first modification has been made. The second one has not since this part is deleted.

20) Section 2.3. Indicate the maximum height that can be reached with the kite in your current set-up. I assume that this is not the same as the cable length.

This is difficult to determine since it depends completely non-linearly (due to the hyperbolic cosine shape of the string) on wind speed, kite surface and weight, sonde weight, and string length and weight. By linearizing over a “small” increment of string in the exact same conditions, I can roughly estimate that with approximately 1/5 of string length

left when at 400 m, and only a $\sim 30^{\circ}$ angle of ascendance of the string at ground level, we would have been limited at 450 m. We added only this conclusion in the text: “This length of line allowed for an approximate maximum sounding altitude of 450 m in the wind conditions encountered in Menorca.”

21) P. 10583, L. 26: "could be" –> "were"

This has been corrected.

22) Equation 2. The transmission term has been neglected. Please state so in the text and justify.

We do not understand this remark. Forward transmission is the same, and backward transmission at either wavelength is mostly removed in the ratio, so only the difference of backward transmission remains, which is included in the term DeltaTau(z). Thus in the end we did not change the explanation of the equation.

23) P. 10586, L. 7. It looks that only the second flight could yield K-WVMR, as it was the only one reaching above overlap (see figure 3).

Indeed, but as the overlap function remains the same, we can evaluate it and divide the C(z) measurements by it. We can then evaluate K_WVMR on each flight. This had been detailed in a parenthesis: “(note that even if the first flight could not reach the full overlap altitude, as the overlap function remains the same, K_WVMR can be evaluated on each flight by dividing C(z) by this function)”.

24) P. 10586, L. 11. Also indicate the distance of full overlap (150 m).

This modification has been made.

25) P. 10587, L. 13. HYSPLIT does not give you the optical properties. This must be a typo.

They were in fact deduced from the inversion of the elastic and N2 Raman channels, but the text was confusing. This has been corrected by adding “and optical parameters

deduced from the inversion of the other channels of WALI".

26) P. 10588, L. 12. Indicate your profile truncation criteria at the far range (e.g. a threshold on SNR or standard deviation).

We added : "(truncation at SNR = 5 on the N2-Raman channel)".

27) P. 10589, L. 1. The RMSE is actually a "RMS difference" between two types of measurements not exactly co-located. I think that calling it an "error" is inappropriate. Correct the term throughout the paper (used several times).

RMSE had been chosen in coherence with a previous article by P. Chazette, but following this judicious remark, the term RMSE had been changed to RMSD throughout the current paper.

28) P. 10589, L. 2. Add "coefficient" after "correlation".

This has been corrected.

29) P. 10589, L. 17. The local differences within the PBL should be mentioned as sources of this decorrelation.

As previously explained, this part has been removed following Reviewer #1.

30) The formula on L. 2 on P. 10589 and Equation 3 have not been rendered correctly.

We do not see any rendering trouble in the PDF document downloaded on AMT. Could you please point out the problem?

31) P. 10591, L. 2. It is unclear which calibration factor is varying by 17%. It looks that your results show a much more stable calibration.

We were actually referring to Bock et al.'s drifting calibration factor. We try to avoid the confusion by adding "the same study by Bock et al".

32) P. 10591, L. 4. The MODIS discrepancies do not seem as dramatic as the AERONET. 33) P. 10591, L. 17. Add "of AERONET" at the end of the line"

This has been corrected.

34) P. 10592, L. 15. The word "not relevant" does not seem appropriate here. The fact is that they may be sampling a different airmass.

We changed the sentence as: "Radiosounding measurements from the approved balloon launch site were prone to sample a different air mass, which could be aggravated by balloon drifting."

35) P. 10592, L. 26. It is unclear why you mention sounding errors here. Perhaps, you wish to refer to a lack of correlation due to spatiotemporal variability?

It was indeed very unclear. We meant the WMR uncertainty due to the kite-lifted sonde and to atmospheric variability. We put: "uncertainty on the kite measurements" to keep it short.

36) P. 10593, L. 4. You should also say that the kite allows one to characterize the overlap of the lidar. 37) P. 10593, L. 13. "better" –> "worse" 38) P. 10593, L. 18 "excludes"; L. 19 "points" –> "seems to exclude"; "to point" 39) P. 10594, L. 3: "may" –> "could".

These modifications have been made.

40) Figure 5 caption: remove "natural" before "variability"

Figure 5 has been removed.

Interactive comment on Atmos. Meas. Tech. Discuss., 8, 10577, 2015.