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Atmospheric Measurement Techniques Discussions

Interactive comment on "Correction technique for raman water vapor lidar signal dependent bias and suitability for water vapor trend monitoring in the upper troposphere" *by* D. N. Whiteman et al.

Anonymous Referee #1

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A scientific paper, even in a journal devoted to instrumentation, needs to address topics beyond the strictly parochial, i.e it must be of interest to a section of the scientific community beyond the immediate group represented in it. For instrumentation this means finding a balance between a technical report of the particular instrument and a generic argument. This paper errs too much towards a technical report in its initial sections. Examples of this are in section 5, where an inordinately long discussion is given of a particular problem encountered in the MOHAVE campaign, and section 6.2 which is devoted to lidar data products.

The paper is written largely in a narrative style which makes it much too long and tedious to read. Its take-home point is that measurements of water vapour in the lower stratosphere can be used to remove biases in lidar measurements, thus enabling accurate water vapour trend detection in the upper troposphere. This argument is sound and deserves publication, but only becomes clear very late on. The authors should revise the paper to deliver this argument crisply and clearly, removing all the extraneous material and extreme verbosity of the current draft. The impression I get is that the authors have tried to adapt a technical report to a published paper without properly recognising the essential differences between them.

p. 7344 lines 2-5: 'The results discussed there indicate that the estimated total RH uncertainty for corrected RS92 measurements during the MOHAVE-2009 campaign were $\pm$(5%+0.5% RH) for RH>10% and $\pm$(7%+0.5% RH) for RH$\leq$10 %, which corresponds to an uncertainty of $\pm$6% at 50% RH, $\pm$10% at 10% RH, and $\pm$24% at 3% RH.' This sentence is very confusing because some of the numbers seem to be percentage RH and some seem to be percentage error in the percentage RH. Even so, they do not make sense. At 10% RH, the error is quoted as 7% + 0.5%. Is this not the same as 7.5%? And how does it then equal 10%? And are these RH or error percentages? This section needs a complete redrafting.

p.7344 l. 15 ALVICE

p. 7345 l. 12 have a strong

p. 7350 signal-induced noise in the photomultipliers can also produce the described effect , as is acknowledged on p. 7355; it would be helpful to refer to this briefly at the beginning of the section to emphasise that the current method doesn't apply to it.

Section 5 is much too detailed: the corrections proposed are quite straightforward and do not merit over six pages of text. In the end the authors choose a very simple correction because the signal-to-noise limits application of the 'correct' solution. It would be sufficient to describe this correction and give a measure of its accuracy.

p. 7358 l. 20. the final calibration value 20 used for the processing of the ALVICE Raman lidar data was determined by averaging the calibration constants determined from corrected RS92 and frostpoint hygrometer (FP). The two calibration constants differed by approximately 5 %. This averaging was done to compensate for the dry bias of the corrected RS92 data compared with frostpoint hygrometer shown in Fig. 12. I don't understand this. The FPH is generally regarded as the most accurate instrument for vertical profiling so why are you averaging the two calibration constants? Is this a trade-off of accuracy and precision? A proper explanation of this method is required

p. 7631. What is the point of including the format of the data files in a scientific publication? This belongs in a technical report not a published paper which should have some general relevance.

p. 7362 l. 9. 6 km not 7

p.7363 l.23. Here a large discrepancy between lidar and radiosonde is attributed to smoothing of the lidar in a region of large gradient. Does this mean you are comparing profiles with different height resolution? This is an elementary error, easily rectified by smoothing the sonde profile in the same way as the lidar, which surely a group of authors of this experience will have done? Please clarify.

p.7363 l. 27. The reminder of this section could be omitted. Water vapour is known to be variable everywhere (not just over a mountain) and a comparison of a radiosonde or FP with an all-night average profile could not be expected to be as good as with a 1-hr average. Similarly, the differences here are much too large to detect any but the grossest variations in ζ1.

Section 7.1 If this has already been published it should not be included here: reference should be made in the introduction to the previous study. It is not simple comparing a total column measurement to an integrated lidar profile because so much of the water column is in the lowest layers of the atmosphere where the lidar does not measure. The paper does make this point and describes a correction procedure, but that procedure

is not error-free and this section neither supports nor refutes the argument in the paper on correcting lidar water vapour profiles. If the authors want to include this comparison for completeness the section should be much shorter, refer primarily to the previous paper, and address honestly the uncertainty in determining an 'overlap correction' – this is not going to be the same each time a measurement is made.

Section 8. What is the point of this section? It needs to be drastically shortened, much of the narrative condensed into a Table and draw a conclusion relevant to the narrative of the paper. All I have learnt from reading it is that there are a lot of malfunctioning Raman systems, some with wet biases and some without. Unless the results from the other lidars have been published and proper reference can be made to them I don't see any case for a section starting 'The data acquired by all three lidar systems during MOHAVE-2009 have undergone various versions of processing.'

Section 9 is an interesting idea which allows the results of MOHAVE to be generalised to periods when FP measurements are not available. While it certainly should be included, this section again is far too long, and labours what is an obvious (and potentially elegant) argument. Of course, if MLS and FP agree during MOHAVE the lidar profiles corrected with each should be similar.

The Appendix has four pages and four figures, and its material is tangential to the argument in the paper. This material belongs in a paper on the accuracy of radiosondes, which would make it easier to find and reference in future studies. A summary with one take-home figure is appropriate here, but no more.

Fig.5 caption 'estimate'

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