Atmospheric Measurement Techniques Discussions

Atmos. Meas. Tech. Discuss., referee comment RC1
https://doi.org/10.5194/amt-2021-132-RC1, 2021

Comment on amt-2021-132

Anonymous Referee #2

Referee comment on "Mitigation of bias sources for atmospheric temperature and humidity in the mobile Raman Weather and Aerosol Lidar (WALI)" by Julien Totems et al., Atmos. Meas. Tech. Discuss., https://doi.org/10.5194/amt-2021-132-RC1, 2021

The paper by Totems et al. is a comprehensive study on different bias sources in Raman lidar measurements of temperature and water vapor mixing ratio, including mitigation strategies. The paper is not only interesting for this type of tropospheric lidar, but some aspects can be transferred to all other types of lidars. For instance, the spatial inhomogeneities of PMT sensitivity or questions of range-dependent beam overlap can be crucial for other lidars, too. After a general description of the potential bias sources and their mitigation (or at least quantification), the authors apply this information to their own Weather & Aerosol Raman lidar WALI. The authors documents that WALI fulfills the criteria set by WMO.

The manuscript is mostly well written, conclusive and well elaborated. The general methods of identification of potential bias sources are extensively described, even if some points are still confusing. Furthermore, I am missing some details in the application of these methods to WALI. Here, a more careful description is needed. Further details are given below.

Specific comments

l. 113: It is not clear to me, why r^ and T^ are used here, instead of r and T used in the remaining manuscript.

l. 137: Do you expect significantly different errors at other temperatures?

l. 142/143: This sentence should be rephrased (because R' is not needed at all for temperature measurements). Maybe: In order to fulfill WMO requirements for temperature and WVMR measurements, the SNR of the Q' ratio must be 6-10 times larger than the R' SNR, respectively.

l. 144-146: a) The phrasing seems to be odd. Please improve. b) If I understand correctly, the authors want to emphasize the relevance of bias estimation. However, they concentrate in the next sentences on SNR.

Section 2.3: This section is partly only relevant for receivers using free-space optics, with the described topics being of minor importance, if fibers are used. I recommend making clearer, which part of this section is of general relevance, because applicable to all kinds of receivers, and which is only relevant for receivers without fiber.

l. 229-231: This sentence sounds odd. Please rephrase.

l. 257: Please explain why a range-averaging should be applied here to calculated a range-dependent overlap ratio. The procedures with horizontal and vertical beam seem to be

identical, but only the differential extinction becomes more important with vertical beam. It is confusing to repeat the equations 19 and 20 (for horizontal beams) as 21 and 22 (for vertical beams), but introduce at the same time implicitly the overlap ratio OR. I suggest to define OR explicitly, but remove the Eq. 21 and 22. OR is partly used with a "hatch" (l. 104) and partly not (l. 581), but the difference does not become clear to me. Please explain additionally the difference between R(z) with and without overbar.
l. 271/272: If the overlap measurement has only limited relevance in practice, this should be emphasized already in the beginning of this section. Furthermore, I suggest adding a comparison of capabilities of the "theoretical method" described in Section 2.4 and the empirical correction (practical method) described later.
l. 289-291: This seems to be a contradiction to the statement in l. 361. Please explain.
l. 303: Could you please explain the "build-in leaks at 532 nm"?
l. 316-318: If I understand correctly, you expect a 4°C change of the seeder temperature within 5 min. This seems to be quite large, even for an uncontrolled system. Please check. Does it run into a more stable state after a few hours of operation?
l. 358-361: This is an interesting result, but double negations should be avoided. I suggest writing: "… even with the use of fiber optics the angle of incidence on the interference filters depends on the image positions in the focal plane of the telescope (i.e. mainly the distance to the optical axis), in contrast to what could be expected."
l. 361: As mentioned above, this seems to be a contradiction to the statement in l. 290.
l. 446: Line 332 says that a 1 mm fiber is used for soundings and Line 433 says that a 0.6 mm fiber is used for alignment/tests. Please check for consistency.
Figure 8: Please explain the normalization of the sensitivity. It seems odd that it is nearly always above 100%.
l. 477: In l. 464 a diameter of 1 mm is given. Please check for consistency.
l. 483: In l. 479 a diameter of 21 mm is given. Please check for consistency.
l. 486: Could you please give an example, which effect this sensitivity change has on the result (i.e. WVMR or T profiles)?
l. 496: It would be interesting to see an example of these spikes and to learn how their effects are mitigated.
l. 508: If I understand correctly, the baseline varies with time and is therefore measured every 8 min. Is this baseline subtracted from the previous or the following profile series? How strong is the variation with time?
l. 570: I recommend avoiding highly subjective terms like "rather lukewarm".
l. 605: The remaining bias of OR seems to be of similar magnitude like the initial correction. Does this make the elaborate overlap measurement with horizontal beams obsolete?
Figure 12 b/e and l. 608: The displayed data shows a very large scatter. Please provide also the error of the mean. Please explain the model to get the correction for OR (red line) from the measured mean ratio.
l. 643/644: What kind of variability do you mean? Spatial or temporal? Why does it only affect nighttime-data?
l. 646: I wonder why a wrong correction of OR shall be responsible here. OR has initially been measured and then corrected using the same set of radiosondes that is now used for comparison. Please comment. Regarding this whole section, I would prefer to see an independent comparison with another set of radiosondes than used for the corrections and calibrations.
l. 653: Please provide the distances between lidar/radiosonde and their respective closest ERA5 gridpoint, and between these two gridpoints. Has the drift of the radiosonde been taken into account or is the drift during the ascent up to 6 km much smaller than the distance between LCSE and Trappes sites (and ERA5 gridpoints)?
l. 667/668: It would be interesting to learn about potential reasons for these remaining differences, even if the authors cannot verify these in the context of this paper.

Technical comments / typos:
l. 19: typo “homogeneous”
l. 167: Please add: “0.12-0.4 % for temperature”
l. 171: Please introduce the sigma variable
l. 187: typo “thus”
l. 193: “effective refractive index”
l. 403: OD is not given in the figure.
l. 466: Add “neutral density filter”
l. 474: “not vary by more …”
l. 484: “convolution with …”
l. 525: typo “leads”
l. 577: typo “channels”
Fig. 11: The “^” in the legend should be removed.
l. 770 and 779: The superscript “2” should be corrected, here.