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Atmospheric Measurement Techniques Discussions

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Interactive comment on "Lidar temperature series in the middle atmosphere as a reference data set. Part A: Improved retrievals and a 20 year cross-validation of two co-located French lidars" *by* Robin Wing et al.

Anonymous Referee #2

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The paper by Wing et al. is the first of a pair of papers describing the improvements made for Rayleigh lidar temperature retrieval and the utilization of this data set for comparison with satellite data. The paper is mostly well written and extensive. It covers general topics of Rayleigh temperature calculation and is therefore very important for the growing community of middle atmosphere lidars. After a repetition of the general design of the lidars used for this study, the authors describe potential issues for the data quality, like electronic signal contaminations, tilted background levels etc. A thorough examination of these issues is important and highly welcome. The cleaned photon

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Discussion paper

count profiles are used for the calculation of temperature profiles. Here, two co-located lidars allow for a comparison of the results and the removal of data with minor quality. The authors state that their procedures provide an improvement compared to the Rayleigh temperature calculations in the NDACC database. As a result, a data set for the calibration of satellite data is build.

As mentioned, the paper covers several important topics for Rayleigh lidar data. On the other hand, partly basic textbook knowledge is repeated. I recommend a more concise presentation of the study. Some topics are mentioned without physical reasoning, circumventing the transfer to other lidar systems. Several examples and additional aspects are given below. I recommend the revision of the manuscript, addressing these issues.

Specific comments:

- P6-7L140-146: In this section saturation is neglected, but in Section 3.5.1 the correction is described and in Figures 10, 13, 14, 15 stratospheric data is shown. I suggest not to neglect saturation throughout the manuscript.

- P7L147-150: I have not found any number on the integration time for the temperature profiles used here. I assume that it is long enough to at least partly overcome the issue of non-LTE. If not, the potential errors by assuming LTE need to be described. The statement “unable to relax” would not be sufficient, if differences between data sets are examined and “standards” are defined.

- P7L151: This assumption is problematic as there are different studies showing aerosols up to at least 35 km.

- P8Figure3: The count rates are comparatively high and saturation is likely to become a problem (see above). I assume a typo in the vertical resolution of 7.5 m.

- P9Sec3.3.1: Please explain the (potential) origin of these spikes. Fig. 4 shows that they easily reach 10-100 counts, i.e. they are quite substantial. I wonder whether it would be useful to work on the origin of the spikes instead of only removing the resulting counts. Do you remove only the spiky bins or the whole profile? I guess, the first would result in too low counts rates in the altitude of the spikes after integration of several profiles. Please make clear.

- P10L207: Please explain "downstream counting rate".

- P10L207-219: Is the Kurtosis test always only done on the first 100 bins? If yes, how do you detect TES that may appear above that range? If no, how does the exponentially decreasing (i.e. non-Gaussian) signal influence the test

- P11L230-233: I do not see four groups of signals. Essentially it is either high background and low signal or low background and high signal. Please explain. Why does the number of groups depend on the statistics "the authors choose to use"? Which statistics? I am generally missing an explanation of the strategy or method. Why not simply defining a signal-noise-limit to separate good profiles and noisy profiles?

- P11L236 and Fig.6: The green line is not only a running average but contains some offset. Please explain.

- P11L238-243: It remains open how the blue line is derived. It is the result of a black-box-software and the results are discarded by the authors. I suggest removing this section and the blue line in Fig. 6.

- P12L249-256 and Fig. 7: Please explain in more detail how this test works. Please use for Figure 7 the same data set as for Figure 6. Otherwise the reader can hardly comprehend the method. If I understand the test correctly, it only removes the worst profiles of a particular night. If the whole night has a bad signal, the data will not be removed. Correct? In line 253 you do "not exclude" the bad profiles, but in line 256 13 bad profiles are identified (how??) and "discarded". I do see a contradiction between the two sentences.

- P13Sec3.3.4: I am sorry, but I do not understand this section. Why not simply considering only data up to altitude z by defining a criterion like SNR(z) > Threshold ?

- P14L284-294: The noise reduction is interesting. To allow the reader evaluating the technical progress, it would be helpful to learn a) whether these are the most important changes in background count rate for the whole 20 y data set and b) what are the benefits for the temperature calculation if the background is reduced to 1/100 (e.g., range extended by .. km).

- P16Sec3.5.3: It would be helpful for the interpretation of the results (also of the companion paper) to have a quantitative description of the influence of a wrong background shape on the temperature calculation. Additionally, the SIN of the low channel in Fig. 9 is extremely high and the choice of the shape of the SIN profile is essential. Why quadratic?? I suggest validating the resulting temperature profile with independent information.

- P19Fig10: From my point of view the upper range of the temperature is somewhat optimistic. There seem to be superadiabatic gradients at 75 and 80 km. 30% relative error is $\sim$70 K, i.e. the content of information is rather low. Which altitude is chosen for initialization? How is the signal smoothed for the choice of the initialization altitude (L395)? The melding of the signals should be visible in the uncertainties, but is not in Fig. 10. Please explain.

- P20Fig11: I suggest showing the error of the mean instead of the variance.

- P22L450: How many nights are excluded here?

- P22L452: Please mention the averaging window.

- P22L460: This conclusion cannot be proven without acknowledgement of the temperature uncertainty. The shaded area in Fig. 14 seems to show geophysical variability rather than measurement uncertainties. Fig. 13 shows persistent red or blue patches, indicating systematic differences between the lidars.

- P22Fig15: I am surprised about the small differences. Averaging the purple and blue

(40 and 50 km) line in Fig. 14, I would guess the difference is ∼1K. At 70 km the difference is close to 0 K, but ∼1 K in Fig. 11 (green and orange line). Is there any mis-interpretation from my side?

Minor comments:

P2L24-26: Please check this sentence (grammar).

P2L30-35: Please clean up the brackets, making this section easier to read.

P2L54: Remove "of two co-located lidar systems" and similar repetitions.

P3L56-61: I do not see this section relevant for the paper.

P5L99: I assume a dispersion of 0.3 mm/nm. Correct?

P6L136: "multiple scattered photons"

P6L137: "outside of the field of view"

P7L164-166: Example for textbook knowledge that can be removed.

P8L167-173: This section is partly redundant and should be shortened or removed.

P11L220: I suggest using "profile" instead of "scan".

P11L230: The intuition is always subjective. Please rephrase.

P11L235-236: I suggest deleting this sentence.

P13L260: Please explain "partial scan".

P18L367: "in an area of low signal"

P20L423: I suggest writing "The present study" instead of "This study".

P23L471: "colder" should read "lower"

P26L540-544: Sentences are mixed up. Please correct.

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