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Atmospheric Measurement Techniques Discussions

AMTD

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Interactive Comment

Interactive comment on "Intercomparison of stratospheric ozone and temperature profiles during the October 2005 Hohenpeissenberg Ozone Profiling Experiment (HOPE)" *by* W. Steinbrecht et al.

W. Steinbrecht et al.

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1 Response to reviewers comments

We thank both reviewers for their thoughtful and constructive comments. Both referees were quite supportive of our lidar validation paper. The two major suggestions of Referee #2 were:

a.) Not only show the much improved results for the corrected temperature algorithm. Also correct the ozone algorithm, and show results from the corrected ozone algorithm.

b.) Better establish the reference quality of the ozone and temperature profiles from the NASA lidar.

The comments by reviewer #1 were generally minor, but his/her last two comments pointed out that:

c.) Nonlinearities in the counting system, e.g. incorrect background subtraction, can lead to errors at the near and far end ranges of the ozone and temperature profiles.

In response to these major comments we have prepared a revised manuscript that will be submitted shortly.

1. We have corrected and improved the DWD ozone algorithm. We have rewritten and renamed Section 5. New Figures (Fig. 20 and 21) were added that show the required corrections and their typical magnitude for the improved ozone and temperature processing. This addresses points **a.)** and **c.)**.
2. Both improved DWD algorithms now also contain a better background/ signal-induced noise subtraction. Effects of this improved background subtraction are important at the upper range limit, and are discussed in the revised Sect. 5 with the two new Figures (Figs. 20 and 21). This addresses point **c.)**.
3. The old Figure 20 (now Fig. 22) now shows results for the corrected and improved DWD ozone algorithm. Agreement of DWD lidar ozone profiles with both NASA lidar and SAGE ozone profiles is now very good. In Fig. 21 (now Fig. 23) there was a minor change due to the improved background subtraction/ signal induced noise estimation.

4. We have added several paragraphs, especially in the introduction, as well as new references, in order to better establish the reference quality of the NASA lidar. Certainly for ozone, the excellent agreement between (corrected) DWD lidar profiles, SAGE profiles and NASA lidar profiles in the revised Fig. 22 also validates the quality of all three of these instruments for ozone. This is now mentioned in several places and addresses point **b.)**.

To better reflect the above changes, and for a more logical flow, the manuscript text has been reworded and rearranged, particularly in abstract, introduction, section 5, and conclusions. None of the main statements or conclusions of the first manuscript have changed.

We feel that these changes have addressed all major comments of the reviewers, and have improved the manuscript substantially. Again, we thank the reviewers for the constructive and helpful comments.

2 Response to specific comments of anonymous referee #1

Reviewer comments are in italic. Our reponse is in normal typeface.

- While such intercomparison does not documented the continuities of the data series, I recommend to remove in the abstract the sentence from "Long-term Record" to "over the years".

OK, done

- The comparisons have been careful handled and were then able to detect the small altitude drift in DWD lidar. A simulation performed by Leblanc and coworkers (J. Geophys. Res., 103, 6.177-6.187, 1998) shows how such problem affect temperature comparison and should be cited here.

We have added the reference.

- It is not clear in the manuscript, if this intercomparison was performed following the recommended NDACC protocol of blind intercomparison, guaranteed by an independent referee. It seems to be done that way when we read the manuscript. Even if it is not the case it should be mentioned while the publication of this study is still very valuable.

The intercomparison was blind in the sense that the initial data were sent blind to an independent referee. These were the data used for the initial intercomparison (Figs. 6 to 19). However then, in a second non- blind step, all results were available to both groups to allow a detailed error analysis and the correction of the discovered errors (Figs. 20 to 23 of revised manuscript.). We have added a paragraph to the introduction.

- In section 3, in the domain 25-30 km lidar temperature are reported to be too cool. Authors should also consider the potential role of aerosols. Small sporadic layers may appear in this domain even for period outside large volcanic aerosols loading that are difficult to detect and could participate to induce such bias.

This is a good point and we have added a few sentences, also in the conclusion.

- In section 4, where uncertainties were estimated and evaluated, it is important to consider that the photo-counting system can also biased these estimates. It depends strongly of the threshold of the pulse selection before the counting. This effect can be checked on the lidar return when no geophysical information is present in the background part of the signals. Few lines about how the threshold is fixed on the both systems can be added in section 2.

We agree, at high count-rates pulse pileup effects in the counting system can have a substantial effect both on photon-counting linearity, and on photon-counting statistics. However, at the altitudes of interest here we have made sure that these effects are minimized and corrected for. We have added a paragraph on page 42. More important

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is the estimation of signal-induced-noise (SIN) at the far-range end of the lidar signals. We have added substantial discussion of background subtraction effects at the low count-rate end of the signals, much of it in Sect. 5 and in the discussion of Figs. 20 and 21 (see our major point 2.)

- Finally, there is one discrepancy on temperature above 70 km that is not mush discuss in this mancript. It was also reported in a previous intercomparison (Singh et al., J. Geophys. Res., 101, 9.983-10.016, 1996) involving the mobile NASA system and can be due either to the initialization process or background extraction. This should be mentioned and is probably an interesting issue to be further study in the future, to be discuss and considered within the lidar working group of NDACC.

See our major point 2.) We have added text, Figures and references discussing signal induced noise and temperature differences. At the far range end initialization and background subtraction are important. These uncertainties are largely reflected in the large error bars above 65 to 70 km. As with ozone at its upper range limit at 50 km, it is very difficult to make any firm statements about temperature differences near the upper range limit (around 70 km for the MOHP lidar). It looks like the DWD usually gives higher temperatures there, but the number of cases is low, and statistical significance is very poor (compare Fig. 8, Fig. 21 (now Fig. 23)). We did not want to draw any conclusions based on the statistically insignificant differences above 65 or 70 km.

3 Response to specific comments of referee #2

Reviewer comments are in italic. Major comments start with "!". Page-line refers to the AMTD paper. Our reponse is in normal typeface.

!38-12: if you know how to remove it state that now. Otherwise it sounds like you don't know how to correct it, e.g. a problem with the differential filter used in the ozone algorithm has been identified as the source of the error and the DWD data base will be

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reprocessed with this correction".

As explained above, the DWD ozone algorithm has been improved, and corrected ozone profiles have been derived. This has resulted in much better agreement. The abstract has been rearranged and parts have been rewritten.

38-14: Results...consistent: consistently good? bad? What does it mean consistent? They all see ozone in the stratosphere?

Similar ozone bias in the 33 to 43 km region, and similar agreement at other altitudes have been observed in previous intercomparisons of the DWD lidar. The abstract has been reworded, and this point should be clear now.

38-18: not statistically.... put in ( )

Done.

!Abstract is not consistent with paper's title. The abstract focusses on the DWD lidar and says almost nothing about the NDACC system. Hence title amended to something like: Validation of the DWD lidar stratospheric ozone and temperature measurements during the ....."

We describe results from both lidars and other instruments, and address their variations and differences. In addition, we have added more text and information especially about the NASA lidar. We feel that our title is justified and we have not changed it.

39-10: IN 1990

changed

39-25: What does Keckhut give an overview of?

added "of ozone and temperature lidar validations performed within the NDACC framework"

40-12: This paper discusses (or shows or highlights) results from the HOPE inter....

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Much of this text section has been changed in a rewritten introduction.

40-13: as you say focus is on DWD lidar, hence you should amend the title (see above).

As mentioned above, we feel that our title is justified and we have not changed it.

Table: "signal induced noise $\lambda_{on}/\lambda_{off}$; (near 100 km)" entry for NASA lidar is missing or should be marked N/A.

Has been included (and changed to near 50 km). Signal induced noise (SIN) for the NASA lidar at 308 nm is about 200 Hz near 100 km, 600 Hz near 50 km, and is accounted for by non-linear fits. At 353 nm, SIN is negligible.

Fig 1 caption: you may want to put on the figure or in the text what the physical separation between the transmitter and receiver mirrors is.

Added in Table 1. Separations are about 0.7 m for both DWD and NASA lidars.

41-25 "state or configuration" are better words here than "status"

changed

42-5 the range bin is 300 m. The bin width for each sample is 1 microsec, corresponding to 150 m (don't say range bin here).

changed "bin" to "interval"

42-8 is not strongly or appreciably or significantly absorbed by ozone.

changed

42-10 similarly

Changed

!44 How do you use your ozone profiles to correct your density profiles to get temperature? If you use the straight Rayleigh profiles with correction you will have an error on the order of a degree around 30 km. There are corrections in the literature for this

effect, but in your case you know the ozone to make the correction. It is important you discuss what you do (or don't do) concerning this point. I can provide references if needed.

The reviewer is mistaken here. Ozone absorption is only important when using the 2nd harmonic of a Nd-YAG laser at 532 nm, but it is negligible at the 353 or 355 nm wavelengths used here (Leblanc et al. 1998, Sica et al. 2001). As the reviewer states, the temperature error caused by neglecting ozone absorption at 532 nm (Chappuis band) is about -1 K at 30 km, and about -1.7 K at 25 km (e.g. Sica et al., 2001). However, we use 353 or 355 nm, where the ozone absorption cross-section is a factor of 40 smaller than at 532 nm ($7 \times 10^{-27} m^2$, compared to $2.8 \times 10^{-25} m^2$ at 532 nm). The temperature error caused by neglecting ozone absorption at 353 or 355 nm is also a factor of 40 smaller!! It is only about -0.025 K at 30 km, and smaller above. This is negligible compared to most other sources of error. We have added a paragraph and two references discussing this.

46-2 terminated and the time centering adjusted. (delete short sentence).

Ok, changed

!49-2 "since both systems fire at different times": didn't you see previously you synched the choppers so they fire at the same time! Please explain this apparent contradiction.

Both systems were synched, but they were still firing at different times. Firing sequence like this:

1, 2, 1, 2, 1, 2 - same frequency, different phase.

We have deleted the rest of the sentence starting at "since". The point is that the photon noise between the two systems is uncorrelated. The rest of the sentence was obviously more confusing than helpful.

!50-17: if NASA is off by 1.7x and they are the calibrated transfer standard don't all the other inter-comparisons they have done need to be re-evaluated?

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Only the NASA uncertainty estimate is off. Mean values and standard deviations in other intercomparisons are not affected by this! Old intercomparisons may have to be re-evaluated as far as error estimates go. For bias and correlations they still remain valid. However, we don't think it is our job here to re-evaluate all previous campaigns. This would not be helpful for the purpose of the present paper. We have, however, added discussion in several places about using the GSFC lidar (or any other instrument) as the "gold-standard".

50-24: emperature should be temperature

Thanks!

!58-14: you should say explicitly in this sentence (or one more sentence): what the algorithm problem is is it fixable? will it (and when) be fixed

This has been addressed by reprocessing the DWD data with a corrected algorithm. The success of the corrected algorithm is now shown in a revised Fig. 20 (now Fig. 22), and is discussed in an additional figure and in several additional paragraphs.

! I am puzzled by one thing after reading this study: why didn't your try your analysis with a better differential filter and show the improvement to proof this important result in your paper?

Good point. We have now done exactly that.

! The paper would benefit from (early on) rather than just quoting previous studies tell us how well you believe the NASA lidar is calibrated so we have a lower limit on what to expect (in the past against whatever the gold standard is, how well does it compare, 0.1%, 1%, 10%?).

The NASA lidar is accurate within 1 to 3% for ozone between 20 and 45 km, and precise within 1 K for temperature between 15 and at least 50 km. In the introduction and other parts of the text we have added this information. There is now more discussion as well as additional references for the accuracy of the NASA lidar.

4 References

Leblanc, T., McDermid, I. S., Hauchecorne, A., and Keckhut, P.: Evaluation of optimization of lidar temperature analysis algorithms using simulated data. J. Geophys. Res., 103, 6177–6187, 1998.

Sica, R. J., Zylawy, Z. A., and Argall, P. S.: Ozone corrections for Rayleigh-scatter temperature determinations in the Middle Atmosphere, J. Atmos. Ocean. Tech., 18, 1223–1228, 2001.

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