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Atmospheric Measurement Techniques Discussions

Interactive comment on "The ground-based MW radiometer OZORAM on Spitsbergen – description and validation of stratospheric and mesospheric O_3-measurements" *by* M. Palm et al.

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We would like to thank for the constructive and factual review.

1) The ARTS user manual is accessible to the public via (http://www.sat.ltu.se/arts/docs/).

The theory of radiative transfer has been published very often and is discussed in textbooks (e.g. Janssen, 1993). It does not seem necessary to repeat this theory in great detail. The theory presented here is meant as a link between canonized theory and variables and the results presented in this paper. The presentation will be clarified in the revised paper according to the suggestions of the referees.

The validity of the ARTS forward model has been investigated by the ARTS developer group (e.g. Melsheimer, 2005) and is not in the scope of this paper.

We do not agree, that the oscillatory errors are not comparable to other millimeterwave measurements, see also below. It will, however, be investigated to some extend for the revised version of the publication.

2) The oscillations do not vary with time, they are truly systematic errors.

The oscillations are in relative units. Because daytime ozone in the mesosphere is very small, the oscillation have a larger percentage than during night time.

The reason for the "slope" in the comparison with the SABER data is unclear, especially it is not observed in the comparison with MLS data.

3) This will be clarified in the revised version.

4) This is correct, it has to read "standard deviation".

5) Refer to answer to referee #3 (AC C 908).

6) It has been stated in line 5 page 1945 and also in table 1 that the error figures as well as parameters depending on it are given for integrated measurements of 1 hour.

The System noise temperature is by definition independent of the integration time.

7) We agree, that the wording 'unreliable' is too strong. The choice of the upper limit of 0.1 hPa has indeed been chosen due errors exceeding 100 % above.

8) The satellite data are considered high resolution data (in terms of altitude). They have been smoothed using equation 18. We actually prefer the term "simulated retrieval" as has been introduced by Rodgers and Connor (2003). Without this recalculation the measurements are not comparable, because the so called smoothing error is not accessible due to the lack of a proper statistic of the ozone profile (compare Rodgers, 2000, p. 49). The smoothing error describes the error on the profile due to

the low altitude resolution.

9) Figure 9 is supposed to show that an error in spectroscopy can produce oscillatory error patterns similar to those observed in Figure 10. If we could reproduce the oscillatory pattern, it would mean we would have a corrected set of spectroscopic parameters. In this case we would have published the data retrieved with the improved set of spectroscopic parameters.

We did actually try to find an improved set of spectroscopic data, but did not succeed. We suspect that the reason might be the inadequateness of the Voigt function (Drouin and Gamache, 2008) for the radiative modeling of this line, but we cannot not investigate this because we lack the equipment and the know-how to perform such a study.

10) We refrained from giving a top altitude for the data for several reasons:

a) The topmost altitude depends on the SNR of the spectrum. This means it depends on the ozone profile itself and the tropospheric background.

b) The measurement contains information up to 100 km altitude. But this is a mixture of columnar and profile information. The exact information content is encoded in the AVK - matrix.

c) The topmost altitude depends on what one would like to study. For a comparison with models or other measurements the atmosphere up to 100 km altitude has to be taken into account, because that is the limit which contributes to the measured spectrum.

Consideration of those points led to the Cautionary note 3.3. .

11) The profiles shown by Hocke (2007) differ in several aspects from the profiles shown here:

a) The standard deviation of the comparison is not given, only the mean. It would be interesting if an oscillatory structure exists in the standard deviation of the comparison.

b) If the rise in the difference on the upper altitude range in Hocke (2007) is due to a

severe oscillation in the retrieved profiles cannot be seen.

c) Both comparisons are given in percent of the measured ozone values. Those values are much higher in midlatitude regions (6-8 ppm) than in polar regions (4-6 ppm). The oscillatory structure might be therefore be comparable in absolute values.

d) There are little oscillations compared to the MLS instrument in the paper by Hocke (2007). Comparisons to other instruments show oscillations up to 10 percent and with a structure similar to the OZORAM comparison.

Nevertheless an hitherto unknown error in the OZORAM measurements, which does not occur in the measurements presented by Hocke (2007), cannot be ruled out without a detailed investigation in the differences of both retrievals.

General:

"Stating that more investigations are needed ... "

We do not understand if this is a criticism or an encouragement. We do, however, fully agree and there will be ongoing improvements and investigations. We also intend to use the data in further studies especially related to mesospheric chemistry and dynamics.

References

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