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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.

Anonymous Referee #2

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General comments:

This paper contains a description of the OZORAM microwave instrument for measuring ozone profiles, a discussion of the radiative transfer, and a theoretical error analysis of the measurements. It shows a few figures showing phenomena observed at the instrument's Arctic location without elaboration. It reports results of intercomparisons with ozone profiles from SABER and Aura-MLS. Finally, it presents an argument for the cause of oscillations commonly seen when profiles from a ground-based microwave instrument are subtracted from coincident profiles obtained from a different type of instrument.

Generally speaking, the theory of ground-based microwave measurements of ozone and other stratospheric constituents has been well described in the literature, and there is nothing that is fundamentally new in the manuscript in this respect. I give below two detailed references that have not been mentioned in the manuscript. This material could be condensed, leaving that necessary for the arguments presented in the manuscript.

The microwave ozone-measuring instruments developed in Europe (such as OZORAM) and those developed in the U.S. (e.g. the NDACC instruments in Hawaii and New Zealand that are described in the references) followed different development paths and differ in many technical details. For example, in the processing of the data from the NDACC instruments the generation of the ozone signal (the radiative transfer through the stratosphere) and its absorption in the troposphere (which must be accounted for in the calibration) are treated separately: the calibration process calculates the ozone signal that would be seen if the absorbing tropospheric layer was not present. In OZORAM, a radiative transfer model that combines the effects of all relevant atmospheric constituents is used to calculate the radiative transfer through the entire atmosphere and yield a net ozone signal seen at the input to the instrument. To the extent that the published information allows, a discussion of the effects of differences between OZORAM and the previously-described systems on their respective errors would be a useful contribution to the literature.

The discussion of the intercomparisons between OZORAM and the satellite-borne instruments should be improved and published, as it will be useful to those interpreting results obtained from OZORAM measurements.

The comparison between profiles retrieved using the HITRAN spectral line database and those retrieved using the JPL database is also interesting. This reviewer is not aware of previous use of the HITRAN database in this application.

Comments on the manuscript as it presently stands:

The intercomparison discussion is somewhat limited in that only two independent sources of ozone data are used, and that there is no discussion of other available information regarding errors in these measurements. In the absence of other suitable independent ozone data, the inclusion of a summary of published studies regarding errors in the MLS and SABER measurements would allow some conclusions to be drawn regarding the extent to which the differences between the OZORAM and comparison profiles are due to OZORAM as opposed to comparison profile errors.

The error discussion gives only the most cursory description of an important class of errors, namely the absorption by tropospheric water vapor and oxygen of the stratospheric ozone signal. Connor et al (1) found that, for a similar instrument, this error component was about twice as large as the spectral measurement error component. The opacity error therefore is important and needs to be treated for the paper to be complete. Further, this class of errors has two components. One is error in the model relating the tropospheric absorption coefficient to the constituent (e.g. water vapor and oxygen) densities, and the second is uncertainty in the tropsopheric temperature profile supplied to the retrieval algorithm. The means of accounting for weather- and seasonally-induced variations in this profile should be explicitly described in the text.

There's no entry in Table 2 for measurement noise, and the connection between equation 12d in the manuscript and equation 2 is not clearly specified. The noise seen in Figure 2 looks like it is dominated by random fluctuations. These are large because of the 60 kHz spectral resolution throughout the spectrum and the short integration time. Their size can mask other kinds of spectral error, such as baseline effects, that vary on a much longer time scale, if at all. It should be noted that considerably more data will be averaged together for many applications, such as the intercomparisons discussed in the paper, in which case the baseline effects, etc., may become the dominant spectral error. The error analysis would therefore be more realistic if either the data were smoothed to a lower resolution, or many hours of data were averaged together, or both, to obtain an estimate of the spectral error that applies to integration times involved in

typical uses of the data. A figure showing the results of a long integration would be more useful than the one shown in the paper.

The argument in section 5 is not convincing. The character of the oscillation in the intercomparisons shown in Fig. 10 is ~2 complete oscillations over the range of the profile, while the character of the blue difference profile in Fig. 11 is dominantly a single peak tailing into a constant error at higher altitudes. I don't see the relationship between the two.

The oscillatory error pattern in Fig. 11 is characteristic of measurements made with ground-based microwave instruments, and could be due to any kind of error that does not change significantly over the period during which the MLS and SABER intercomparisons were made. It is likely that there are several contributing error components. The conclusion that "...up to 50% of the deviation..." (page 1952, line 15) can be attributed to spectroscopic error appears to recognize this, but the argument that led to the 50% figure, which appears only in the conclusions, should be clarified in the text.

It is curious that the oscillations in the calculated forward model error shown in Fig. 5 don't appear in the corresponding calculation contributing to Fig. 1b in reference (1). Of course, the ozone transition being measured in the earlier work is different, and there are numerous other technical differences between the two experiments. Is it possible to reconcile the cause of the lack of forward model error oscillations in the earlier work given their presence in the results of your calculations?

References:

(1) Connor, B. J., A. Parrish, J. J. Tsou, and M. P. McCormick, Error analysis for the ground-based microwave ozone measurements during STOIC, J. Geophys. Res., 100, 9283-9291, 1995.

(2) Parrish, A., B. J. Connor, J. J. Tsou, I. S. McDermid, and W. P. Chu, Ground-based microwave monitoring of stratospheric ozone, J. Geophys. Res., 97, 2541-2546, 1992.

Interactive comment on Atmos. Meas. Tech. Discuss., 3, 1933, 2010.