

Interactive comment on “Atmospheric ozone above Troll station, Antarctica observed by a ground based microwave radiometer” by M. Daae et al.

Anonymous Referee #1

Received and published: 4 November 2013

1) General comments

This paper presents the retrieval results of stratospheric and mesospheric ozone profiles in the southern polar region based on microwave spectroscopy. The authors carried out continuous ground-based measurements of ozone spectra at 249.49GHz for about two years, from February 2008 to January 2010, at Troll station in Antarctica. The retrieved ozone profiles cover the pressure range from 3 to 0.02 hPa (corresponding to 38 to 72km) including the altitude where the tertiary maximum of ozone appears in the winter polar region. The instruments, calibration method, and retrieval procedure are concisely summarized and necessary information is described sufficiently enough. The

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



error budget is analyzed by perturbing the spectral data and retrieval parameters. The retrieved ozone profiles are validated by comparison with positional and time coincident dataset obtained by Aura/MLS satellite and SD-WACCM model calculations. The error analysis and data validation methods are basically reasonable and appropriate, but some details need more careful discussions.

The measurement results are invaluable to understand the seasonal variation of ozone profiles, especially detailed behavior of the tertiary maximum in the higher altitude part of middle atmosphere in the southern polar region. In addition, the dataset obtained above Antarctica is very important because of the lack of observations in this area compared with the northern polar region.

I recommend this paper for publication, but some details of discussions on data quality are not clear and to be improved. Thus, I think revision of the manuscript, especially chapter 4 is necessary taking the following points into account before the manuscript can be accepted in ESSD.

2) Specific comments

2.2. Dataset

Line 28, Page 517: The authors described that the threshold of noise level is 0.71 K. Does this threshold mean peak-to-peak value or 1-sigma level? In Line 20 of Page 518, there is another noise level threshold of 0.4K. What is the difference between those two thresholds? In Figure 3b, there seems to be systematic wavy feature in the fitting residual indicated in red line. Does the “noise” include such wavy feature not only random noise?

3.2 Results of the retrievals

Line 26, Page 519: It is reasonable way to evaluate the actual sensitivity of the observation, i.e. independence from the a priori by using the measurement response, but I think that the technical term of “measurement response” is not so common. So I

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

think that the authors should give the definition of measurement response more definitely and justify the appropriateness of a threshold value of 0.8 by quoting appropriate references.

Line 8, Page 520: The authors described “a weak secondary maximum of 1ppmv at 91km”. However, both the AVK and the measurement response are almost zero at this altitude, so I think this statement is meaningless. If the authors think this statement is significant, please clarify the reason. I think that similar description “3.2ppmv at 93 km” in line 12, Page 520 is also meaningless.

3.3 Error characterization

Line 7-9, Page 521: The treatment of calibration error is not clear. The hot/cold-load intensity scaling, standing waves, and the tropospheric correction factor are independent and completely different type of quantities. There is no quantitative description about the detailed breakdown of the “calibration errors”. Why those different types of errors are imposed only in the tropospheric corrections? Why an upper limit of +/- 10% is appropriate value? Please explain the treatment of the calibration error more in detail and more quantitatively.

4.4 Temporal variations

Figure 6, Left panel: In the time series plot of VMR at 0.04 hPa (upper left panel), the black data points (BAS-MRT) seem to be distributed “bimodal” in spring and autumn seasons. One group of data points show good agreement with SD-WACCM rather than MLS, and the other group of data points are concentrated almost “zero”. Are these nearly zero values of VMR in spring and autumn are real or artifact? If these data points are artifact, the authors should eliminate these data points from the statistics. The black line (BAS-MRT) and red line (MLS) at 0.04hPa show apparently good agreement in spring and autumn, but in fact, there are very few black (BAS-MRT) data points near the black line. This is very strange to me. The black line is in between the bimodal distributions. If the nearly “zero” values are artifacts, then the present black line should

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

be meaningless. Such a bimodality of VMR values can be seen at 0.39 hPa (middle left panel) between Jan-09 and may-09 as well. In my impression, such bimodalities are due to artificial effect occurring in the retrieval calculation. The authors did not mention about these bimodal distributions in the manuscript. I think that the authors must discuss more carefully and more in detail whether these bimodalities are real or artifact. In case the bimodality is due to artificial effect, the authors should eliminate the wrong data points from the statistics.

Figure 6, Right panel: In the right panel of Figure 6, the correlation coefficients at 0.04hPa are high and almost the same both for MLS-BAS and WACCM-BAS. However, in the left panel, the apparent correlation between the blue and black line (WACCM-BAS) is clearly worse than that of the red and black lines (MLS-BAS). In my impression, the correlation coefficient in the right panel seems to be wrong.

4.5 Comparison of profiles

Line 25-26, page 525 and Figure 8: The authors discuss the data quality by using only one averaging profile. There is, however, strong seasonal variation between winter and summer, especially in the middle mesosphere, as the authors mentioned in the manuscript. I think that the authors should compare the profiles at least summer and winter seasons separately in order to evaluate the quality of retrieval results.

3) Technical corrections

Line 13, page 523: One of the three pressure levels “0.21 hPa” must be “2.21 hPa”.
Lines 18, 24, and 25: Replace “green” by “blue” for the color legend in Figure 6.

Interactive comment on Earth Syst. Sci. Data Discuss., 6, 513, 2013.

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)