Atmos. Chem. Phys. Discuss., 5, S338–S343, 2005
www.atmos-chem-phys.org/acpd/5/S338/
European Geosciences Union

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Evolution of stratospheric ozone during winter 2002/2003 as observed by a ground-based millimetre wave radiometer at Kiruna, Sweden" *by* U. Raffalski et al.

U. Raffalski et al.

Received and published: 7 April 2005

We choose to answer the editor's comment and the comments of the two referees in one document.

In order to increase the transparency of the comments and the authors' answers we include the referees' comment in the text.

Answers to the comments of referee #1

1. The paper reports measurements performed using a microwave radiometer op-

erating near 200 GHz. This frequency range is not common for an ozone sensor. The frequency range of the radiometer is chosen for observation of chlorine monoxide at 204 GHz. Due to strong baseline effects which still could not be eliminated the radiometer is operated for ozone measurements at 195 GHz. Other lines to up to 215 GHz have been tested. The retrieval of the double peak structure at 195 GHz turned out to be most robust and stable with respect to standing wave contribution to the measurement. The instrument is only very briefly characterised, unfortunately the performance of measuring other atmospheric constituents is not discussed, only one comment is made concerning a baseline problem. Very little details of the technical concept are given, and the reader is advised that more information will be made available in a paper "to be published". Also the profile retrieval is only discussed very briefly. In summary both the instrument design and operation and the retrieval procedure are quite standard. Therefore I would suggest to either describe the instrument and the data analysis in more detail if it is felt that some new and interesting results can be presented, this section could replace the "to be published" paper, or to shorten this part considerably.

The authors' answer: We agree with the referee in that the most important part of this publication is about the measurements and the data itself. We therefore shortened the description of the method and the instrument and refer to two publications where more detailed information can be found. However, we left the data analysis part unchanged in order to inform the reader about the resources used in the data analysis.

2. The dynamic and chemical analysis of the winter is straight forward and the found ozone depletion compares with similar data obtained by the SMR instrument on Odin. In fact the analysis makes extensive use of Odin data and the overall impression of the paper is rather a cross validation of the Odin SMR instrument and the ground based sensor in Kiruna. Such a validation effort has its merits and should be published. However the paper needs to be revised by either concentrating on the data analysis and intercomparison only, or to present a detailed discussion of the instrument in Kiruna.

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The authors' answer: The referee's impression that Odin data is extensively used in this publication is hard to understand. Basically we make use of Odin N2O data only for the estimate of the diabatic subsidence. The intention was not to cross validate satellite ozone data with our ground-based ozone measurements. However, we mention Odin ozone loss as well as the ozone loss detected by millimeter wave and FTIR measurements in Kiruna of another year at the end of this paper in order to show that the millimeter wave data have a reasonable order of magnitude and compare well with results of other groups. A proper validation, though, will follow in the near future including not only Odin but also Sciamachy and MIPAS data.

3. The text should be carefully edited to improve the English, and to remove some typographical errors.

The authors' answer: For the removal of typographical errors and in order to improve the English the text has already been proof-read by a native speaker for the first round of the review process. Before submitting the final version we will do that again.

4. The x-axis in the right panel in figure 1 is not correct.

The authors' answer: The x-axis (from 0 to 1) has already been replaced in the context of the first round of the review process. The new version has the correct label. The referee may want to check the online version of our paper.

5. In figure 2 the text in the figure (1993-2000) and the text in the caption (1992/1993-2001/2002) are not consistent.

The authors' answer: This has been corrected.

6. Figure 3, the grey lines are extremely hard to see.

The authors' answer: We admit that figure 3 is quite complex. Nevertheless the information is important for our discussion of the meteorology and the results. The lines depicting the inner and outer vortex edge have been shown in the only color that can contrast to the contour plot below the lines, white. Again we suspect the referee probably has received and looked at the originally submitted version of the paper, that is, before the first round of the review process when the lines still were dark grey. In the online version the line is white and quite good to distinguish from the colored contour plot background.

7. Figure 4, according to the text total ozone above 10 km is plotted, but the instrument only reaches down to 15 km. Has a mean ozone profile been assumed below 15 km? In this case it would be better to plot total ozone above 15 km only.

The authors' answer: As shown in Figure 1, the instrument has a sensitivity for ozone of more than 75 % above 15 km. This was taken as threshold for the statement that our instrument is sensitive above 15 km. But in fact below this altitude the sensitivity decreases not abruptly to zero but vanishes rather smoothly. This is one reason why we have chosen 10 km for the partial columns in order to get maximum information out of our measurements. Below that altitude merely a priori contribution can be found in the retrieval. But even if the sensitivity would abruptly drop to zero below 15 km the integration for the columns should start below 15 km. The vertical resolution as shown in Figure 1 is a measure that expresses the full width at half maximum of the averaging kernel at a certain altitude. The vertical resolution amounts to 10 km at an altitude of 15 km which implies that the averaging kernel at 15 km shows significant contribution to the retrieved volume mixing ratios even 5 km above and below. For example an ideal delta shaped peak at 15 km would be smoothed to an altitude range with significant contribution between 10 and 20 km. In order to get a reasonable result for the column density calculation one has to start the integration below 15 km and that is what has been done in this paper. A clarifying paragraph has been added to the manuscript.

Answers to the comments of referee #2

8. I remain doubtful on origin of the local O3 mixing ratio maximum at 25 km in early winter intra-vortex profiles. It produces a notch, which I believe is too deep and probably of instrumental origin.

The authors' answer: We can only repeat at this stage that the double peak structure or 'notch' as the referee puts it has been reported earlier and has been seen in the Kiruna data quite frequently. Since it does not appear in the data continuously we do not consider this feature being an instrumental effect.

9. I also consider the purely statistical error bounds that have been derived unrealistically small considering all the uncertainties of the experiment.

The authors' answer: As explained in the first round of the review process the overall uncertainty of the experiment is about 1 ppmv. This is also mentioned in the report. To print this permanent error as error bars in the figures would not help to clarify the data but more likely would make the figures more incomprehensive. We rather assume that the reader remembers this overall error when being informed in the figure caption that the error bars presented in the figures are solely statistical errors due to averaging over certain periods.

10. For example, the authors explain the increase of ozone mixing ratios on N2O isopleths in the beginning of December as being combination of change of ozone profile shape (compression) and limited vertical resolution giving artificial increase of the mixing ratios as the subsidence progresses over the winter. This may well be the case and it would then lead to the systematically too low measured ozone loss. On the other hand, the increase between the two first means is to my understanding too large to be explained with this hypothesis alone. Here, I have an alternative suggestion: It seems to me that simply the determination of the 021203-021208 average (black profile at fig. 5), for a reason or another, is way too low, e.g. at 20 km it should be closer to 3 ppmv than 2 ppmv.

The authors' answer: We agree with the referee in that the explanation given for the unusually low ozone values in mid-Dec is not completely satisfying. However, we do not have any better one. The quality of the mid-Dec data is no worse than later in the winter season and we do not see any reason to be more doubtful about it.

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Interactive Comment

Technical corrections

11. Font sizes of some figures (in the print version) continue to be hopelessly small, as I already said in my original review. One can argue that the web-version is zoom-able, so this does not matter so much, but often the print version is the only one you have with you.

The authors' answer: The font size in figure 3 is admittedly small. However, in order to present the PV values in the contour plot this cannot be avoided. It might not be the most important information, though. All other font sizes but in fig 3 and 4 are now even more increased.

12. After some zooming I was able to see that comma is used as decimal point in figures 5 and 6. There is one printing mistake on page 144 when discussing MW/Odin comparison: should be 0.5 +/- 0.2 ppmv and 0.9 +/- 0.2 ppmv instead of 0.5 - 0.2 ppmv and 0.9 - 0.2 ppmv, respectively.

The authors' answer: This has been corrected.

Interactive comment on Atmos. Chem. Phys. Discuss., 5, 131, 2005.