Atmos. Chem. Phys. Discuss., 5, S233–S236, 2005
www.atmos-chem-phys.org/acpd/5/S233/
European Geosciences Union


Atmospheric Chemistry and Physics Discussions

ACPD

5, S233–S236, 2005

Interactive Comment

# *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.

**Anonymous Referee #2**

Received and published: 23 March 2005

General comments:

The study introduces a newly build MW radiometer and ozone observations made with it in the winter of 2002-2003 at Kiruna. Observations include an exceptionally strong minihole event in the beginning of December and several warming events. These are given as such without in depth quantitative analysis. In qualitative terms the effects of warming events to the ozone mixing ratios at higher altitudes are seen from the temporal mixing ratio developments. The main focus of the study, however, is the vortex





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ozone loss aspect. The authors have done quite detailed meteorological analysis of the Kiruna stratosphere winter to help put measurements in the PV coordinates. I see the met analysis as a strong point of the paper. Despite the possible methodological problems discussed below the estimated in-vortex ozone losses agree reasonably well with earlier publications on this same winter. The prominent feature of 2002-2003 winter, an exceptionally large early ozone loss (discussed in many other papers ), has been reproduced in KIMRA measurements as well. The paper is also a well written, concise report and introduces significant new instrumentation. Therefore, after observing these minor comments/corrections, as the case may be, either in the manuscript itself or in the discussion it can be published in ACP. It should be instructive to compare the vortex ozone losses retrieved by different methods in this interesting and well mapped Vintersol winter.

The main bonus of this paper is of experimental nature: An introduction, and to my knowledge, the first scientific application of new powerful polar ozone chemistry profiler KIMRA, a 195-225 GHz MW radiometer capable of continuous O3, ClO, N2O, HNO3 measurements at a strategic campaign and monitoring location of Kiruna. In the long run, if well maintained and sufficiently funded, KIMRA has potential to produce valuable information both on the year to year variability of polar ozone chemistry and long term trends of ozone profile including the important 40 km height region where, in the nearest decades, one expects to see the first signs of anticipated ozone recovery.

The main weakness of the 2002-2003 winter KIMRA experiment was the failure of non-ozone channels to produce data and therefore, for example, the diapatic correction for the directly measured ozone losses had to be made using ODIN vortex averages of N2O. However, I consider this acceptable in this particular winter when Kiruna was well inside the vortex during several fairly long periods of entire winter until mid March, which made it possible to gather sufficiently representative in-Vortex ozone data.

Specific comments:

Since my comments on the first version of this paper has been sufficiently taken into account in the present discussion version I do not have many further suggestions. 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. I also consider the purely statistical error bounds that have been derived unrealistically small considering all the uncertainties of the experiment. 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.

Technical corrections

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.

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 ś 0.2 ppmV and 0.9 ś 0.2 ppmV instead of 0.5 - 0.2 ppmV and 0.9 - 0.2 ppmV, respectively.

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



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