

Interactive comment on “Retrieval of stratospheric ozone profiles from MIPAS/ENVISAT limb emission spectra: a sensitivity study” by N. Glatthor et al.

Anonymous Referee #2

Received and published: 9 January 2006

General Comments

The paper “Retrieval of stratospheric ozone profiles from MIPAS/ENVISAT limb emission spectra: a sensitivity study” reports on a series of ozone retrieval sensitivity studies applied to one orbit of MIPAS data. The results of the study were used to define a new version of the operational MIPAS ozone retrieval.

The paper is clearly structured and well written. The main results are adequately summarized in the abstract, and all relevant details are supported by appropriate figures and tables. MIPAS measurements provide global ozone profiles on both the day and night side of the earth, and information on the retrieval and in particular its uncertainties

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are relevant for all users of this important data set.

However, the paper is very technical and in my opinion provides no novel scientific results, new retrieval methods, or a description of a significantly improved data set. I therefore feel that ACP is not the right journal for this type of paper, which I would rather expect as a technical note provided with a new data version by ESA.

In addition, I have serious concerns with the approach taken by the authors in several respects:

- only one orbit of data has been used (how many profiles are included?) which in my opinion is not enough to come to meaningful results. For example, stratospheric variability in the Northern Hemisphere is at a minimum in October, and results in NH winter and spring might be quite different. As a minimum, a set of orbits covering different seasons and atmospheric conditions (ozone hole, no ozone hole, high tropopause, low tropopause, vortex air, non vortex air etc.) should be used in a sensitivity study on which the decision for a change in processor is based
- the decision on whether or not a change in retrieval settings is an improvement should be based on first principles (as the authors do) but also on comparison with independent measurements. The authors use results from a previous study (Wang et al., 2005) and reference to an upcoming paper in a very general way in the discussion, but given the fact that only one orbit was analysed, I don't think this is enough
- in several places, the authors argue that any change of the ozone profiles which is within the estimated overall uncertainty of the retrieval is not significant and a change in retrieval settings therefore acceptable or not necessary. Considering that the error budget is dominated by spectroscopic uncertainties, I'm not convinced that this is a valid assumption e.g. when testing the impact of the a priori

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profile. In any case, an end-to-end comparison between old and new retrieval should be included.

Specific Comments

The discussion of latitude dependent occupation matrix casts some doubts on the use of the quantitative method used to determine the optimum set of micro windows. Although the authors present some plausible arguments why some of the micro windows should rather not be used, and also discuss the offset resulting from the use of windows in different MIPAS bands, it still is puzzling why the polar set should be the most appropriate globally. Using one fixed set of micro windows will certainly reduce apparent inconsistencies such as jumps, but the differences observed point at an yet unresolved problem in the data.

Technical Comments

Section 2.2: Define OM

Reference Mengistu et al.: author list mixed up

Reference Wang et al.: has been published

Interactive comment on Atmos. Chem. Phys. Discuss., 5, 12031, 2005.

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