**Atmospheric
Chemistry
and Physics
Discussions**

Interactive
Comment

Interactive comment on "Retrieval of stratospheric ozone profiles from MIPAS/ENVISAT limb emission spectra: a sensitivity study" *by* N. Glatthor et al.
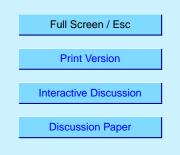
Anonymous Referee #1

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General Comments

This paper provides a justification for the choice of microwindows and cloud flagging employed in the IMK processor for ozone. This is a valuable data set and therefore deserving of good characterisation. The paper therefore also offers a sensitivity analysis with respect to a priori assumptions, continuum retrieval, and regularization. These are all important aspects that should be publishable within a journal paper.

This paper is very clearly structured and written. The real problem with the paper is that it does not have very well argued conclusions which are of significant import; it chooses to simply present results without strong investigation or explanation. To be

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acceptable, I feel forced to point out that the paper should have a major conclusion, for example, in the area of spectroscopic errors, cloud influences on the data or our modelling of radiative transfer in the tropics. Otherwise the paper becomes a rather routine report of the retrieval of ozone from limb emission profiles as is noted in the introduction to the paper itself. I would strongly encourage the authors to continue their studies and produce a new paper focussing on one or two of a number of important issues that they just begin to touch on in the paper.

One of the issues sometimes raised is the quantity of data which are required for retrieval studies. In this paper, one orbit of data is analysed. Here what matters is the choice of orbit which does seem to cover a wide range of atmospheric situations in terms of a priori and regularization studies. However, the authors should comment on this and convince us that this is the case not only for ozone but also for contaminants. For statistical studies, such as cloud flagging one orbit may not be sufficient and I see no reason why further orbits cannot be analysed.

Specific comments

Specific areas which the paper which are very interesting but require more work:

1) Section 3.2 Influence of the background continuum emission. This may well be true but the authors should first explain by reference to spectroscopy of expected particles and gas continua why one might expect to be able to allow the approximations they have made. Is it not possible to compare the continua retrieved at each microwindow which would show where the wavelength dependences exist.

2) Section 3.3 Influence of the cloud index. This is a start to a study which could be useful. However, it says that 1.8 and 4 are different choices of threshold but does not test for cloud indices in between the thresholds. The retrievals are different but how do we know what is correct? Much more work or justification needs to be done here.

3) Section 3.5/3.6 latitude dependent microwindows and spectroscopic errors. if there

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are deviations from expected radiative transfer in the tropics then this should be characterised and reported more comprehensively. What does this tell us about our ability to model radiative transfer in the tropics and to what level in radiance terms? Could the error on the relevant microwindows be increased and a new microwindow selection performed for the tropics? Similarly, in terms of spectroscopic error, MIPAS should provide an excellent test of the relative spectroscopy accuracy of different spectral features. Such a test might be done in the forward model sense as well as the retrieval sense. What does this tell us? How can we characterise the radiance calibration errors and therefore distinguish between spectroscopy errors and radiance calibration errors? The study needs to come to a conclusion on this.

4) A general issue is validation. Reference to the Wang et al. study is fine but for major conclusions, it would be better to present specific figures showing the variations in ozone retrievals relative to the error bars of the intercomparison with other ozone measuring instruments. If using other infra-red instruments, the issue of which spectral bands are being used by these instruments should be discussed.

Technical comments

Given the large revisions and further work required in the areas above, I do not supply detailed technical comments but just a few to guide the authors.

1) There are a number of English errors which need to be corrected but no doubt the authors planned to do this anyway.

2) The climatological profiles used should be referred to as the IG2 climatology V3.0

3) The quality of the error figures (Figure 2) is not very good and should be improved as I found them difficult to read.

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

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