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Atmospheric Chemistry and Physics Discussions

Interactive comment on "MIPAS level 2 operational analysis" *by* P. Raspollini et al.

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The authors would like to thank referee #3 for his/her thoughtful annotations and for his/her useful suggestions.

Below the answers to all individual comments are provided.

General comments:

1) First of all, a clarification: as it is explained in the Introduction, the Optimised Retrieval Model (ORM) is the scientific code used as reference for the ESA operational Level 2 processor performing both the Near Real Time (NRT) and Off-Line (OL) analysis. The only difference between NRT and OL analysis is given by the auxiliary data (the OFL analysis using extended retrieval height ranges and more stringent convergence criteria).

It is true that the computing time required for a single profile processing in the NRT and

OL is not reported in the paper, even if it is a relevant parameter for operational analysis. The reason is that the computing time for NRT analysis was already discussed in the previous paper on ORM (Ridolfi et. al., (2000)). In the revised paper a sentence will be added at the end of Sect. 7 with the indication of the run time performances of the code for both NRT and OL analysis. However, the decision of why data processing cannot be split among a reasonable number of computers is an operative choice that is not part of the scientific optimisation process. From the point of view of retrieval optimisation it has to be noted that the splitting of the processing of an orbit in different processes has the disadvantage that the retrieved profiles in the previous scans are not available as initial guess for the subsequent scans and more iterations are needed for the retrieval.

2) The errors contributing to the total error budget depends on the species, the error source, the altitude and the atmospheric condition. Therefore, it is not generally possible to summary this information with a few numbers (see also reply to next point). As required in a subsequent specific comment, convergence errors will be quantified (see answers to specific comments).

3) Actually, the information providing the final accuracy of the OL analysis at all altitudes is given, for midlatitude daytime conditions, in the plots of Figures 7 and 8. Concerning the NRT analysis, and other atmospheric conditions (mid-latitude night time, polar summer day-time, polar winter night time, equatorial day time), which imply an increase of the amount of data by one order of magnitude, the corresponding plots (as well as the corresponding data) are provided at http://www.atm.ox.ac.uk/group/mipas/err/ (quoted in the text). It must be underlined that Figs. 7 and 8 provide the quantitative information about all the errors that are discussed in the paper and several specific comments seem to have missed this point. In order to make the text clearer, the statement 'In this section the results ... are reviewed in order to verify the estimation of each error component...' has been moved and is now an introduction of the subsections of Sect.4. The use of a plot instead of a table seems to us an easier and more compact mean to

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visualize the data. A significant complexity is already given by the plot of the different error contributions which are individually reported since, as written in the paper at the end of Sect.5, most of the forward model errors can be either random or systematic according to the ensemble of measurements that is being considered.

4) This paper is part of a Special Issue on MIPAS. The results of intercomparison of MIPAS data with correlative measurements will be reported, for each retrieved species, in a dedicated paper. This fact is not explicitly mentioned in the paper and a sentence will be added in the Introduction to explain it.

Specific comments:

1) The numbering of the plots will be corrected. Concerning the readability of printed plots, Figures 7, 8 look quite clear on our printed-out version.

2) The paper refers to the scientific algorithm only, for which no changes where needed after the launch with respect to the pre-flight version. No mention is deliberately done in the paper to the different versions of MIPAS operational products, since they refers to differences in both Level 1 and Level 2 processors, as well as to differences in auxiliary data. However, what is described in this paper is in line with V 4.61 and V4.62 of MIPAS operational products. This information will be added in the paper.

3) Pag. 6529, lines 7-14. See the answer to the first general comment above.

4) Pag. 6531, line 15. We agree that mention of the fact that CO2 lines are used to retrieve temperature and pressure profiles has to be done the first time we speak about p,T retrieval (change made in the text). CO2 profile relative to year 2001 has been used so far. We found that the relative error in pressure (delta(p)/p) induced by the relative error on CO2 VMR (delta(CO2)) is not greater than 0.5*(delta(CO2)/CO2), while an error of about 1% in CO2 VMR induces an error in T between -0.1 and +0.1K (with largest errors at high altitudes). For future reprocessing of data, a yearly update of CO2 profile will be used. The error contribution of CO2 VMR error is already included

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in the total error budget of pressure and temperature shown in Fig.7.

5) Pag. 6534, lines 2-5. The results of the analysis performed by the ORM with MIPAS balloon and ATMOS measurements will be summarised in the text, as well as the conclusion of the tests performed in the frame of the AMIL2DA project.

6) Pag. 6535, line 5. The 'initial guess profile' is the profile used in the forward model for the simulation of the measured atmospheric spectrum. This can only be the whole profile from 0 to 120 km, as stated in the quoted sentence, even if the retrieval range is limited to the height range 6- 68 km. Indeed, the 'retrieved' profile above and below the retrieval range is not given exactly by the climatological profile, but by the climatological profile multiplied by an appropriate factor.

7) Pag. 6535, line 17. The terms used are standard. Some confusion was caused by the fact that day and night were not explicitly noted in the text. We will use capital letters to denote the conditions as: typical mean yearly profiles for Tropical (0°), Mid-latitude Day (45°) and Mid-latitude Night (45°), and typical mean seasonal profiles for Polar Winter ($>$70° in winter vortex, nighttime conditions), Polar Summer ($>$70° in summer sunlit conditions).

8) Pag. 6537, lines 9-11. If the microwindows were chosen simply to minimise the random error (i.e. maximise the retrieval precision) then every spectral line of the target species would contribute positive information and the microwindow selection would continue until every line of the observed spectrum was included. However, our selection has been based on minimising the total error (i.e. maximise the retrieval accuracy) so the additional systematic errors associated with some spectral lines would outweigh their reduction of the random error and this determines the point at which additional microwindows start to contribute "negative information". As part of the ESA study that generated the selected microwindows we did investigate the maximum number of microwindows that could be found while still improving the retrieval accuracy - typically of the order of 100 for most species, while less than 10 microwindows

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are used for each species. Another study, just focussing on pT microwindows, can be found in http://www.ecmwf.int/publications/library/ecpublications/_pdf/workshop/2004/ws_AIRS_dudhia.pdf. This reference will be added to the text in order to identify the source of our statement.

9) Pag. 6540, line 19-23. We agree that the text is not sufficiently clear. The text will be expanded with the following explanations. If the assumed shape of the profile above the highest tangent altitude is different to that in the real atmosphere, then the retrieval tries to compensate for the error in the estimated slant column by attributing a higher or lower value to the retrieved concentration at the highest tangent point. This error can propagate to the lower tangent points with an amplitude that quickly damps out as the distance from the highest tangent altitude increases. The profile below the lowest tangent point is observed through the tail of the IFOV on the low altitude side. For this case, differences between the assumed and real shape of the profile lead to a incorrect computation of the IFOV convolution for the lowest tangent altitude and hence to an error in the retrieved concentration for the lowest retrieved point.

10) Pag. 6542, lines 15-24. As far as the first part of the comment (how the threshold relates to accuracy) is concerned, we are not sure if we understand what is asked. According to the subsequent comment, some quantitative estimates of the convergence error will be added in the revised version of the paper. As far as the second part of the comment is concerned (uniqueness of the solution), particular care is dedicated to the definition of the initial guess of the retrieval (weighted mean between retrieved profile at the previous scan and a merging of ECMWF profile, if available, and climatological profile) in order to start as close as possible to the solution. Furthermore, the microwindow selection tries to avoid the use of thick lines so that the problem is moderately non-linear. A comment will be added in the text about the uniqueness of the solution.

11) Lines 25-27. With the new thresholds, convergence error is about 0.3 times the random error, with the old thresholds this ratio is in some cases approximately equal to

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1. The text will be expanded accordingly.

12) Pag. 6543, lines 25-28 and Pag. 6544, lines 1-3. These lines imply that further work is needed. Indeed this is stated in the final sentence of the Conclusions, as required by the referee.

13) Pag 6548, lines 4,5. The altitude where the error becomes noticeable is different for the different species and different latitudes and can be deduced from Figs. 7 and 8 and from plots available at the web page quoted in Sect. 5. A reference to the Figure and to the website will be added in this section.

14) Pag 6551, lines 9-10. Text changed as suggested.

15) Lines 12. As stated in Sect. 2.3, the microwindow selection performs the minimisation of the quantity defined by Eq. (5). Line mixing is one of the terms that contribute to Eq. (5). Therefore, there is not a threshold of the line-mixing error for each microwindow. The comparison between the error spectra and the residuals that is discussed in Sect. 4.3.1 is only indirectly related with the microwindow selection process and is instead intended to verify the correctness of our assumptions about the line-mixing error.

16) Pag. 6552, lines 3-14. Probably the problem of this section is no so much the unbalance between the discussions of simulations and GEOFIT tests, but the fact that it did not put these two activities in the correct perspective. This section will be partially rewritten as follows. '... Simulations have shown that gradients in temperature are the largest source of error among all gradients (Carli et al., 1998). Pressure gradients have negligible impact compared with the associated temperature gradients (typically 1% change in pressure for a 1 K change in temperature). In Figs. 7 and 8 a standard estimate of a gradient of 1 K / 100 km is made. In order to verify the impact of the homogeneity assumption, ORM results were compared with those obtained by the GEOFIT retrieval algorithm (Carlotti et al., 2001) that performs the simultaneous retrieval of all the observations acquired along a full orbit and accounts for the horizontal inho-

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mogeneities. Differences between the results of the two retrieval codes may be larger than the random error bars, but they are not correlated with the horizontal variability of the atmosphere. Therefore the observed differences are to be attributed to the differences between the two models rather than the horizontal homogeneity assumption in the ORM and it was not possible to provide a stringent experimental constraint to the amplitude of this error. However, the results of original simulations (Carli et al., 1998) and the absence of a positive detection of any effect suggest that retrieval errors due to horizontal inhomogeneity are estimated with conservative values.'

17) Pag. 6553, lines 4,5. It seems there has been a misunderstanding here. What we meant is that ideally, the best way to assess the non-LTE effects of the measurements of a given instrument is to have accurate and independent measurements of an instrument not affected by non-LTE. We understand the reviewer comment in the sense that, in reality, that is not the usual case, and that is true. We have worded the sentence to avoid confusion. New text: 'Ideally, the assessment of non-LTE effects would be best performed by comparing with co-located accurate and independent measurements taken by instruments not affected by non-LTE. Such measurements ...'

18) Lines 6-11. Some explanations are needed.

I. Kutepov et al. work was mainly devoted to understand why SABER temperatures around the mesopause region (80-85 km) in the summer hemisphere were colder in ˜10-20K (and poses the mesopause 3 km lower) than rocket in situ (falling spheres) measurements, as were reported by Mertens et al., Geophys. Res. Lett., 31, L03105, doi:10.1029/2003GL018605, 2004. In their work, they were able to show that vibrational- vibrational exchange of v2 quanta among the CO2 isotopes significantly changes the CO2 isotopes(010) populations in these extreme conditions and, henceforth, the SABER wide band radiance. The effects of this on the SABER retrieved temperatures was to increase them by 10-20 K in the 80-85 km range, and hence explains the previous disagreement between SABER and falling spheres temperature data. So, in conclusion, the comparison of SABER temperature with lidars (Garcia-

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Comas, 2003; Mertens et al., 2006) and falling sphere (Mertens et al. 2004; Kutepov et al., 2006) gives a very good overall agreement even under the extremely cold summer mesopause. We should stress that the above- mentioned V-V process was already implemented in the SABER operational non-LTE temperature retrieval scheme (version 1.06, before Kutepov's findings) although it was switch off for the sake of computational efficiency and in the thought that it has a minor effect (as it has, except for the very extreme conditions of the polar summer mesopause). This means that this process was already included in the CO2 non-LTE model we have used for the analysis of MIPAS data (from which the SABER code has been derived) and, henceforth, DO NOT APPLY the Kutepov's findings to it.

II. In addition of the point above we should also mention the following reasons:

a) Kutepov's findings were limited to the region above 75 km. The contribution of the mesopause emission to limb paths at tangent heights at and below 70 km is very small under these conditions because of the low temperature and the log-pressure decay.

b) SABER is a wide-band instrument and hence affected by all bands, including the contributions of the weak isotopic bands which Kutepov et al. found were not correctly modelled. MIPAS is a high-resolution instrument and the lines selected at high altitudes for T retrieval are mainly from the fundamental major isotope 010-000 band, which are not significantly affected by the V-V coupling mentioned above.

Action: We have just added a reference to the Kutepov et al. work.

19) Line 10. The manuscript is still under the revision process. No action.

20) Lines 19-25. The work of Manuilova et al (1998) refers to studies using simulated spectra, using a different set of microwindows and with a different (not updated) O3 non-LTE model. The estimates given in this paper are for the measured spectra, for the actual microwindows used in the O3 retrieval, and includes an updated O3 non-LTE model. There were already included estimates in the text (see last line in p. 6553 and

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first 3 lines in p. 6544). We have included the appropriate reference to these estimates. Action: Included the reference: López-Puertas et al. (2003)

21) Pag. 6555, line 13. All details above the previous studies of the chemical excitation rate of NO_2 and the much smaller rate found from the analysis of MIPAS spectra are given in Funke et al. (2005). Since that discussion is quite long, and this paper is already quite long and not specifically devoted to non-LTE, we think it does not worth to include it here. The specialized reader would read the Funke et al. paper anyway. For the referee information, we may brief that the reason for the lower rate is that previous estimates were too large because of the high-noise wide band (not resolving the fundamental and hot v3 bands of NO_2) measurements on which they were based. No action has been taken.

Typos, style etc, will be corrected as suggested.

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 6525, 2006.