Atmos. Chem. Phys. Discuss., 6, S2977–S2982, 2006
www.atmos-chem-phys-discuss.net/6/S2977/2006/

Atmospheric Chemistry and Physics Discussions

ACPD

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Interactive Comment

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

P. Raspollini et al.

Received and published: 8 September 2006

First of all, we gratefully thank Dr. MariLiza Koukouli for the careful reading of the manuscript and her useful suggestions.

With reference to the general comment we note that the paper is also addressed to MIPAS Level 2 data potential users (to this purpose it describes the latest upgrades in both Level 2 code and auxiliary data, and the performance of the code in term of accuracy and vertical resolution) as well as to people interested in Level 2 analysis (verification of correctness of the different choices implemented in the code).

The paper only deals with Level 2 analysis. Concerning the Level 1 analysis, only the results of the characterisation of the instrument performances as deduced from the analysis of the spectra are discussed, given the importance of the accuracy of Level 1 products for Level 2 analysis. A deep insight on Level 1 analysis is performed in a separate paper, to which reference is made.

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Point 1 about the title. It is true that the title is not as informative as it could be and that further information is probably desirable if an individual publication is considered. However, this paper is part of a special issue on MIPAS results entitled 'MIPAS: potential of the experiment, data processing and validation of results' and the title of the paper clearly identifies the tasks and the focus of the paper.

Point 2 about testing/validation/comparison campaigns post-launch. As already stated above, this paper will be part of a Special Issue. The comparison of MIPAS operational products with correlative measurements will be the subject of dedicated papers that will be included in the same Special Issue. Therefore this paper provides instead a verification of the choices that have been implemented in the code and of its performances on the basis of self- consistency tests (comparison of assumed error spectra with the measured residuals, verification of assumptions on the calibration of the spectra by means of analysis made on the spectra and so on). In the Introduction of the revised paper a sentence will be added specifying that the results of intercomparison with correlative measurements will be contained in dedicated papers of the same Special Issue.

Note has been taken of the specific comments annotated in the pdf document directly sent by e-mail to P. Raspollini by the referee Dr. Mariliza Koukouli. Of these comments, a reply is here given of the points that can be of general interest and that deserve some discussion. The unmentioned comments have been accepted and the text of the revised paper will be modified as suggested.

1. Pag. 6526, line 12. The question is asked whether the target products are listed according to the sequence of the VMR retrievals. The target products are not listed in the sequence of the retrieval, which is subsequently given in Sect. 2. The order of retrievals is the following: tangent pressures and temperature profile simultaneously, and then sequential retrieval of H2O, O3, HNO3, CH4, N2O and NO2 profiles. In the abstract we believe that this technical feature is not relevant and the target species are listed on the basis of their importance and chemical relationship.

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2. Pag. 6527, line 4-5. It is suggested not to mention AK in the abstract. Averaging Kernel, together with the VCM of the retrieved profiles, provides a fundamental information for the characterisation of the Level 2 products. As a consequence, the hint in the Abstract to the fact that the Averaging Kernels will be subject of discussion in the paper and that the equations needed for their practical applications will be provided seems important to us. We believe therefore that the use of known terms in the abstract is a secondary requirement.

3. Pag 6528, line 17. The question is asked why the 'no operator choice' has been mentioned. The fact that the code has to work without an operator played a role in the development phase since it required an effort to make the code sufficiently 'intelligent' to decide the best option to be adopted among the foreseen ones on the basis of the measurements. Example: selection of the more appropriate set of microwindows to be used in presence of corrupted spectra.

4. Pag 6535, line 25. The question is asked of whether the microwindow technique is used for other high-resolution satellite spectroscopic measurements. The microwindow selection algorithm has also been applied to TES spectra (limb & nadir), ACE transmittance spectra and performance studies for future MIPAS-like instruments. However, to our knowledge there has been anything formally published on any of these.

5. Pag 6537, line 19. The question is asked of whether there are any references to quote here for these seven years of work. Some papers and Technical Notes written in these seven years by the authors of this paper have been already quoted in the text, like Ridolfi et al. (2000), Dudhia et al. (2002a), Dudhia (2002) and it is not necessary, in our opinion, to repeat the references here. Only the reference to the Final Report of the ESA Study that supported the development of the ORM will be added here.

6. Pag 6538, line 5. The question is asked of how other satellite measurements analysis tools deal with clouds. The method here describes a "cloud filtering" rather than "cloud detection" approach. Most satellite instrument retrieve profiles of extinction in

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order to identify clouds. For our case, which concerns infra-red retrievals, this has two problems. First of all, temperature profiles would have to be retrieved first and these would still be in error. Second, the retrieval process itself costs time, may not converge (for example in the case of cloud scattering) and may smooth cloud extinction to higher vertical levels above the cloud. For our purpose, the ideal solution is a cloud filtering algorithm which is fast, does not involve retrieval, and classifies cloud influences on the measured spectrum to be analysed independent of the altitude of the cloud. The implemented algorithm achieves all these aims and is realizable because MIPAS is a spectrometer with wide wavelength coverage unlike instruments like CLAES. We propose to modify the second paragraph on p.6538 (line 24) to have a different starting sentence and also to include an additional Spang reference which places the algorithm in the context of other cloud measurements: "For MIPAS operational processing, the ideal cloud filtering algorithm was one which would be fast, did not involve iterative retrievals (to save CPU time) and which classified cloud influences on measured spectra independently of the altitude of the cloud. Fortunately, the MIPAS spectral coverage allows this to be achieved very well using threshold tests applied to the ratio of the integrated signals in two microwindows (Spang et al., 2001, 2002)."

7. Pag 6539, line 27. The question is asked on what Figure 3 demonstrates. According to the reviewer, it would be more formative for the purpose of this paragraph (which is to say that the new MIPAS spectroscopy is improved re the previous one) to show the modelled HNO3 bands for both spectral databases. The comparison between MIPAS measurements and simulations with the old and the new spectroscopic database has already been shown in the quoted technical note (Flaud and Piccolo, 2003). The comparison is there made in selected microwindows, because high spectral resolution can best highlight the spectroscopic differences. The figure given in the present paper is meant to show the generally good agreement that is obtained between observations and simulations also for a broadband comparison. The differences that are present when the old spectroscopic database is used are rather difficult to observe in the case of a broad simulation and do not help much the reader. For this reason in the paper we

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show the state-of-the-art and refer to other publications for the characterisation of the rate-of-improvement.

8. Pag 6548, line 1. A clarification is asked of how the assumed profile outside the retrieval range affects so much the retrieved profile. Referee #3 also made a similar comment. We are referring to the error called HIALT in Table 3.

Sect. 3.3 (rather than Sect. 4.2.2) will be expanded with the following considerations. 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.

9. Pag 6548, line 22. Some clarifications on the meaning of the 'various errors' and the 'number of papers' were required. The text will be changed as it follows: '... in several papers reporting spectroscopic parameters no real estimation of the errors is performed'.

10. Pag 6550, line 1. The word 'also' was required to be deleted. The word 'also' was intended to underline that the presence of a 'ghost' was not visible even on averaged spectra. The word 'also' will be replaced with 'even'.

11. Pag 6550, line 23. Clarification is required on the meaning of 'to neglect some effects'. We mean that with a dedicated selection of the used spectral points it is possible to exclude, for example, the points that are more affected by the Non-LTE, so that the assumption of LTE can be appropriate. The text will be corrected as follows:

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’Time efficiency is obtained by means of both physical and mathematical optimisation, as well as the use of microwindows that allows to reduce the effect of some deficiencies in the forward model.’

12. Pag 6555, line 27. It is asserted that the quoted website is not in use. Actually, the indicated website is in use, a temporary link problem may have been encountered.

13. Pag 6557, line 15. Clarification is required on when AKs are calculated (before, during or after the retrieval). The AKs are calculated before the retrieval, using the climatological profiles as linearization state. This explanation will be provided in the text.

14. Pag. 6575, caption of Fig.4. An approximate reference to the values of potential temperature height/pressure levels is required. The explanation that the range 500-3000 K of potential temperature approximately corresponds to the altitude range 20-120 Km will be added. Furthermore in the text the justification on the use of equivalent latitudes will be added.

15. Pag.6577, Fig. 6. The question is asked whether there was a particular reason to use a different scale for this plot than the previous ones. A new plot with scale +/- 25% will replace the old one.

16. Pag. 6578, Caption of Fig. 7. It is asked the meaning of ’rootsumsquare’. Root-sumsquare will be replaced in the text with ’root-mean-square’.

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