

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

Anonymous Referee #3

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General comments

I think that it is a helpful paper for the users of MIPAS data since thorough examination and description of all possible error sources makes the experimental dataset to be more sound. However, I think that the following issues are to be addressed before the paper can be published.

*) The computational efficiency issues are mentioned throughout the whole paper. However, there is no quantitative estimates of the times required for a single profile processing using the different versions of the code, namely, NRT, ORM and OL. Without these estimates it is not clear why the data processing cannot be split among a reasonable number of computers to eliminate a lot of troubles associated with optimization.

*) Though the paper deals mainly with the error values, the latter are often presented

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only qualitatively (see the comments of the other referee and specific comments below). In general, it would only give more credibility to the data if the qualitative estimates were accompanied by the quantitative values in brackets.

*) From the point of view of the end user the greatest practical utility would be a paragraph with a table and a plot that demonstrate the final accuracy of the OL and NRT codes at all altitudes. Though the paper is devoted to this very topic, the information is spread among different parts and is not represented as a whole.

*) Although the verification of some MIPAS products has been mentioned in the paper, it would be advisable to have a separate section with the intercomparison of the absolute values of atmospheric profiles retrieved from MIPAS measurements. I think that there is a whole lot of data to be compared to (lidars, rockets, space experiments). Since the estimated error bars will be used in this section it makes sense to put it to the end of the paper just before or after the "Example of the results" section.

Specific comments

Organization of the plots. As it has been already mentioned, the numbering of the plots 4-9 should be corrected. The readability of printed plots with error budgets (Fig.7,8 in current enumeration) is not good enough though the pdf version enables one to expand the plots.

P. 6256, line 14: no change in respect to what version of the code?

P. 6529, lines 7-14: see the general comment about the values of times and thresholds above.

P. 6531, line 15: it is not written here that (p,T) values are retrieved from CO₂ radiance. The first mentioning of this channel is on P. 6537, line 6, that seems to be too late. It is also worth mentioning what CO₂ profile has been taken for the retrievals and how sensitive is the temperature to CO₂ VMR. Please note that this is the basic channel since it provides the (p,T) information for other retrievals, therefore, it should be studied

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and described with a special care.

P. 6534, lines 2-5. The results of the validation are not discussed and the errors are not estimated. The intercomparison with stratospheric balloon and solar occultation experiments can also be mentioned and revised in the section proposed in "General comments".

P. 6535, line 5: it should be added here that the climatological profiles have been used not only for the initial guess but also below and above the retrieval range. The first mentioning of this fact is on P. 6540, lines 19-21, and it deserves more discussion (see below).

P. 6535, line 17: add "Southern and Northern midlatitudes" or change the description of the atmospheres to some standard notation like "Midlatitude Summer", "Midlatitude Winter", "Tropical", "Subarctic Summer", and "Subarctic Winter" to avoid the misinterpreting.

P. 6537, lines 9-11: it is not clear if it is a proven statement or it comes from the authors experience? Is it defined by the noise in the microwindows or by some other factor? Please, specify.

P. 6540, lines 19-23. First of all, as it has been mentioned above, it is not clear how this technique will affect the (p,T) retrievals. The phrase "it may introduce the bias in the highest ... point of the retrieved profile" is incorrect due to the geometry of the measurements – the optical paths for lower tangent points in limb geometry contain the contributions of the high-lying layers that can (and will) affect the signal. The lower the tangent point the lower the effect on the retrieved value but, anyway, the general formulation should be changed. Though this is partially proven and described on the P. 6541, lines 8-13, it requires some refinement.

P. 6542, lines 15-24: please, show how the threshold relates to accuracy in each channel for self-consistent retrievals. Moreover, in general, good convergence doesn't guar-

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antee an accurate retrieval – say some words about the uniqueness of the solution, especially, for the case of the optically thick lines.

P. 6542, lines 25-27: add quantitative estimates (see the "General comments")

P. 6543, lines 25-28 and P. 6544, lines 1-3: how should one understand these lines – that the current errors do not correspond to the requirements and that the forward model errors presented in the paper are not "sufficiently understood"? If this is correct then it should be reflected in the conclusions.

P. 6548, lines 4,5: specify the altitude when the error becomes noticeable

P. 6551, Section 4.3.1 Line mixing:

lines 9,10: please, reformulate. The error cannot be taken into account by selection of the microwindow. May be, "the error of this approximation is minimised by microwindow selection".

line 12: "are most affected" – the values are missing. In general, there should be a threshold for each microwindow, below which the line mixing effects are noticeable. Please, give some representative example.

P. 6552, lines 3-14: since the actual estimates of errors has been obtained using the simulation rather than comparison with GEOFIT. May be it makes sense to write more about the former and less about the latter?

P. 6553, lines 4,5: in general, this is not true. One has either to use a "non-LTE free instrument" or use a "non-LTE instrument" with a correct non-LTE model.

lines 6-11: there was a number of reports this year (e.g. Kutepov et al at AGU Joint Assembly) about the correction of SABER temperatures in the mesosphere/lower thermosphere for latitudes higher than 45 degrees in summertime. Approximately 15–20% of measurements are subject to the change that will affect the temperatures at the altitudes contributing to the MIPAS signal through limb geometry using. I think that

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one has either revise the conclusions from this point of view or specify the times and latitudes used for the comparison.

line 10: please, check the second reference ([Mertens et al, 2006]) in this respect.

lines 19-25: It seems appropriate to give some estimates and a reference here. The microwindows for ozone retrievals from MIPAS measurements have been studied in the work of [Manuilova et al, 1998] where the estimates for the altitudes of non-LTE effects have been done (≈ 65 km for all microwindows).

P. 6555, line 13: reference for previous studies of the chemical excitation rate of the $\text{NO}_2 \nu_3$ states is required. A brief comment on why the rate appeared to be 50 (!) times lower would be helpful, too.

Typos, style, etc

Please, select either British or American spelling and check the whole text against the chosen pattern in order to avoid the mixing like that on P. 6529 line 3 – "Optimised", line 7 – "optimized".

P. 6558, line 15: though the "seed" is widely used, it remains being slang. Use something more common like "the starting number", or rephrase like "the same AKMs should be obtained regardless of the way...".

P. 6540, line 21: "avoid" → "avoids"

P. 6541, line 8: "istance" → "instance"

P. 6557, line 8: "concides" → "coincides"

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