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Atmospheric
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Discussions

Interactive comment on “MIPAS database: Validation of HNO_3 line parameters using MIPAS satellite measurements” *by* J.-M. Flaud et al.

J.-M. Flaud et al.

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Here are the author replies to comments of reviewers #1 and #3. The replies are submitted by the corresponding author on behalf of all the co-authors and are reported here below within reviewer comments. Author replies begin with the label “**AC:**” and end with the sign “======”.

1 Comments of Reviewer #1

General: The paper describes choices made to improve the spectroscopic data of HNO3 used for MIPAS/Envisat level-2 analysis. The new database is validated by comparison of forward modelled spectra on basis of the MIPAS PF3.1 and the new MIPAS PF3.2 database with MIPAS observations. Such a paper is important to characterize a large set of atmospheric measurements. It helps the community to avoid misunderstanding during validation activities and scientific interpretation of the data and to further develop spectral databases for different instruments. The first part of the manuscript - the description of choices leading to the new database - is clearly structured. However, there are still parts which need clarification (see below). In the second part - the validation of the choices by use of measured spectra - new results are presented. However, though containing the central part of the manuscript (see paper caption) it is very short and rather weak (details are given below). I would strongly recommend that the authors elaborate this section by a more detailed description of their calculations and by performing some additional retrievals/forward calculations which in my opinion are necessary to convince the reader of the advantages of the new spectroscopic dataset.

AC: ok, see below the replies to the specific comments ...

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Specific:

p. 4255, l. 11: “On the average these new line intensities are weaker than those in the HITRAN2K linelist: INTHIT2K/INTNEW $\approx 1.13 \pm 0.06$”.

$->$ In the abstract of Toth et al., 2003 a value of 1.14 ± 0.06 is given. What is correct ? Which value was used for MIPAS PF3.1 ?

AC: The exact factor is 1.136 ± 0.06. This will be changed in the text.

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p.4256, l. 1: “following value: 2.31(23)”

$->$ Could you specify which entries from Table 3 you use? I cannot reconstruct this number. What does the number in brackets describe? How is this calculated from the Table?

AC: There has been a misprint in the quoted value which is the average of the results of Table 2 : It should read 2.30(28). The number in brackets is the RMS of the average. The text will be modified.

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p. 4256, l: 6-7: “As a consequence we retained the Toth et al. (2003) intensities at $11.2\mu\mathrm{m}$”

$->$ Does this mean that they are retained for MIPAS PF3.2? However, above it was stated that a rescaling on basis of the Toth data has also been done for MIPAS PF3.1, but in the first row of Table 3 there are different band intensities for MIPAS PF3.1 and MIPAS PF3.2. Shouldn’t they be equal in case the whole band intensity has been normalized to the Toth’s one? This should be made clearer in the text.

AC: The text will be modified. In fact Toth et al. have measured individual line intensities which have been fitted. Since the calculation of line intensities is covering a larger range of J and Ka values in MIPAS PF3.2 it is normal that the total band intensity is slightly larger.

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p. 4256, l. 11: “corrected taking into account the fact that at $11.2\mu m$ the intensities of

Toth et al. (2003) have been used.”

$- >$ Please explain this correction more explicitly.

AC: OK. The text will be modified.

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p. 4258, l. 4:

$- >$ Could you state here whether this model has been used for MIPAS PF3.2 air-broadening coefficients?

AC: YES. The text will be modified.

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p. 4258, l.18:

$- >$ Which version of MIPAS calibrated spectra have been used ? The reprocessed ones?

AC: YES, we used Level 1b reprocessed spectra V. 4.62, this will be specified in the text.

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p. 4258, l. 23: “This filtering led to the selection of 55 spectra with nominal tangent altitude of 12 km and 55 spectra with nominal tangent altitude of 24 km.”

$- >$ Why the same number at both altitudes? I would guess that more spectra are sorted out at 12 km than at 24 km?

AC: On the contrary, in general we would expect that more spectra are sorted out at 24 km than at 12 km, this is because lower altitudes are more likely to be affected by clouds. In any case we found the same number of spectra at these two altitudes because whenever we found a cloud-affected spectrum at altitude greater than or equal to 12km we discarded the full lim-scan of measurements in order to avoid using the related HNO3 profile, possibly retrieved with a large systematic error due to cloud contamination. A sentence will be included in the text to explain this concept.

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p. 4259, l. 14:

$->$ Here it is stated that HNO3 has been retrieved with the MIPAS off-line processor. It seems to me that it has been retrieved once (using MIPAS PF3.1) and that these results have been fixed for all forward model runs later. If this is correct: why have no retrievals been made with PF3.1 and with PF3.2 and comparing than these residuals ? I would suggest this approach such that the “best fits” can be compared.

$->$ Further, which spectral windows have been used for HNO3 retrieval ? From a mixture of the different spectral regions in Table 3 or only from 820-950 ? I would find it reasonable to perform retrievals in one region and then e.g. check for inter-band consistency.

AC: As suggested also by reviewer #3, comparing the best fit residuals is surely a worthwhile test and in the revised version of the manuscript we will show also how “best fit” residuals change when moving from the old to the new line database. However we think that the most crucial step for the assessment of spectroscopic data is still the comparison between observations and simulations carried-out with old and new spectroscopic data, and keeping the same HNO3 atmospheric distribution. This is

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because, if we change simultaneously both spectroscopic data and VMR profiles, there is the risk that changes in line strenghts are fully compensated by a change in the retrieved VMR and finally it is hard to judge whether the residuals are improved due to the changed VMR or due to the changed line strenghts. For this reason we plan to keep in the paper also the current type of comparison of residuals obtained with the same HNO3 VMR distribution retrieved using PF_3.1 line data.

The HNO3 VMR profiles have been retrieved using the following spectral intervals: 1: 876.375 - 879.375 cm^{-1} and 2: 885.1 - 888.1 cm^{-1}. These intervals will be specified for clarity in the revised version of the manuscript. It is certainly reasonable to perform retrievals in one region and to check for inter-band consistency, however the inter-band consistency check is affected by systematic errors which limit the accuracy of the validation as explained below. A few stetements will be included in the revised manuscript to make clear this point.

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p. 4259, l. 15:

$->$ The comparison in Table 4 is made over the spectral interval 840-930 cm-1 and this includes CFC-11 and CFC-12 as major contributors. In fact, in this wavenumber range these are the most important gases beside HNO3. Thus, I think it is necessary to retrieve also CFC-11 and CFC-12 individually to get reasonable comparisons, at least for the tangent height of 12 km but also for 24 km, since in the tropics there is a significant radiance contributing at these altitudes.

AC: We share the concerns of the reviewer here. In the revised version of the manuscript we plan to account for the residual contributions due to species other than HNO3 with the so called “residual and error correlation analysis” approach that is described on a different paper. This correction allows to remove from the actual observed residual spectrum all the known error contributions that do not arise from HNO3 line data errors or HNO3 VMR errors.

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p. 4260, l. 1 and Figs 1 and 2:

$->$ In these figures the same spectral ranges as in Table 4 should be shown such that the reader has the possibility to see at which wavelengths there are improvements.

AC: reporting in Figs 1 and 2 the same spectral ranges indicated in Table 4 would result in a very compressed scale from which it is hard to appreciate the improvements. Therefore in figs 1 and 2 we show only the spectral range in which the improvements in the residuals are the most important. The improvements achieved in the spectral regions not reported in the plots are quantified by the numbers reported in Table 4.

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$->$ Figures of the comparison should also be shown for the 24 km tangent altitude tests.

AC: Since in the paper we actually report the results only for 24 km tangent altitude, we assume that here the reviewer meant to say “12 km”.

A similar justification holds here as above: the improvements in the residuals that we obtain with the new line database are important, but not dramatically spectacular, therefore we prefer to show only plots relating to 24km that is the altitude at which the achieved improvements are more visible (because this is approximately the altitude of the maximum of the HNO3 VMR profile). Improvements achieved at 12 km are summarized by the numbers reported in Table 4. In the revised manuscript we will include a statement to better clarify this strategy.

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$- >$ There is large broadband residual offset in all cases shown. (Called “Average difference” in Table 4). This is much larger than the spectral noise of MIPAS (which is only given in the Figures, but should also been stated in Table 4.). The authors should discuss the reason for this offset in more detail (on p. 4260, l. 10 these are attributed to “other atmospheric species which are not perfectly modeled”. Can these species be identified regarding their large spectral contribution ?

AC: The large offset in the residuals is mainly due to deficiencies in modelling of gaseous continua, however this will be corrected for in the revised version of the manuscript, using the “residual and error correlation” technique mentioned above. The remaining uncorrected features of the residuals will be discussed in the revised manuscript. The values of the spectral noise will be also reported in Table 4.

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p. 4260, l. 3...:

$- >$ The authors state that it is not possible to make statements about the consistency of the new database in different spectral regions. However, would it not be possible to retrieve HNO3 from different spectral regions and compare the resulting HNO3 profiles as has been done by e.g. Boone and Bernath on basis of ACE-FTS data (shown in Fig. 3 of Rothman et al., 2005) for the HITRAN04 database ?

AC: the approach suggested by the reviewer is possible, however the ratio between the average HNO3 profiles retrieved from different spectral regions has a systematic error (see http://www.atm.ox.ac.uk/group/mipas/err/) larger than the possible small inter-band line database inconsistencies we are attmpting to assess. This is the reason why this approach was not used in our case.

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$->$ Finally, a comparison of HNO3 results between MIPAS PF3.1 and MIPAS PF3.2 using the spectral windows of the standard processor would be helpful for people interpreting the MIPAS level-2 dataset.

AC: this information is already available to the authors therefore, if considered relevant, it can easily be included in the manuscript. We will include a related plot with text description in the revised manuscript.

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Technical:

p. 4254, l. 14: “483 to 630 cm-1”

$->$ In Table 2 637 cm-1 is given.

AC: OK, this error will be corrected.

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p. 4255, l. 24: “Such a problem is not easy.”

$->$ I recommend to omit this conclusion.

AC: ok, this sentence will be removed.

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Table 2 and 3:

$->$ Could you state in the caption what the numbers in brackets mean? Errors?

AC: YES, the number in the brackets are the errors, this will be specified in the revised manuscript.

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p. 4259, l. 2-3: “(Ridolfi, M.: Accurate broadband forward model for MIPAS, private communication, 2004)”

$->$ redundant, since M. Ridolfi is an author of the actual paper.

AC: ok, this reference will be removed.

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2 Comment of Reviewer #3

The paper by J.-M. Flaud et al has two important aspects. The first (sec. 2,3,4) is the in depth critical review of knowledge of line parameters for HNO3 in the 8.3 and 11.2 micrometers. The second is the demonstration of how measurements of atmospheric spectra can help to select among contradictory data the most reliable set. The only observation I can see is that the VMR adopted for the computation is the same for both the “old” and “new” spectroscopic line parameters (pag 4259-12), while the change in line strength is expected to alter the VMR in the retrieval process inversely to the change of line strength (rule of thimb estitnate). A quick check on how this influences the fit residuals is probably worthwhile.

AC: OK, we agree, see also the above reply to a similar comment from reviewer #1. In the revised manuscript we will include a plot (with related text description) showing how the residuals improve when the simulated spectra make use simultaneously of both the new line data and the HNO3 VMR retrieved with the on-line processor spectral intervals and by exploiting the new line data.

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Interactive comment on Atmos. Chem. Phys. Discuss., 6, 4251, 2006.

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