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Atmospheric Measurement Techniques Discussions

Interactive comment on "Methane and nitrous oxide retrievals from MIPAS-ENVISAT" *by* J. Plieninger et al.

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Point to point response to the reviewers' comments

The authors like to thank the reviewers for the useful comments. In the following we give our replies. The reviewers comments are printed italic.

1 Comments of Referee 1

- **Comment 1:** *The abstract is missing details, I think. What is the time period covered by the new MIPAS data sets? What is the quality of the new data sets in terms of error estimates and resolution? It might be worthwhile to mention here how large the high bias of the old data products was to indicate that the new products provide significant improvements.*
- **Reply:** We agree to include the details regarding the time period, error estimates and resolution. To mention the quantity of the bias of earlier data products might be misleading since there is no validation for the direct predecessor of the data discussed here. See our reply to Comment 3.
- **Action:** We have added details regarding the time period, error estimates and resolution to the abstract.
- **Comment 2:** *p7807, l4: Which apodization was used?*
- **Reply:** We use a Norton-Beer Strong apodisation.
- **Action:** We have added this information in the revised version.
- **Comment 3:** *p7807, l19-p7808,l2: In order to help the reader judge if these high biases are important, I would suggest to add relative numbers. For example, 0.7 ppm for CH4 and 80 ppb for N2O in the lower part of the profiles correspond to ∼40% and ∼25% relative difference, right? After reading the introduction for the first time, I was wondering if these new MIPAS data are really needed, but these numbers point to large errors in the earlier data products, indeed.*
- **Reply:** We certainly can refer to those relative values. However, please keep in mind, that this particular study (Glatthor et al., 2005) does refer to far older MIPAS data and the bias for these was estimated versus some climatology and

did not result from validation efforts. Also only the highest values measured by MIPAS were used for those comparisons. In our current work we describe the improvements implemented in versions V5R_CH4_222-225 and V5R_N2O_222-225 over the versions V5R_CH4_220-221 and V5R_N2O_220-221 (see sect. 3, first paragraph). In the time since the work of (Glatthor et al., 2005) and to version 220/221, there may have been additional changes. Unfortunately, to our knowledge, there is no validation for data version 220-221. This is the reason why we cite all the available validation work of earlier versions.

- **Action:** We have added relative values of previous bias estimations.

- **Comment 4:** *p7810, l23-25: How did the CH4 and N2O spectroscopic data change in the HITRAN2008 update? The update seems to change the retrievals significantly, so it might be worthwhile to discuss this a bit*

- **Reply:** Since for CH4, the old MIPAS retrieval did already use HITRAN 2004, only updates introduced in HITRAN 2008 did affect the difference between our new retrieval and the old one. The line positions and their intensities have been updated for 12CH4 (Albert et al., 2009). For 13CH3D a new band was introduced based on a prediction by Ulenikov et al. (2000), using a method from Tarrago and Delaveau (1986). There are many additional changes in the data set, but they do not affect the spectral region of the MIPAS retrieval.

  The changes in the spectroscopic dataset of N2O when using HITRAN 2008 data instead of HITRAN 2000 with updates from 2001, were in fact already introduced in HITRAN 2004. The additional changes in HITRAN 2008 do not affect the MIPAS CH4 and N2O retrieval, because they are in different spectral regions. For HITRAN 2004 almost the entire N2O database had been recalculated (Rothman et al., 2005). The following can be found at Rothman et al. (2005): The positions and line intensities were taken from Toth. The air broadening coefficients have been gained by polynomial fits to the experimental results of Toth (2000), Lacome

et al. (1984) and Nemtchinov et al. (2003). The self broadening coefficients were obtained by polynomial fits to experimental data by Toth (1993). For the exponent of the temperature dependence for the half width of the air broadening the value 0.75 was used. This is in accordance to Lacome et al. (1984) and Nemtchinov et al. (2003). The air pressure shift was taken from Toth.

- **Action:** We have added the following explanation at the end of the first paragraph of section 3.1:

  For CH_4 all the differences in the spectroscopic dataset are described in (Rothman et al., 2009). For N_2O the updates in HITRAN 2008 are not affecting the spectral region of the MIPAS retrieval. In the HITRAN 2004 version, many updates were introduced of the N_2O spectroscopy over the HITRAN 2000/2001 data set which had previously been used by the MIPAS retrieval. Almost the entire dataset has been revised, details can be found in Rothman et al. (2005).

- **Comment 5:** *p7811, l19-22: The reference to Bardeen et al. (2008) points to a study on numerical simulations of meteoric dust in the mesosphere and upper stratosphere. Do you expect such particles to cause significant influences on the MIPAS radiances and do these events occur frequently so that a large number of MIPAS profiles would be affected?*

- **Reply:** We have evidence, that consideration of continuum emission above 30 km altitude improves the retrievals. Meteoric dust could be one possible explanation for a previously unconsidered source of continuum emission but this certainly does not exclude other possible sources. Also, in case the reference to Bardeen et al. (2008) might not be sufficient, we included an additional reference to Neely III et al. (2011) who point out, that there are aerosol layers above 30 km which cannot be neglected by remote sensing instruments.

- **Action:** We included a reference to Neely III et al. (2011).

- **Comment 6:** *p7812, l5: Do you mean "radiometric offset" instead of "spectral offset"?*
- **Reply:** Yes, thank you for pointing that out. Apologies, for using the wrong term.
- **Action:** We reworded as suggested.
- **Comment 7:** *p7812, l6-7: It is pointed out here that with the revised retrieval scheme 10403 instead of 10378 profiles converge. Is this improvement of 0.2% statistically significant?*
- **Reply:** First of all, if you look at the number of not converged retrievals, this number did drop by 41% (36 instead of formerly 61 retrievals with no convergence). This merely reflects the fact, that almost all retrievals converge anyway. On the other hand, any change in the retrieval setup could potentially lead to worse convergence. In our case this did not happen. We think that this is worthwhile to report, even if the observed improvement is not world-shattering.
- **Action:** None.
- **Comment 8:** *p7812, l7-8: Your assumption that the retrieved background radiation is probably related to aerosols needs further analysis, I think. Maybe the background signals are just due to calibration errors of the instrument?*
- **Reply:** Calibration errors are additive while background radiation is propagated through the entire radiative transfer formalism. Thus they are not equivalent. The old retrieval scheme tried to reproduce the effect by adjusting the radiometric offset, but this calibration correction was much less successful than the approach to treat the signal as atmospheric background emissions.
- **Action:** None.

- **Comment 9:** *p7814, l4-7: Perhaps be a bit more specific and provide numbers here? How much did the RMS and DFS actually change?*
- **Reply:** RMS did drop by 6.0%, DOFs increased by 2.3% for CH4 and dropped by 2.7% for N2O.
- **Action:** Explicit numbers are given as suggested.
- **Comment 10:** *p7815, l3-11: Can you provide an explanation for the reduced vertical resolution in the UT/LS? Figure 9 indicates that the vertical resolution decreases to ~7 km for CH4 and ~10 km for N2O in the UT/LS, which is much coarser than the vertical sampling and field-of-view of the instrument. This reduced vertical resolution might be a drawback for scientific studies, I think. Why is the vertical resolution at mid latitudes better than at tropical or polar latitudes?*
- **Reply:** Both the tropopause and the polar winter stratosphere are particularly cold which reduces the signal. The Jacobians scales with the Planck function.
- **Action:** We mention this explanation at the end of the second paragraph of section 4.
- **Comment 11:** *p7815, l24-28: I am not sure if the additional plots of the columns of the averaging kernel matrix provide much extra value. The integral value of the averaging kernels (rows) would be more interesting to add, providing a measure of measurement information versus a priori information in the results.*
- **Reply:** We agree, that the showing of the columns is somehow redundant. We removed them for simplicity's sake. We also included the integral values of the ak rows.
- **Action:** Removed the plots of the columns of the ak and added the integral values to the plots of the ak rows. We changed the text accordingly.

- **Comment 12:** *p7816, l8-11: Are there references discussing these instrument errors?*
- **Reply:** References were included.
- **Action:** We added references as suggested.
- **Comment 13:** *p7816, l25-p7817,l11: This paragraph is presenting the retrieval errors for a mid-latitude profile. Are the results for other latitudes/seasons similar?*
- **Reply:** We agree, that a further investigation of this point is needed.
- **Action:** We have calculated error budgets for further profiles and compared them to the error estimations already presented in the manuscript. We have calculated the extended errors for further mid-latitude (north and south, day and night) as well as for polar summer and winter and for tropical (day and night) profiles.
- **Comment 14:** *p7817, l15-18: I am surprised that the vertical resolution estimates for the full resolution retrievals (Fig. 13) vary so largely. In the UT/LS they alternate between 6-7 km and 12 km for adjacent altitudes (on a 1 km grid)? Is this related to the method that was used to estimate the resolution? Are these FWHMs of the averaging kernels or are the estimates based on concept of information density (Purser and Huang, 1993)?*
- **Reply:** The resolutions are FWHMs calculated from the rows of the averaging kernels. The large oscillations in the resolution between adjacent retrieval altitudes reflect the fact that the retrieval grid is finer than the tangent altitude spacing. If a retrieval grid point is close to a tangent altitude, then its resolution is higher, if it is further away, the resolution at that retrieval altitude is lower. The effect on the mean resolution plots is probably more pronounced in case of the full spectral resolution measurements, because here the tangent altitude spacing is more or less fixed. For the low resolution period it is a function of the latitude and

our plots are binned, so that the mean resolution of geolocations with a slightly different tangent altitude spacing are taken, which should smear the effect.

- **Action:** We have given an explanation how the resolution is obtained.
- **Comment 15:** *p7818, l7-11: Strictly speaking, if the new data products are really "improved" can only be demonstrated by validation with other/external instruments. However, it certainly looks like a step forward.*
- **Reply:** A validation paper of the new product is close to submission.
- **Action:** None.

2 Comments of Referee 2

- **Comment 1:** *The MIPAS mission identified as "reduced resolution" by the authors has been called by ESA "optimised resolution mission". The name "optimised" comes from the fact that, despite the spectral resolution of the instrument has been reduced due to instrumental problems, the spatial resolution of the measurements has been improved as a result of the lower time used by MIPAS to acquire an interferogram. Therefore I strongly recommend the authors to adopt the conventional name.*
- **Reply:** We have published our reduced resolution data as "reduced resolution" data prior to the ESA recommendation and are reluctant to change the name of our data product. However we will add a footnote, that ESA call their related data product "optimised resolution product".
- **Action:** We have added a footnote, that ESA call their related data product "optimised resolution product".

- **Comment 2:** *The spectral resolution of MIPAS is given before and after apodization. Which kind of apodization do the authors refer to?*
- **Reply:** We use a Norton-Beer Strong apodisation.
- **Action:** We supplied this information in the revised version.
- **Comment 3:** *Page 7808 – line 15-16: 'The data sets of these versions are disjoint in a sense that one observation is either 224 or 225.' What does this sentence mean?*
- **Reply:** It means, that for each measurement scan we provide one derived profile which was either calculated using setup 224 *or* setup 225. In other words: for one level-1 product there exists either 224 or a 225 product, but not both.
- **Action:** We added a sentence explaining this for clarification.
- **Comment 4:** *page 7809 line 1-2: Are the tangent altitudes used in the CH4-N2O retrievals coming from previous retrieval steps as well?*
- **Reply:** This is correct. Thank you for pointing that out.
- **Action:** We added this information in the revised version.
- **Comment 5:** *page 7809 line 17: The a-priori profile is zero, as stated at line 12 of the same page, therefore the diagonal element of the regularization matrix artificially pulls the profiles toward zero.*
- **Reply:** This is correct.
- **Action:** We mention this explicitly in the text in the revised version.

- **Comment 6:** *Why the retrieval setup has been tested for the Optimised Resolution on 110 orbits in summer (north hemisphere) and for the full resolution on 16 orbits only and in winter (North hemisphere)? A part from the highly reduced statistics that you have for the Full Resolution mission, the use of data from one season only may not represent completely the real behaviour of your retrieval setup.*
- **Reply:** We do not develop an empirical inductive correction to our data but we aim at an improvement based on sensitivity studies reflecting causal relations. Thus the statistical aspect should not be over-exaggerated. A validation of the entire dataset has been performed and is close to submission.
- **Action:** None.
- **Comment 7:** *page 7811 lines 12-15: Which spectroscopic parameters have been changed? Line strengths, broadening coefficients, line positions or all of them? Are the changes for both N2O and CH4 or the differences are due to CH4 only?*
- **Reply:** There have been changes in line strengths, broadening coefficients as well as in the line positions. The changes in the retrieved profiles are due to differences in both the CH4 and N2O spectroscopic dataset. The changes in CH4 profiles are mostly due to the changes in the CH4 spectroscopy, and to a smaller extent to changes in N2O spectroscopy. The changes in N2O profiles are mostly due to the changes in the N2O spectroscopy, but the larger changes at the very lower boundary in southern latitudes are due to the CH4 spectroscopy.
- **Action:** We mention the separate dependency of the changes in the N2O of CH4 profiles on the spectroscopic changes in the datasets for both gases in the revised version.

- **Comment 8:** *Page 7812 lines 9-10: What does it mean that the degrees of freedom 'increase slightly'? please quantify.*
- **Reply:** For CH4: by 3.9 %, for N2O: by 5.9 %.
- **Action:** We included those values in the revised version.
- **Comment 9:** *Page 7812 lines 12-16: Actually in the tropics the CH4 VMR difference oscillates from negative to positive to negative and then positive, similar behaviour shown for N2O. For both gases the differences are in phase, that means that continuum accounts for part of CH4 and N2O signal. Do you regularize the continuum to zero value as well? I don't understand why the retrieval of continuum above 30 km lowers the VMR below 20 km and enhance the DOF of the VMR profiles. Is it because before the offset was altitude dependent and therefore you had correlations between offset and continuum in the altitude range where you retrieved both?*
- **Reply:** First of all, the comment seems to indicate a misunderstanding. We indeed use a zero a priori for the continuum and the gases. However our regularisation matrix has a diagonal entry only for the grid-points above 70 km (and only for CH4). Thus the retrieval does not push the entire profile towards zero but only towards a smooth profile which in the case of CH4 is nailed to zero only for the top of the model atmosphere (above 70 km).

  Regarding the referees analysis: we think that this is basically correct. The ill-posed problem with compensating high radiometric offset and negative continuum contributions and vice versa also effected the profiles in the old setup. An oscillating artefact in the profiles hence is likely. In solving this in the new version, the differences between new and old version make these issues visible.

  To explain that there are differences below 32 km, there are several effects: First, the radiometric offset was set from altitude dependent to constant, which is di-

rectly affecting the retrieval in lower altitudes as well. Especially the ill-posedness described in p7811, line 23 ff, did affect the values of CH4 and N2O. Solving this did of course affect the values. Secondly, the Tikhonov approach works as a smoothing regularisation which could also explain, that changes in higher altitudes might have an effect on lower altitudes. Also higher order compensations could lead to these changes. If, for example, the continuum change above 32 km leads to a change in CH4 in these altitudes, this might affect the retrieved CH4 below, because the path of the radiation emitted in these lower altitudes leads through the higher altitudes, hence the spectra even from lower altitudes still depend on the atmospheric state of higher altitudes as well.

- **Action:** None.
- **Comment 10:** *Page 7813 lines 6-14: From what I read here, it looks like the authors have applied changes to the retrieval setup only if they produce results that are lower below 20 km. Isn't it a little like to force the results to be in agreement with other data?*
- **Reply:** We do not quite see, how such a general accuse can be inferred from such an action where we weaken instead of strengthen the prior constraint used. However, of course we know a priori that stratospheric CH4 and N2O cannot be higher than related tropospheric values. Thus we consider tropospheric values as a valid upper bound and this is our motivation for changes and our final criterion. We do not push our results towards other stratospheric measurements as an end in itself. Further we do not "calibrate" our data, but try to find the physical mechanisms which cause a better agreement with our a priori knowledge.
- **Action:** None.
- **Comment 11:** *Page 7813 lines 20-24: Can you explain which criterion have been used to select the new MWs? Were they selected because of the presence of*

water lines or because the lines of CH4 and N2O did not saturate? From Figure 6 it looks like the number of spectral points used below 20 km has been reduced drastically. What about the retrieval error of the altitudes below 20 km? To me both error and vertical resolution should deteriorate in the new retrieval.

- **Reply:** The selection of the MWs has been done by visual inspection. The aim was to avoid regions where the emission was saturated, hence the reduction of the microwindows at lower altitudes towards higher wavenumbers. Also the introduction of water vapour as a joint fit gas made it necessary to select additional regions where larger water lines were present.

  The noise error of the new setup did drop for CH4, and increased for N2O (please see our reply to Comment 14). The degrees of freedom in the setup CH4_V5R_224/225 (with the new microwindows) are 2.5% smaller than in the previous setup CH4_V5R_222/223 (still using the old microwindows). For N2O the dof dropped by 1 %. Since the vertical extension is the same, the resolution drop should roughly be of the same magnitude. However, the vertical resolution in the UTLS indeed seems to be reduced.

- **Action:** None.

- **Comment 12:** *Page 7815 line 4: Since the vertical steps of MIPAS measurements ranges from 1.5 to 3 km I would not consider a vertical resolution of 7 km a good resolution*

- **Reply:** To avoid quibbling about words we have removed the word “good”. However, the 7 km are only the upper bound of the range.

- **Action:** Removed the word “good”.

- **Comment 13:** *Page 7815 line 12-15: Figure 10 and 11 report not all the AK but a subset of them, since, if I understood correctly, your retrieval was performed at 1 km vertical steps*

- **Reply:** That is correct and we did this (using every 3rd column/row) to avoid overloading the figures. For clarity we pointed this out in the text of the new manuscript in addition to the annotation of the figures, where we already had mentioned this.

- **Action:** Pointed out in the text, that the shown AK columns/rows are a subset of the entire AK.

- **Comment 14:** *Page 7816: You cannot extrapolate the errors obtained for just one scan to the full mission. I suggest to evaluate them for few orbits considering all seasons. From figure 12 it looks like that the noise error is larger than the systematic error at low altitudes. Is this caused by the small number of spectral points used in this altitude range?*

- **Reply:** We agree, that a further investigation of this point is needed. Hence we calculated error budgets for further profiles and compared them to the error estimations already presented in the manuscript. However the reviewer’s suggestion to calculate the error budget for entire orbits unfortunately is not possible, since it would be too time and cpu consuming.

  Regarding the referee‘s comment to figure 12: we think this is an interesting observation. We examined this by looking at the mean noise errors of the previous version (CH4_V5R_222, N2O_V5R_222) and compared with the new version (CH4_V5R_224, N2O_V5R_224), each for the test dataset. For N2O, the mean errors below 20 km were in fact larger (about 10 %), but for CH4 they were smaller below 10 km (about 7%) and about the same size between 10 and 20 km. Hence this thesis is not supported; if it was the CH4 the errors would be expected to be larger in the new version as well, or should at least not become smaller. However, these small changes in the noise error could be explained by the changes in the microwindows. On the other hand, they could also be caused by changes in the regularisation or by the joint fitting of H2O and HNO3.

- **Action:** We calculate the extended errors for further mid-latitude (north and south, day and night) as well as for polar summer and winter and for tropical (day and night) profiles. We discuss the variability of these error budgets in the revised version.

- **Comment 15:** *Page 7817 lines 12-24: Very few details are given for the Full Resolution retrieval setup. Which MWs have been used? Are they the same as for the Optimised Resolution? Are you using the same regularization matrix? Do you retrieve offset and continuum as for OR?*

- **Reply:** As described in p7810, third paragraph, the same setup including offset, continuum and regularisation matrix was used. Only the MWs have been adjusted.

- **Action:** None.

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