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Atmospheric Measurement Techniques Discussions

# *Interactive comment on* "Validation of revised methane and nitrous oxide profiles 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 their kind and helpful comments. Based on their suggestions we have made an updated version of our manuscript. In the following we give our replies. The reviewers comments are printed in italic face.



## 1 Comments of Referee 1

- **Comment 1:** *...my main suggestion is that it should be made clear early in this study how it fits in with other similar work which has been submitted by this group. There is a recent Plieninger et al. [2015 AMT] which, as I understand it, introduces the new retrieval and which is appropriately discussed early in this work. But there is also a reference to a recently accepted Laeng et al. [2015 AMT] which only covers CH4 and is apparently based on an older retrieval version which should certainly be referenced well before the conclusion. Are the averaging kernel methods used here the same as in that manuscript? If not, is it possible that the differences noted in the conclusion are the result of the different comparison methods and not the result of changing versions? Indeed, it seems strange to mention in the conclusion that the bias between MIPAS CH4 and other datasets is reduced by 0.08 ppmv when compared with the validations performed in Laeng et al., but there seems to be no manuscript which shows a direct comparison between the two retrieval versions (is this true?)*

- **Reply:** We agree to give a short discussion of Laeng et al. (2015) and their findings, and give a reference to the descriptions of the profile versions discussed in their paper. The Laeng et al. (2015) paper indeed refers to an earlier data version.

  Regarding to the averaging kernel approach: Laeng et al. (2015) do present comparisons without the applications of the AK. They did a sensitivity study to assess if the application of the AK was needed, where they found the influence of the vertical resolution on the profiles to be negligible. In our study, however, also $N_2O$ is validated, and here the application of the averaging kernel turned out to be necessary. For reasons of consistency, we have applied the AKs also to $CH_4$.

  However, since both their and our study come to the conclusion that for $CH_4$ from the reduced resolution period the application of the AKs to the reference instruments does not change the profiles significantly, the noted differences cannot be attributed to the different approaches.

The differences of the two retrieval versions CH4_V5R_222/223 (discussed in (Laeng et al., 2015)) and CH4_V5R_224/225 (discussed in our work) are discussed in detail in Plieninger et al. (2015). They present the influences of individual changes from versions CH4_V5R_222/223 to CH4_V5R_224/225 applied to the retrieval setup in Chapters 3.3 to 3.5. These individual differences are plotted in their Figs. 5, 7 and 8. All these changes mainly lead to lower values in the upper troposphere and lower stratosphere.

Here, in Figs. 1 to 3, the comparisons to ACE-FTS, HALOE and SCIAMACHY are shown for the V5R_CH4_222 and V5R_CH4_223 dataset as already validated in Laeng et al. (2015). The comparison approach is the same as described in our current study, including the application of the MIPAS AK to the profiles measured by ACE-FTS and HALOE. Since the differences to the reference instruments are larger than with the new datasets in all cases, this supports our statements that the new datasets have a smaller bias than the old datasets.

- **Action:** Added a few words about Laeng et al. (2015):

  "A comparison of MIPAS IMK/IAA $CH_4$ to profiles measured by other instruments can be found in Laeng et al. (2015). They discuss data versions CH4_V5R_222 and CH4_V5R_223 which cover the MIPAS reduced resolution period only. These versions are the direct predecessors of the CH4_V5R_224/CH4_V5R_225 versions under discussion in this work. The retrieval setup of versions CH4_V5R_222 and CH4_V5R_223 can be found in Plieninger et al. (2015). Laeng et al. (2015) found the MIPAS $CH_4$ profiles below 20 to 25 km to be biased high and give 14% as the most likely value."

- **Comment 2:** *Page 5 – "MIPAS retrieval uses a zero a priori". First, this is a bit*



*unusual. Is there some reference explaining why this was done? Is it true for all MIPAS tracer retrievals (it is clearly not true for temperature, based on statements earlier in the manuscript), or only for these particular species, or only for these particular retrieval versions? Also, perhaps it would be better phrased as ". . . the a priori is set equal to zero".*

- **Reply:** The choice of a zero a priori profile must be seen in the light of the Tikhonov first order regularisation. It pulls the vertical gradient ($\frac{dvmr}{dz}$) of the retrieved profile towards the gradient of the a priori profile. If the latter is constant (in our case all zero), the a priori profile has a zero gradient, and the retrieved profile will be smoothed by the regularisation. It will not be pulled towards the absolute values of the a priori profile itself, because the absolute values are in the null space of the regularisation matrix.

  This approach is in use for almost all MIPAS ENVISAT gas retrievals with the IMK/IAA processor. However, for the sake of completeness it must be stated, that in the version of the $CH_4$ retrieval under discussion here, we introduced some diagonal elements to the regularisation matrix, which in addition to the mechanism described above, actually do pull the profile towards zero. This affects altitudes from 70 km upwards only. It was introduced to limit negative values which turn up above that altitude, even in mean profiles. For further details please see Sect. 3.3 in Plieninger et al. (2015).

- **Action:** We added a short description of the regularisation of the retrieval:

  "A Tikhonov first-order finite differences constraint in combination with an all zero a priori profile is used. This serves to smooth the retrieved profile, instead of pulling it towards the a priori profile itself (von Clarmann et al., 2009). For $CH_4$ there are additional diagonal elements in the regularisation matrix for altitudes at 70 km and above (these are altitudes above the highest tangent altitude), where the profile is hence pulled towards zero."

- **Comment 3:** *In the next paragraph: "neglected elements below 0.001". It's not clear to me, based on statements later in the paragraph, whether negative terms with a larger absolute value than this are included or not (they certainly need to be). Would it be correct to write "neglected elements with absolute values below 0.001"? "lower most" should be "lowermost".*

- **Reply:** This comment seems to indicate a misunderstanding. We are afraid that our choice of words "neglected elements below 0.001" was misleading. What we meant is described in the following sentences of the paragraph.

  The application of the averaging kernel involves a sum where each summand is the product of the value of the averaging kernel and the reference value at the respective altitude. However, at altitudes represented by the far upper and lower tail of the averaging kernel (covering the entire MIPAS retrieval grid from 0 to 120 km) data of the reference instrument are not always available. This was considered acceptable if the respective value of the AK was smaller than 0.001, because the contribution of this summand to the sum then is negligibly small.

  The threshold was not on the absolute, hence negative values actually were neglected in the sense stated above. However, as explained, it is not a hard neglection, since if valid data points are available the calculations are carried out (using those negative AK elements).

  We have tried different thresholds and also tried it on the absolute of the AK elements. However, we noticed only changes in the amount of successful calculations (reflected by the number of data points in the mean profiles), not in the profile values themselves. If we use the same threshold 0.001 and apply it to the absolute AK values, we obtain fewer data points in the mean at altitudes at 30 km and above. Hence, in the new version we raised the threshold to 0.01, but applied it to the absolute numbers for the sake of consistency. This yields more data points in the mean, but as explained above alters neither the profiles nor their differences.



- **Action:** We applied the AK threshold as absolute values, raised the threshold to 0.01 and adjusted the plots and the text. We also changed the wording to avoid the term "neglect".

- **Comment 4:** *Page 6 – "In some cases this approach failed" – What does this mean? How can one "fail" to have a tropopause?*

- **Reply:** The temperature profiles retrieved from MIPAS ENVISAT including their a priori information (based on ECMWF in the altitude range of interest) were used for the determination of the tropopause. In particular in polar winter conditions the tropopause determination fails occasionally, yielding tropopause heights that are way too high. This is due to the special temperature distribution in this region and, for this particular purpose, relatively low resolved grid of the MIPAS temperature (i.e. 1 km). This problem could be solved by using temperature profiles or even tropopause information from other sources. However, since only a few measurements (~1%) were affected and the polar regions carry a small weight in our weighting function for the global mean, we think it would not be worth the effort.

- **Action:** We added the information that we use the MIPAS temperature profiles for the determination of the tropopause and gave a possible reason why in some cases the tropopause could not be found.

- **Comment 5:** *Page 8 – "...SCIAMACHY show, that" There should be no comma here.*

- **Reply:** Thank you for spotting.

- **Action:** We removed the comma as suggested.

## 2 Comments of Referee 2

- **Comment 1:** *Major issue: I found that the comparisons with the surface data added no value to the paper. I believe they should either be justified fully with a detailed explanation of the methodology and the expected errors associated with the comparison of upper tropospheric values or they should be removed completely. I feel the authors would not have presented the results had they not felt them valuable so I would prefer they demonstrated their value rather than removing them. However, I leave it to them.*

- **Reply:** Our motivation to include the surface measurements was to get comparisons to data which is on the one hand very precise compared to the satellite data ($CH_4$: 1.5 ppbv precision (Dlugokencky et al., 2009); $N_2O$: 0.2 ppbv precision (Hall et al., 2007)) and on the other hand uses a fully independent measurement principle. That means they do not rely on the knowledge of the correct radiative transfer, a priori information of e.g. pressure or temperature profiles or spectroscopic datasets. Any systematic error in these could lead to similar errors in all satellite instruments which might not show in differences between different satellite profiles.

  Possible errors of our method could be that the value measured by MIPAS 3 km below the tropopause is still influenced by stratospheric values and that the tropospheric profile is not sufficiently constant in altitude. Considering that there are no atmospheric sources for $CH_4$ or $N_2O$, the latter would mean that the actual concentrations in the upper troposphere should be lower than the surface values. Both errors would essentially lead to an overestimation of the satellite-derived concentration. So even if we cannot expect a perfect agreement between the MIPAS values and the surface concentrations, at least we would expect the satellite measurement to be equal or roughly lower than the surface values. Since this is not the case we suspect our measurement in the upper troposphere to be biased



  high.

- **Action:** We included statements of our motivation for the surface data comparisons, added a qualitative estimation about the errors of our method, and extended the discussion of the differences found between MIPAS and the surface data.

- **Comment 2:** *Page 12109 line 13: The description of the visibility flag needs another sentence or two to understand fully its usefulness.*

- **Reply:** The visibility flag serves to indicate, whether retrieved profiles at a certain altitude were actually based on measurements at that altitude. To exclude the retrieved values at those altitudes where this is not the case is sensible, since all the information which was used to obtain them must come from different altitudes. We agree to give a more detailed explanation.

- **Action:** We added a few explaining sentences: "...visibility flag is 1. The latter is a value which indicates for one profile point, whether the retrieval actually used measured data which was emitted at the altitude of this profile point. It is 0 if there are no spectra available either because the measured spectra in that altitude are influenced by emissions of clouds, or because there are no measurements available for that altitude. This is the case for all altitudes outside the MIPAS scan range, which is smaller than the range of the retrieval altitude grid."

- **Comment 3:** *Page 12113 line 20: "cites" goes to "sites"*

- **Reply:** Yes, thank you for spotting.

- **Action:** Correct spelling as suggested.

- **Comment 4:** *Page 12118 line 27: This comment goes towards the major issue. Is this technique valid? Does it have any heritage? What do these comparisons* *add to the characterisation of the data sets? How do I interpret differences in the comparisons? Etc..*

- **Reply:** We believe our method is valid. Atmospheric $CH_4$ has a lifetime of the order of 10 years (e.g. O'Connor et al., 2010), while tropospheric mixing is of the order of 10 days (e.g. Good et al., 2003), hence much shorter. To our knowledge there is no heritage for this method. The value of these comparison lies in their high precision and their independence from satellite measurements which all rely on similar a priori information as e.g. pressure and temperature profiles, spectroscopic data sets or correct descriptions of radiative transfer. In the light of the systematic errors, we would expect the surface values not to be lower than the MIPAS values 3 km below the tropopause. Since this is not the case, the MIPAS measurements at these altitudes are likely to be biased high.
- **Action:** Please see our actions to Comment 1.
- **Comment 5:** *Page 12120 line 3: Both profiles are clearly "not constant" between 10 and 13 km*
- **Reply:** We had meant to describe the lower part of the profiles, where the MIPAS profile is approximately constant, and the ACE-FTS profile has a slight decrease with altitude. We agree that our original wording lacked precision.
- **Action:** We removed this and replaced it: "Above around 13 to 14 km a strong negative vertical gradient is observed by both instruments, which becomes less pronounced above approximately 40 km."
- **Comment 6:** *Page 12120 line 5: The term "belly" shaped should be changed. The entire description of the profile shape needs to be cleaned up.*
- **Reply:** We agree to rewrite this paragraph.



- **Action:** We rewrote the paragraph:

  "The profiles of the two instruments agree quite well with respect to their shape and values. ACE-FTS shows a steady decline of mixing ratio with altitude over the entire profile. Above 12 km, this is the case for MIPAS as well. Between 9 and 12 km MIPAS shows a slight increase with altitude. Above around 13 to 14 km a strong negative vertical gradient is observed by both instruments, which becomes less pronounced above approximately 40 km. Between 15 and 46 km the MIPAS profile shows slightly higher values than ACE-FTS. Above that altitude the profiles agree well."
- **Comment 7:** *Page 12120 lines 8-9: I do not understand what the authors mean by "no longer be distinguished".*
- **Reply:** We meant that the two curves showing the mean MIPAS and ACE-FTS profiles in the plot are so close together, that they are actually above each other and hence can not be distinguished.
- **Action:** We changed our wording to: "Above that altitude the profiles agree well."
- **Comment 8:** *Page 12120 line 20: Why include an altitude with only one data point? There should be a minimum data point threshold for statistical comparisons. The inclusion of this point generates an outlier that needs to be mentioned and then discarded by the reader. It should just be discarded by the authors.*
- **Reply:** We agree to show only data points where 10 or more data points were available to calculate the mean.
- **Action:** We changed the comparisons, so that each mean profile point is calculated from at least 10 different profiles. We altered the text accordingly.
- **Comment 9:** *Page 12125: The text should refer to Figure 11 somewhere*

- **Reply:** Thank you for spotting.
- **Action:** Added reference to Figure 11 in paragraph discussing the MIPAS-SCIAMACHY comparison for the full resolution period.



## References

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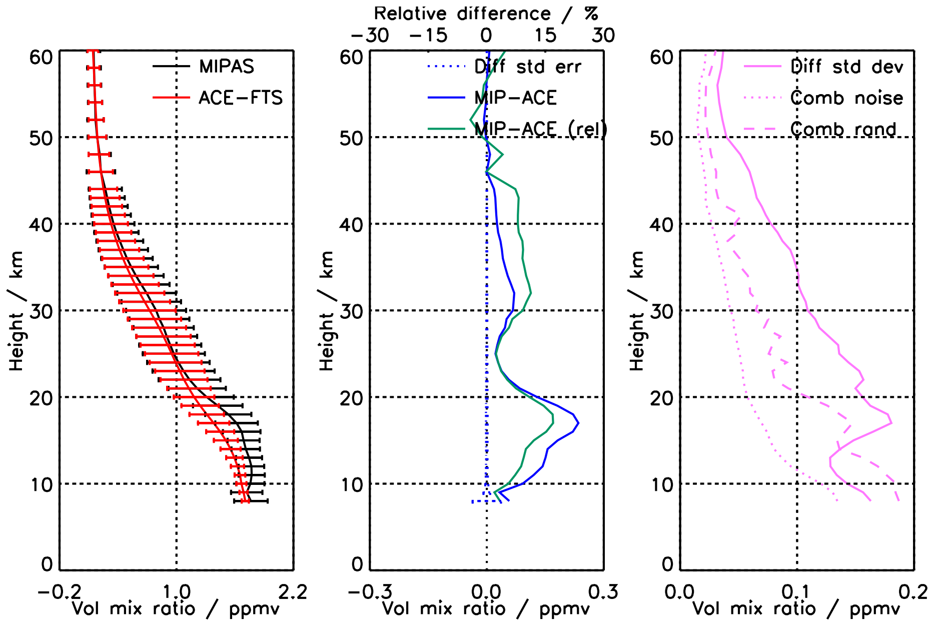


**Fig. 1.** MIPAS V5R_CH4_222/223 ACE-FTS comparison - details as in Fig. 1 of the manuscript



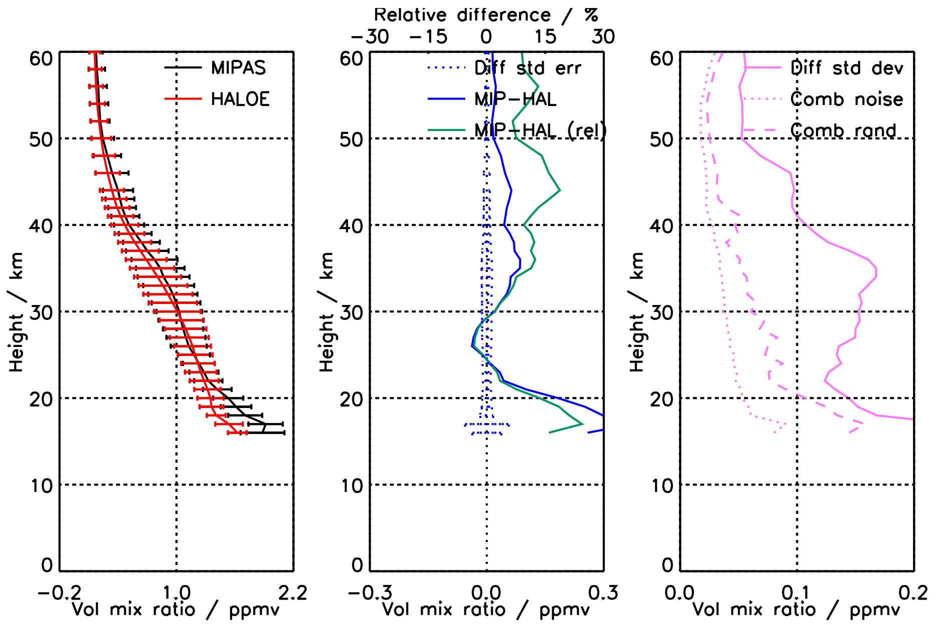


**Fig. 2.** MIPAS V5R_CH4_222/223 HALOE comparison - details as in Fig. 1 of the manuscript

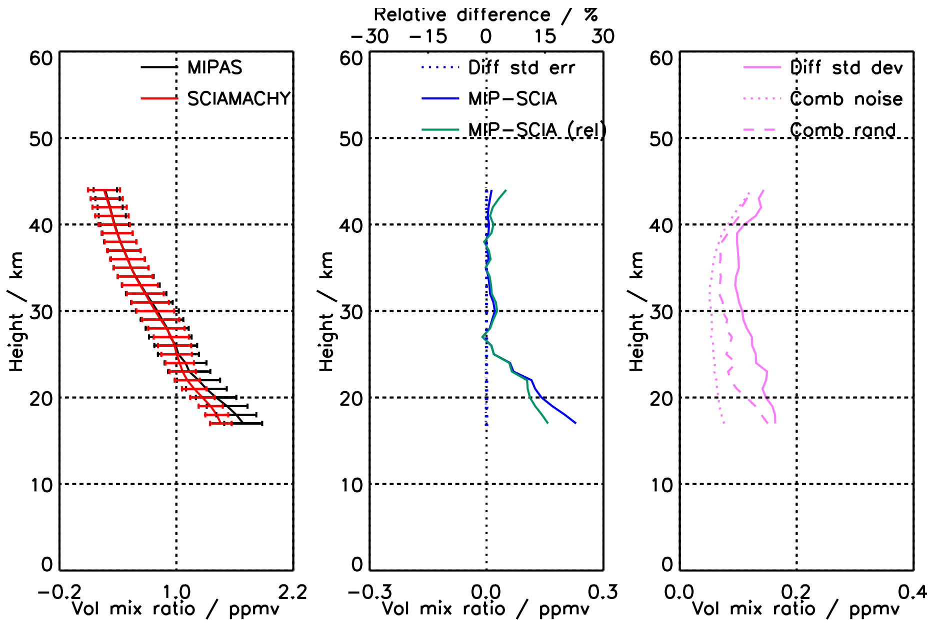


**Fig. 3.** MIPAS V5R_CH4_222/223 SCIAMACHY comparison - details as in Fig. 1 of the manuscript