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Atmospheric Measurement Techniques Discussions

Interactive comment on "Validation of water vapour profiles (version 13) retrieved by the IMK/IAA scientific retrieval processor based on full resolution spectra measured by MIPAS on board Envisat" *by* M. Milz et al.

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We would like to thank reviewer # 1 for the helpful and constructive comments.

Reviewer # 1 pointed out several inconsistencies between plots and text which were introduced during the writing process. In this period various plots had to be redone a few times as the number of used measurements permanently increased during the re-processing with the new retrieval setup (Version 13) and text and plots partly diverged.

The datasets available at IMK for the comparison was extended since the submission of

the manuscript to AMTD. For two of the instruments(ACE-FTS and MIAWARA), the new figures are now based on an increased number of collocations which means a broader basis for statistical analysis. However, the results with respect to bias and precision analysis and the according figures do not change noteworthy. Also the conclusions of the paper remain the same.

Please find below our answers addressing the specific reviewer's comments.

Reviewer Comments

General comments: The paper presents a very extensive validation of the water vapor profiles obtained with the newest version of the scientific retrieval processor at IMK/IAA. With no doubt the presented results are of great interest for MIPAS data users. The paper is good structured and the results are mostly well presented. However, a little bit work is still required to extend the algorithm description which in the present form of the manuscript is too sparse not explaining some important details. As pointed out below in the specific comments, the presentation of results is not always optimal, some of the plots are too small, the number of plots can be slightly reduced. It is also a pity that authors do not use color plots. Further issues are listed in the specific comments below. I think, my suggestions require rather a minor revision of the manuscript. Thereafter, the manuscript will certainly be suitable to be published in AMT.

Specific comments:

Reviewer comment:
1. Page 494, lines 7 to 9:
Please explain why the exact knowledge of the hydropause altitude is required to regularize the retrieval of the VMRs and not required to retrieve logarithms of VMRs. How does this lead to a better vertical resolution?

Answer:
The optimal regularization depends on the atmospheric state itself. Since variations of the hygropause cause large variations in humidity in a certain altitude, it is difficult to determine the actual (altitude-dependent) regularization strength in advance. In a log retrieval, the resolution affects relative rather than absolute changes in the atmospheric state, and the dependence on the atmospheric state thus is implicit and has not to be considered explicitly. Since in some cases unnecessary smoothing is avoided, the altitude resolution is improved. The text will be modified to make it clearer.

Reviewer comment:
2. Page 494, lines 25 to 26:
This is explained by non-local thermodynamic equilibrium emissions in some of the spectral regions selected in the new retrieval setup. - Please explain why an inclusion of spectral intervals with non-local thermodynamic equilibrium emissions is expected to improve the retrieval results. Otherwise, if no improvement is expected, explain the reasons to include these intervals into the retrieval.

Answer:
These lines had been selected because of their large sensitivity. The problem of non-local thermodynamic equilibrium emissions became evident only after the data set to be validated had already been analyzed. The text will be modified to address this unclear statement

Reviewer comment:
3. Page 495, Table 1:
Please explain the sign of the displacement.

Answer:
A positive sign represents displacement towards the satellite. A short note will be added to the caption of Table 1.

Reviewer comment:

4. Page 495, Sec. 3:
The description of the retrieval in Sec. 3 needs to be extended providing the formulas for A and G. According to the variable description presented in Sec. 3 the measurement vector x contains the water vapor profile (most probably in VMR but this is not stated here) which is in contradiction with the description in Sec. 2 where the logarithms of VMRs are stated to be retrieved. Assuming the description in Sec. 2 to be correct, authors have to explain how to rewrite Eq. 1 for ln x. Furthermore, authors should describe here how a priori and noise covariance matrices are set up.

Answer:
We retrieve x = log(VMR(H_2O)); with this definition Equation 1 remains unchanged. We add a clarification and the explanations for $\mathbf{G}$ and $\mathbf{A}$ to the text. Please Note that in our application the regularization term $\mathbf{R}$ replaces the inverse a priori covariance matrix $\mathbf{S}_a$.

Reviewer comment:
5. Pages 495 - 496, Sec. 4.1: Please explain why for satellite instruments a stronger coincidence criterion than for ground based measurements and non-satellite platforms is applied.

Answer:
Some of the ground based measurements are campaign based and have only few measurements (e.g. The LAUTLOS campaign). In order to obtain a sufficient number of coincidences the coincidence criterion was loosened. For reasons of consistency this criterion was applied to all ground based and non-satellite based observations

Reviewer comment:
6. Pages 496 - 497: the terms "statistical uncertainty of the bias" (as given by Eq. 3), "precision", and "de-biased mean squares difference" seem to have the same meaning. If it is true, please always use the same notation or explain the difference if not true.

Answer:

The "de-biased mean squares difference" is the estimate of the precision. We will reword the text to clarify this. We prefer to mention both terms, because the first is the usual one, while the second is rather a description than a name and contains information what the nature of precision is. The term "statistical uncertainty of the bias" is indeed a different quantity. It is the estimated random error of the bias. It is explained after Eq. 4. The major difference between these two quantities is: the precision does not depend on the number of comparison pairs but is a characteristics of the measurement. The statistical uncertainty of the bias converges towards zero when the number of comparison pairs increases towards infinity.

Reviewer comment:
7. Page 497, Eq. 4:

- **Reviewer comment:**
  What is the meaning of $\langle\rangle$?

  Answer:
  $\langle\rangle$ expresses the expectation value of the quantity inside the brackets, in this case $\langle\chi^2\rangle$ is the expectation value of χ^2.

- **Reviewer comment:**
  Formally, a dependence of χ^2 on n should be also indicated.

  Answer:
  The expectation value of χ^2 is the number of degrees of freedom, in this case K-1, as the debiased values are considered.

- **Reviewer comment:**
  From line 23 "calculated on basis of the number of measurements used for each altitude gridpoint" one can also assume that the number of comparison pairs, K, depends on the altitude. This should be stated more clearly in the text.

  Answer:
  The quantiles of the χ^2 distribution as a function of the number of degrees of freedom, here K-1, are calculated with a standard program library. The text is corrected accordingly.

- **Reviewer comment:**
  According to von Clarmann (2006) 2 should be equal to the number of the comparison pairs, i.e., K, why do you have K - 1 instead?

  Answer:
  Whenever the χ^2-test in von Clarmann (2006) involves correction by a bias which has been determined from the data themselves, also there the reduction of the number of degrees of freedom by one is considered (Eq. 54 or 56 in von Clarmann, 2006). It is not considered, when such kind of correction is not applied (Eq. 10 or 53 in von Clarmann, 2006). Our use is in agreement with this.

- **Reviewer comment:**
  The provided description does not describe the procedure for the validation of the precision. Authors should provide the formulas explaining how the quantities shown below in the validation sections are calculated.

  Answer:
  The formula for χ^2 is given. The confidence limits of the χ^2 function are not usually reported by an analytic formula but numerically calculated by integration of the respective χ^2 function of the respective degrees of freedom using a standard program library. A sentence addressing this has been inserted to the text.

- **Reviewer comment:**
  The description of the random error components is a bit misleading. First, all errors needed to be included are listed then authors present the components of the MIPAS random error. Other errors from the "needed" list are not discussed here.

These appear only far below in the text where the comparisons with other instruments are discussed. At this point the reader can get a wrong impression that only the MIPAS errors are accounted for in the above discussed total variance.

Answer:
In order not to lead the reader on wrong track, we insert on p 497, l.20: The error sources considered for the reference instruments varies from instrument to instrument and is reported in the respective section.

Reviewer comment:
8. Page 498, Eq. 5: The transformation to the common a priori information is questionable. At least for the instruments measuring the water vapor absorption, the retrieval problem is commonly non-linear. Thus the averaging kernels are changed during the iterative process. Thus, the result of this transformation does not necessary show what the reference instrument would produce if the same a priori information as in MIPAS were used, which is strictly true for linear problems only.

Answer:
The reviewer is right. We used the Averaging kernel of the last iteration. We will add a caveat mentioning the fact of the changing averaging kernel to the text.

Reviewer comment:
9. Page 498, Eq. 6: This equation does not seem to be used below. If so, there is no need to present it. If the smoothing error is used anywhere in the comparisons below this should be clearly stated in the corresponding sections. Furthermore, author have to mention where the climatological covariance for the water vapor is obtained from.

Answer:
The reviewer is right. The equation will be removed and the text modified in a way that it is clear to the reader that we are aware of this approach but didn't use it as the equation contains a term which is not available (the covariance of the climatology)

Reviewer comment:
10. Page 500, Fig. 3 and all χ^2-plots below:

- **Reviewer comment:**
  As pointed out above the testing procedure is not clear. An example 2 distribution and its 95% confidence interval has to be illustrated. Why is the 95% confidence interval altitude dependent? Most probably because the number of matching pairs changes with the altitude but this is never mentioned in the text.

  Answer:
  The reviewer is correct with the assumption that this is because the number of matching pairs changes with the altitude. This, however, is stated in line 23 on page 497 of the discussion paper.

- **Reviewer comment:**
  The axes have to be labeled

  Answer:
  Labels will be added to all plots

- **Reviewer comment:**
  What exactly is shown in the plot? From the description in the text one expects to see the result of Eq. 4 but this should be around K - 1 whereas the "95% confidence interval" surrounds an area around 1.

  Answer:
  We show the so-called "reduced χ^2" which is χ^2 divided by the number of degrees of freedom. The expectation value of this quantity is one. A clarification will be added to the text after Eq. 4.

Reviewer comment:
11. Page 500, line 24: "altitudes between 19 and 57 km and above 55 km" - this interval specification is a bit strange

Answer:
In accordance with the other two reviewers comments the sentence is corrected

Reviewer comment:
12. Page 501, line 11: "...indicates overestimation of the precision..." according to the terminology introduced in Sec 4, precision is characterized by the remaining difference between two sets of the measurements after the bias is subtracted. Thus, this is a value obtained from the comparison and it can be neither over- nor underestimated because it is a result of a calculation. Using the same words as for Fig. 3 one should say that the assumed errors are overestimated, not the (actual) precision.

Answer:
We disagree but we realize that that our original wording was not accurate: On page 496 we define "Precision is the reproducibility of a measurement, estimated as the bias-corrected root mean squares difference between MIPAS profiles and the true water vapour profiles." Since we do not know the true atmospheric state, nor a statistically significant set of independent measurements of the atmosphere in exactly the same state, we can only estimate the precision. This can be done either *ex ante*, i.e. by Gaussian error propagation through the retrieval, involving the so-called **estimated** standard deviation, or *ex post* by using the toolbox of **estimation** theory. χ^2-analysis basically tests the consistence of the *ex ante* and *ex post* estimates. Too large or small χ^2 values thus indicate, that the *ex ante* estimates of the precision are significantly larger or smaller than the actual precision, i.e., the actual precision had been under- or overestimated. We don't see anything wrong with this use of terminology. We prefer 'precision' here because it is more specific that 'errors' in a sense that it excludes systematic errors.

Reviewer comment:
13. Page 501, line 12: "For altitudes above 48 km and below 21 km the assumed errors appear to be realistic." - this contradicts the previous sentence and does not agree with the results shown in Fig. 5.

Answer:
This sentence was obsolete and will be removed

Reviewer comment:
14. Page 504, Fig. 8/10 and 9/11 and all similar plots below: The results for smoothed and unsmoothed profiles would be better comparable if they were shown in one plot. I suggest to join figures 8 and 10 as well as 9 and 11 using lines of different colors for smoothed and unsmoothed profiles.

Answer:
We modified most of the plots including labels, clearly readable legends and colors. For cases where smoothing was applied, the according plots with untreated and smoothed profiles and comparisons have been combined to one plot.

Reviewer comment:
15. Page 504, lines 21-22: "...suggests underestimated random errors or disregarded error sources" - of MIPAS or ILAS?

Answer:
We clarify this sentence by adding "... for one of the instruments compared or due to less than perfect co-incidences."

Reviewer comment:
16. Page 505, lines 3-4: "Between 15km and 40km both instruments agree very well with a relative bias below 3%, which lacks significance." - at 40 km the measurements disagree by at least 15%. May be you mean 26 km as the upper limit?

Answer:
In accordance with the other two reviewers comments the sentence is corrected.

Reviewer comment:
17. Section 5.1.3: At the beginning of the subsection you write that the comparisons with ILAS-II were already performed using a precursor version of the MIPAS retrieval.

At the end of the section you should provide a short remark how the new results compare to the previous studies.

Answer:
The reviewer is right. A remark comparing the two studies will be added.

Reviewer comment:
18. Page 507 lines 1 and 17 - 19: The following statements are in contradiction. "The maximum mean difference is approximately 0.75 ppmv (15%) at around 23 km" (for unsmoothed profiles) and "The prominent bump at 23 km is weaker... The maximum difference of 1 ppmv or 20% is found at 23 km."(for smoothed profiles). The bump is weaker but the maximum difference is larger, something is wrong here.

Answer:
In accordance with reviewer # 3 the sentence is corrected.

Reviewer comment:
19. Page 510, Fig. 28: the figure would be better readable if it was plotted in color. Axes have to be labeled.

Answer:
As mentioned above, most plots are modified for better appearance

Reviewer comment:
20. Page 510, line 20: "For the southward viewing scan, additionally the PV values at reference altitudes were considered..." - in which sense were the PV values considered? What this information was used for?

Answer:
PV values from the ECMWF reanalysis for each measurement geolocation were compared on different levels of potential temperature. For cases where two or more MIPAS-E measurements fulfill the coincidence criterion to one measurement of a reference intrument the PV values at certain reference levels were compared. The measurements

with the smallest differences in PV at most altitudes were compared.

Reviewer comment:
21. Page 511, line 1-2: "...for altitudes where similar airmasses were observed according to the PV." - which altitudes are these? Please, indicate these altitudes in the plots.

Answer:
The potential temperature levels and the according PVU values will be named in the text together with the estimated altitude of the according levels

Reviewer comment:
22. Page 511, line 6: "...the two closest coincidences agree very well for altitudes below 25 km." - actually it is rather 21 - 23 km, e.g., in the right panel of Fig. 29 the profiles are quite different already at 22 - 23 km.

Answer:
The reviewer is right. The text will be modified.

Reviewer comment:
23. Pages 510 - 511, Fig. 28 and 29: The PV values have to be provided to support the discussion.

Answer:
The PVU values and the relevant altitudes will be given in the text.

Reviewer comment:
24. Pages 511 - 512, section 5.3.2: please discuss shortly the comparison results obtained in previous publications (Müller et al., 2008; Feist et al., 2007) and how these compare to the new results.

Answer:
The reviewer is right. A remark addressing these studies will be added.

Reviewer comment:
25. Page 512, lines 4 - 5: the statements "inside the range of the confidence limits for all altitudes" and "slightly smaller than the lower confidence limit for all altitude between 15 an 50 km" are contradictory. In the second statement "altitude" ! "altitudes"

Answer:
The text will be modified.

Reviewer comment:
26. Page 512, lines 12 - 13: "This may indicate that the smoothed error budget for MIPAS is overestimating the total random error." - Why for MIPAS and not for AMSOS?

Answer:
We will reword the sentence as follows: This may indicate that the combined random error budget for the smoothed MIPAS and the untreated AMSOS profiles is overestimating the total random error.

Reviewer comment:
Does "smoothed error budget" mean random error budget for smoothed profiles?

Answer:
Yes. The text will be modified accordingly

Reviewer comment:
27. Page 513, Fig. 34: The plots are unreadable. Rearrange the plots showing 3 panels horizontally and 2 panels vertically, enlarge the panels, plot in color. Axes have to be labeled. Legends must not be covered by the curves.

Answer:
The Plots will be redone and rearranged.

Reviewer comment:
28. Page 513, Fig. 35: Plot would be better readable if it was plotted in color. Axes have to be labeled. Legends must not be covered by the curves.

Answer:
The Plots will be redone

Reviewer comment:
29. Page 513, Fig. 36: Rearrange and enlarge the panels, the plot would be better readable if it was plotted in color. Axes have to be labeled. Legends must not be covered by the curves.

Answer:
The Plots will be redone

Technical corrections:

Reviewer comment:
Page 500, line 29: word "indicating" occurs twice in "...limit indicating indicating that..."

Answer:
one "indicating" will be removed

Interactive comment on Atmos. Meas. Tech. Discuss., 2, 489, 2009.