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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Tropospheric ozone from IASI: comparison of different inversion algorithms and validation with ozone sondes in the northern middle latitudes" *by* C. Keim et al.

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The authors thank the referees for their interest in the article and their suggestions for improvements.

The comments made are addressed below.

Reply to Referee #1:

Two major points arise from the review of referee #1. The first point concerns the nature of the paper currently available in ACPD that would be more suitable for a technical journal or for a technical note according to the referee. The authors agree

that validation papers do not provide generally new insights on science (but are essential for a correct use of satellite data) and the question about where to publish them is tricky. However, this paper is part of the first special issue on the new satellite instrument, IASI. It then makes sense that a paper dedicated to the validation of one of the most important product of IASI, ozone, is included in this first special issue. Major changes have been done in the paper to improve the scientific quality of the discussion and are detailed in the following (the authors thank again the referee who greatly helped with his remarks).

The second point (related to the first one) concerns the weakness of the result discussion. The referee noticed that the paper fails being convincing for explaining quantitatively the differences observed when comparing all the data sets. Consequently, important changes have been made in the revised version of the manuscript in order to improve the scientific quality of the discussion:
(1) a table and a paragraph in which the authors discuss the difference of the retrieval approaches of the different groups has been added in section 3 (see the response to referee #2 for more details)
(2) the section 7 has been totally rewritten in order to better describe the results of the comparisons but also to provide an argued discussion about the possible reasons of the differences observed between the different retrievals (the new section 7 is provided at the end of the document).
Due to the complete rewriting of the section 7, the authors hope that the comments referred in the review as p11458 line 6 (*I do not understand the sentence "Both Figures show the similarity. . .".*), line 12 (*"the effect of convolution is even larger". Please quantify, give some figures.*), line 20 (*All the paragraph. What is the link between all the sentences of this particular paragraph. I am lost. This will need some rephrasing.*), p 11459, line 5 (*"The Figure also shows the high quality of the retrieval results for both teams. The Figure also shows the effect of the sonde profile convolution on the comparison quality."" Could it be possible to be less vague? Please*

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perform a quantitative comparison, give figures.), line 11 (*"These effects due to the convolution are less pronounced for the retrieval at LISA". No, these effects are also well pronounced at LISA: in the domain 0- 6 km, R is improving from 0.54 to 0.79, and in the domain 0-11 km, R is also improving from 0.74 to 0.81, when applying the averaging kernels. This also needs discussion.*) are now clarified.

A third point concerns the LPMAA retrievals.
For the revised version of the manuscript, the LPMAA retrieval algorithm had been critically investigated. After some errors in the data analysis had been detected, the IASI spectra have been entirely reprocessed with significantly better comparison results. LPMAA has been engaged from a long time in the IASI project from the preparation of the mission to the validation and data analysis. The retrieval algorithm developed at LPMAA is not optimised in term of calculation time. It is not well fitted to process a large number of retrievals. One goal of this paper is the separation of the quality of the IASI spectra and their content of information on the one side and the quality of the retrievals on the other side. We decided to keep the (new) results obtained at LPMAA, even if they do not cover all the situations selected for the paper, because they increase the number of independent algorithms. The insight in the analysis brought by the LPMAA retrievals is i.e. the manifestation that it is impossible to avoid the peak in the differences at about 9-10 km. The LPMAA retrievals are not included in the statistical part of the paper due to their small number, but in the rewritten conclusion.
The reprocessing and the rewriting of the addressed sections, considers the comments referred in the review as p11458 line 19 (*"Again, the comparison shows the good quality of the retrieved profiles". No, I regret to say that LPMAA retrievals for some unknown reasons are not consistent with LATMOS and LPMAA retrievals. This needs discussion. More globally, the LPMAA retrievals are only present in section 7.1 and do not bring any insight in the analysis. This would require some in depth discussion to know whether LPMAA retrievals should be presented in the present manuscript.*),

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Figure 2b (*There is a stronger variability in the LPMAA than in the LISA and LATMOS data sets, and the results are worst. This needs comments.*), and conclusions (*Results from LPMAA do not appear in the conclusions.*).

Reply to the comments listed as p11458, line 13 (*"This may be explained by the weaker sensitivity of LATMOS and LPMAA in the lower troposphere. . .", but the difference is present in the entire free troposphere. Thus the argument is not fully valid.*) and p11461, line 19 (*"Compared to . . .". It is rather surprising to understand some differences between LISA and LATMOS retrievals when reaching the conclusions. This should be explained in sections 7.1-3.*): To clarify the point about the possible effect of the convolution of the sonde profile with the averaging kernels of the retrievals, the authors added a paragraph after Eq (3) and discussed this effect in details in section 7 (two figures have been added to illustrate the discussion).

Reply to comment referred as p11458, line 11 (*"the convolution introduces a small but visible difference (about 0.01 ppmv)". Checking on Fig. 2b, it is rather 0.4 ppmv at 8-12 km.*): With this sentence (that is not any more in the revised text), the authors did reference to the effect of convolution that takes place more in the lower troposphere and wanted to give an estimate of the effect. The difference at higher altitudes has more likely other origins related to the difference between the a priori profile and the sonde profile. If the a priori profile is significantly far from the solution (sonde profile), the retrieval does not allow reproducing the real state of the atmosphere. This is explained in the revised version of the manuscript and illustrated with a figure.

Reply to the comment on Figures 3a and b: (*In the domain 8-12 km, some LATMOS retrievals differ by about -20 to -30% from the sondes (with or without AKs). This needs comments.*): The use of one constant a priori profile for all latitudes and seasons leads to situations, where the a priori profile is too far for a good retrieval, as

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explained in the previous reply and the new manuscript.

Reply to comment referred as p11459, line 12 (*"The correlation. . .". Any reasons? The standard deviation is also less in LATMOS AK than in LISA AK data sets. Why?*) and p11460, line 7(*"The bias of the LISA..", this needs some discussion.*): We extended the section about the averaging kernels, the following is now explained there. The sensitivity of the retrieval on the real profile is expressed in the averaging kernels. A real retrieval has an averaging kernel matrix (AK) between the two extremes zero and unity.
In the case in which the AK equals zero, the retrieved profiles become the a-priori profile, and the convolution of the sonde profile also gives the a-priori profile. Obviously, in this case, the difference between sonde profile and convolved sonde profile becomes zero with a zero standard deviation.
When the AK is unity, the retrieved profile does not contain the a-priori profile, and the convolution does not change the sonde profile. In this case, the difference between the two profiles is sensitive to retrieval errors and to errors in the sonde measurement, and its standard deviation contains the random part of the errors.
In the comparison of retrieved profiles with sonde profiles, a small difference to convolved sonde profiles and a small standard deviation are not necessary a measure for the retrieval quality. The difference has to be interpreted with caution and discussed also according to the difference with the raw sonde profiles.

Reply to the comment on Figure 1(*Do the averaging kernels presented refer to a total or partial column or to a selected altitude and which one in that case?*): The averaging kernels refer to the retrieved profiles. The figure shows the diagonal values of the averaging kernel matrix and therefore the sensitivity of the retrieved profile on the real profile of the same altitude.

Reply to the comment on Figure 5(*The EUMETSAT distribution is not really Gaussian.*

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This might indeed be induced by the use of NN retrieval scheme (non-Gaussian statistics). Does that impact on the methodology you are employing to quantify the difference between 1) all the retrievals and 2) all the retrievals and the sondes? This could be discussed in the text. Why not also over plotting (for instance in green) the envelope curve of the AKsmoothed sonde profiles on this Figure.): The type of the distribution (Gaussian, bi-modal, ...) does not impact on the comparisons methodology, as it is a priori unknown. The high number of profiles leads to a Gaussian like distribution for the random error sources. whereas the shape of the distribution of the systematic error is not predictable. We believe, that independent of the distribution, the comparison of mean values is more objective than i.e. median values. Same is the case for the standard deviation to measure the broadness of the distribution. We rewrote the discussion section, also to consider this comment. The idea of over-plotting the figure with the distribution for the AK-smoothed sonde profiles is acknowledged and realised in the revised version.
We thank the referee also for the minor points (including p11460,l27) which have been corrected in the current version of the manuscript.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 11441, 2009.