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Comment

***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.**

Anonymous Referee #1

Received and published: 21 June 2009

The manuscript entitled “Tropospheric ozone from IASI: comparison of different inversion algorithms and validation with ozone sondes in the northern middle latitudes” by Keim et al., describes different inversion algorithms from three French laboratories (LISA, LATMOS and LPMAA) together with the official EUMETSAT retrieval scheme to derive the total and partial columns of tropospheric ozone from IR spectra recorded by IASI on MetOp. The ozone retrievals are, in the first part, compared to each other and, in a second part, validated against ozone sondes. The global exercise relies on measurements performed in the northern middle latitudes over a maximum period ranging from June 2007 to August 2008. There is an overall agreement between the retrievals of tropospheric ozone column of better than 2 Dobson Units (DU) for the

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three labs, and better than 5 DU for EUMETSAT. Finally, one group (LISA) developed a climatology of the annual variation of partial column of ozone from IASI retrievals that is compared to a climatology already referenced in the literature.

Compared to the first version of the manuscript sent to the ACPD journal I already reviewed, I acknowledge the fact that the authors have taken into account the majority of the technical points I noticed. Tables and Figures are much more readable. Some sentences/paragraphs have been entirely reworded. This is a good point. Unfortunately, my main concern was and is still related to the actual level of scientific results contained in this paper. In my opinion, this manuscript is very well describing the different methods used by all the groups, is detailing the methodology for actually making the comparisons taking into account different vertical resolutions and errors either systematic or random. But it fails being convincing for explaining quantitatively the differences observed when comparing all the data sets. My conclusion again is that the manuscript when some major points are addressed can be published in a technical journal or as a technical note.

Major points

The paper lacks of quantitative explanations about the differences within all the data sets. Please specify the actual differences in ppbv and percentage and give some elements of answer to understand from where these differences could arise.

1) In the sections 7.1 and 7.2,

p. 11458, l.6: I do not understand the sentence “Both Figures show the similarity. . .”.

p. 11458, l. 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.

p. 11458, l. 12: “the effect of convolution is even larger”. Please quantify, give some figures.

p. 11458, l. 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.

p. 11458, l. 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.

p. 11458, l. 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, l. 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 perform a quantitative comparison, give figures.

p. 11459, l. 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.

p. 11459, l. 12: “The correlation...”. Any reasons? The standard deviation is also less in LATMOS AK than in LISA AK data sets. Why?

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?

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.

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.

2) In the section 7.3

p. 11460, l. 7: “The bias of the LISA..”, this needs some discussion.

p. 11460, l. 27: Please specify the unit Dobson Unit (DU): “0.6 DU”.

Figure 5. The EUMETSAT distribution is not really Gaussian. 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 AK-smoothed sonde profiles on this Figure.

3) Conclusions

p. 11461, l. 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.

Results from LPMAA do not appear in the conclusions.

Minor points

Within the text, “a priori” is written “a-priori”, no hyphen. Same for “correction-factor”, “KC-sondes”, “in-situ”, “IASI-column”

p. 11444, l. 9: GOME: acronym

In the section 4, please specify the countries of the different cities.

p. 11455, l. 12: I would get rid of the term “unlucky” unless it is explained why the dependency is unlucky.

p. 11460, l. 18: I would replace “in agreement” with “consistent”.

p. 11461, l. 3: add “, respectively” after “symbols”.

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p. 11461, l. 13: change “at mid-latitudes” into “at northern mid-latitudes”.

p. 11461, l. 21: specify the labs and change into “6.1% for LATMOS instead of 17.7% for LISA”

p. 11461, l. 23: specify the labs and change into “3.5 DU for LATMOS instead of 0.3 DU for LISA)”

p. 11462, l. 6: I would split the sentence “The retrieval at EUMETSAT. . .” after “developing phase”.

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

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