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Atmospheric Chemistry and Physics Discussions



# *Interactive comment on* "First steps towards the assimilation of IASI ozone data into the MOCAGE-PALM system" *by* S. Massart et al.

**S. Massart et al.**

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We would like to first thank the reviewer for the time taken on the manuscript, and for helping us to improve the description of our methods and results and to bring out more clearly our main ideas and conclusions. Our responses to the different comments are detailed below, along with the description of the changes that have been done in the revised version of the manuscript.

Reply to H. Eskes general comment

The established difference between the MLS and the SCIAMACHY analyses must not be interpreted as a bias of the satellite data. The modifications to the SCIAMACHY data are done to ensure that the two data sets carries consistent information (in terms

of average) to the assimilation system. In the revised version, we have been careful in not identifying the data correction as a bias. The term "bias" is now only used for the comparison of the analyses with independent data.

The difference between the MLS + SCIAMACHY analysis and the MLS + IASI analysis is due to:

1. the design of SCIAMACHY that does not provide data during the night (in particular during Polar Nights)
2. the large specified errors of IASI in the South Polar Region (SPR) (see below our response to the specific comments).

Where both SCIAMACHY and IASI data are available with a similar spatial density, the improvement brought by the MLS + IASI analysis is not significant. The conclusion has been modified to point out this result.

To the reviewer opinion, we missed an opportunity to further detail the bias and the standard deviation of the IASI data with respect to daytime or nighttime measurements. To investigate this, we have separated the daytime and the nighttime data and we have computed the bias and the standard deviation for the two data sets. These results are interesting and are commented in the revised version. In particular, we have replaced Fig. 10 by two figures, one for the nighttime and one for the daytime measurements. Fig. 3 below has also been added and commented to show the impact of the surface emissivity on the bias and on the standard deviation. The conclusion has also been modified to take into account these modifications.

Using a perturbation method, the neural network of the LATMOS can also provide IASI averaging kernels (Turquety at al., 2004). Unfortunately, as it is time consuming, the real-time processing of the IASI data did not allowed us to derive both the columns and the averaging kernels on the global scale. Thus, the IASI total ozone column averaging kernels are not considered in this study. However, when applied to the IASI



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spectra, the neural network algorithm provides data with a maximum sensitivity in the free troposphere (Turquety at al., 2004). The sensitivity extends up in the stratosphere to around 1 hPa. In the lower atmosphere the sensitivity is directly linked to thermal contrast (difference between the surface and the atmospheric temperatures), as discussed in Clerbaux at al. (2009). When the thermal contrast is large and positive, the IASI instrument is sensitive to the lower layers. When the contrast is small, the sensitivity decreases accordingly. Detailed analyses of the LATMOS-IASI vertical sensitivity are given by Boynard et al. (2009). These results are recalled in the new version of the paper.

Reply to H. Eskes specific comment

Referee: *Abstract: The first lines in the abstract can be improved.*

The abstract has been rephrased.

Referee: *p6692, l22: "Onboarding" ?*

The sentence has been split up into two sentences and the term "Onboarding" has been removed.

Referee: *p6693, l20: This is a strange conclusion. One could argue the opposite: the mismatch in resolutions could lead to all sorts of (representativeness) problems.*

Yes, you are right. The introduction has been rephrased and the arguments for using data assimilation have been more clearly stated.

Referee: *An introduction on the MOCAGE-PALM system is missing. A more broad introduction to existing ozone assimilation systems is missing. There are very few references in the introduction. The position of this ozone assimilation work in research world-wide is not clear. How does the MOCAGE-PALM system compare with other ozone assimilation systems?*



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The introduction has been rewritten (paragraph 3 and 4) in order to answer these remarks. More references have also been added.

Referee: *p6695, l23: Total ozone columns are considered without averaging kernels. As mentioned, it is important to provide the reader with a better general understanding of sensitivity of IASI vs altitude, and dependence on a-priori.*

The end of the first paragraph of Section 2.1.1 includes now a detailed discussion of the IASI sensitivity as a function of altitude (see also above the response to the general comments).

Referee: *Superobservations: How are these formed (simple mean, or weighted mean)? What is the error bar, and how is the error bar of the super-observation related to the errors of the individual observations? When an estimate of the observation error is presented, it is important to distinguish again observations and superobservations.*

Concerning the IASI data, we had initially no idea on the errors of the individual observations. Thus, the IASI super-observations could not be computed using a weighted mean and we used a simple mean. Moreover the estimate of the observation error is expressed on the super-observation grid. This means that we assume that each individual observation on the 2 degrees by 2 degrees super-observation grid has the same error.

While creating the super-observation, it is also possible to compute the standard deviation of all individual observations located into each super-observation cell grid. This is a prior estimate on the super-observation error. As this estimation (not presented here) shows that there is no agreement with the diagnosed standard deviation of the super-observations (Fig. 10b of the initial paper), we decided not to add this information to the paper.

Concerning the MLS and SCIAMACHY super-observations, they were also built using a simple mean. This is now explicitly mentioned in the text.



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Referee: *p6697, l18: "precision" Please be more specific on what is meant by "precision". Does this include systematic and/or random error components?*

Random error. This has been modified in the text.

Referee: *p6697, l27: "values from 5 to 100% below 100 hPa" ??*

These values are given by Froidevaux at al. (2008).

Referee: *p6698, l1: "The standard deviation of the observation error required by the assimilation algorithm is set to the specified observation error. The correlations between the measurements of a same profile are neglected." Are these statements consistent? Please be more clear what is meant here. The (super-observation) mean of independent observations has a smaller error than the individual members. How is this modelled?*

We rephrased those sentences. What we wanted to mean is that the random observation errors are assumed to be independent, not the measurements.

Concerning the way we build the super-observations, please see above.

Referee: *p6700 How is the troposphere modelled? Is it based on a zonally-averaged climatology? Does the model generate regional ozone enhancements related to the local emissions? Can we expect regional model biases due to the chemistry treatment? The chemistry in higher stratosphere is very fast. This may be problematic and can result in systematic biases. This because the analysis increment is lost very rapidly. Are systematic biases observed (vs MLS)?*

The DA system uses the linear ozone parameterization (Cariolle and Teyssèdre, 2007) in its latest version. It is based on the linearization of ozone production/destruction rates using an altitude/latitude chemical model. So we do not introduce any non-zonal forcing in the system, nor any production due to local precursor emissions. So if zonal asymmetries are produced in the ozone analyses, they must be introduced by the data, not by the underlying model. Although we expect the ozone parameterization



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to have limitations in boundary layers, it appears to perform with accuracies similar to full chemical models in the free troposphere and the stratosphere (Geer et al., 2007). Those precisions have been added in Section 3.1.

Referee: *p6701 l4: Please motivate the choice of a 2 degree correlation length.*

From a previous work based on an ensemble of assimilation runs, we have estimated the length-scales of the forecast error correlation functions (assumed to have a Gaussian shape). The length-scales were found strongly inhomogeneous in time and space, with values ranging from 100 km to 300 km in the meridional direction and from 100 km to 600 km for the zonal ones. However, the assimilation system is based on homogeneous horizontal length-scale. Therefore, the horizontal correlation of the forecast error is computed with a trade-off value for the homogeneous length-scale of 220 km (that corresponds to a distance of 2° at the equator).

Referee: *p6701 l10: A bit more detail on the estimation of the "two correction coefficients" is needed. Where does the independent information come from? Is it the superobservation or the individual observation error which is estimated?*

This methodology does not use independent information but assimilated observations. This has been specified in the text. Since we assimilate super-observations, the estimate is based on them. The two multiplicative corrective coefficients are $s_b$ and $s_o$. The first one is applied on $\mathbf{B}$, the covariance matrix of the forecast error, so that the assimilation is performed with $s_b^2\mathbf{B}$. The second coefficient is applied on $\mathbf{R}$, the covariance matrix of the observation error, so that the assimilation is performed with $s_o^2\mathbf{R}$. These details are now in the revised text in the last paragraph of Section 3.1.

Referee: *p 6702: "a value of 0.35 log(hPa) ". log(hPa) does not make sense. A log can only be applied to a dimension-free number. Suggestion: use something like log(p/p0).*

In the original manuscript, an error was made with the unit of the vertical length-scale. The vertical correlation is computed with a space dependent dimensionless length-



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scale $L$. The correlation $\mu_{ij}$ between two pressure levels $i$ and $j$ is expressed as a locally Gaussian function of the logarithm of their pressure ratio ($p_i/p_j$) and of a local length-scale $L_{ij}$,

$$\mu_{ij} = \exp\left[-\frac{1}{L_{ij}^2} \cdot \log^2\left(\frac{p_i}{p_j}\right)\right] . \quad (1)$$

This equation has been added to Section 3.1 and makes it clear that this length-scale is a dimensionless number.

Referee: *Figure 3: Does this reflect a difference between SCIAMACHY and MLS, or is there a model component, e.g. 10 DU could well be a tropospheric model bias? Figure 6 demonstrates significant biases in the troposphere. Please discuss this in more detail.*

Figure 6 shows that the model underestimates the tropospheric ozone in the equatorial region and overestimates it in the South Polar Region (SPR). Assuming that the assimilation of MLS constrains mainly the stratosphere without bias, the MLS analysis would underestimates the total ozone in the equatorial region and overestimates it in the SPR. However, Fig. 3 shows the opposite. The difference between SCIAMACHY and MLS showed by this figure come probably by the fact that the SCIAMACHY averaging kernels were neglected. Section 3.3 has been modified to explain that the difference between SCIAMACHY and MLS could come both from a tropospheric bias in the model and/or the neglect of the averaging kernel information. Section 4.1 and the conclusion now discuss this point.

Referee: *p6703: Is it justified to correct SCIAMACHY data? Or: would it not be better to correct the model troposphere ? Why this choice?*

Before performing a combined assimilation, we removed the difference between the two data sets. Changing the tropospheric parameterization of the model in order to have coherent data sets, proved to be more complex than modifying the data values. We decided to modify the SCIAMACHY data instead of MLS because we may introduce



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new error sources when projecting a total column difference back to a vertical profile and because we have a lesser confidence on the SCIAMACHY data since we do not use the averaging kernel information. To make it clear that this does not mean that there is a bias in the SCIAMACHY data, we have rephrased the third paragraph of Section 3.3 to explain this choice.

Referee: *p6703: What is the Pannekoucke method? Please explain briefly.*

The ensemble method consists on the construction of an ensemble of realizations of the random forecast error by an ensemble of assimilation runs. Statistical calculations on the ensemble of realizations allow the estimation of the standard deviation and the correlation length-scales of the forecast error. Those details are given in the revised text.

Referee: *p6703, bottom: I am not very familiar with the approach, but I get the feeling that this error estimation is providing a lower limit because not all contributions to the total error are accounted for. Please comment.*

Some additional errors would have to be taken into account like the errors on the dynamics. Nevertheless we expect that the shape of the diagnosed error is correct and we then adjust the amplitude using the corrective coefficient $s_b$. This is now discussed in the revised text.

Referee: *Fig 5: Replace log(hPa)! Does the figure show that the relative correlation length is very small in the troposphere?*

The label in Fig. 5 has been corrected as the length-scale is dimensionless. This figure shows that the length-scale is quite constant within the troposphere and the stratosphere, with smaller values for the boundary layer.

Referee: *p6707, l12: " ... is computed each day at 12 UTC ..." this is not collocated in time. Could this be related to the larger rms at the SP edge mentioned on p6708?*

The circulation at the SP edge is intense so it is true that the lack of collocation can



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increase the rms. This is now discussed in the last two paragraphs of Section 4.2.

Referee: *p6708, l22: ".. this implies .. " I do not understand why this is implied ?*

The sentence has been rephrased.

Referee: *p6709, l2-3: "This means that the random variability of our analysis is about 10% for the ozone concentrations and below 2% for the total ozone columns." How is this conclusion obtained? Please explain these numbers.*

In terms of random variability, the difference between the MLS + SCIAMACHY analysis and the ozonesondes is below than 20%. Assuming that this error is the sum of the analysis error and the ozonesondes error, with an instrument error between 5% and 15%, the random variability of our analysis is about 10% for the ozone concentrations. Concerning the total ozone columns, the differences between the analysis and the OMI-DOAS data are mainly below 4%, the instrumental error being around 2%. With the same assumption on the summation of the errors, this means that the random variability of our analysis is below 2% for the total ozone columns.

Referee: *p6709: Does the observation operator compute a simple total column? Specify this vertical operator. There is no mention of a vertical kernel!*

The observation operator computes a simple total column. As it is specified above in the paper, the averaging kernels for IASI are not available. The method to construct the observation operator is further detailed in the revised manuscript.

Referee: *Figure 9: This is called "number of differences ...". Why is not it simply the "number of LATMOS-IASI observations"? I assume there is an analysis for every observation.*

Yes. Change has been done in the text.

Referee: *p6711, l2: Is this an underestimate of the errors? Why does the result agree with the MLS/SCIAMACHY conclusions?*



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The ensemble method provides an underestimate of the errors. But the error made by the method is consistent with a diagnosed underestimated error of about 1%, and an error of about 2% compared to independent data. This is better pointed out in the revised text.

Referee: *p6713, l6: Please be a bit more specific about the bias correction applied. I assume a 2D lat-lon bias map is subtracted from the LATMOS-IASI data.*

To assimilate the LATMOS-IASI data, we removed the monthly mean bias from the LATMOS-IASI super-observations. The monthly bias is computed as a latitude-longitude bi-dimensional field resulting from a monthly time average of the 2° by 2° gridded differences found between our previous analysis and the LATMOS-IASI data.

Referee: *p6713, l10: observation error: is this for individual observations, or for the superobservations ?*

For super-observations. This has been corrected.

Referee: *p 6713, l13: "As this work is a first approach to the assimilation of the LATMOS-IASI data, we did not yet determine ..." This is not a proper motivation!*

The computation of standard deviation and the length-scale is time consuming (at least 5 assimilation runs to compute in parallel for the whole period). The improvement of our system would have required much more time. We believe that the present results are already worth to be reported.

Referee: *Figure 11: It is surprising to see this kind of systematic differences in a bias-corrected product. Please be very precise on the formulation of conclusions.*

This is due to the distribution of the observations. SCIAMACHY does not provide data during the night. As a consequence, during August over the SPR, there are only IASI measurements as showed by Fig. 1 of this commentary paper. Therefore, the assimilation of the IASI data provides information on the troposphere (the stratosphere is mainly controlled by the assimilation of MLS data) while the troposphere is not con-



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strained by the assimilation of SCIAMACHY. The phenomenon is similar over the North Polar Region in winter (see Fig. 2 of this commentary paper). This explains why we found differences between the two assimilations. This explanation is given in section 5.3 of the revised paper.

Referee: *p 6713, l25: "seem to bring significant improvements" ? See earlier comments on the IASI bias-correction and interpretation. The gains, overall, are not very conclusive.*

As the paragraph has been modified in order to answer the comments on the Fig. 11, these sentence have been suppressed and the conclusions reformulated.

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Interactive comment on Atmos. Chem. Phys. Discuss., 9, 6691, 2009.





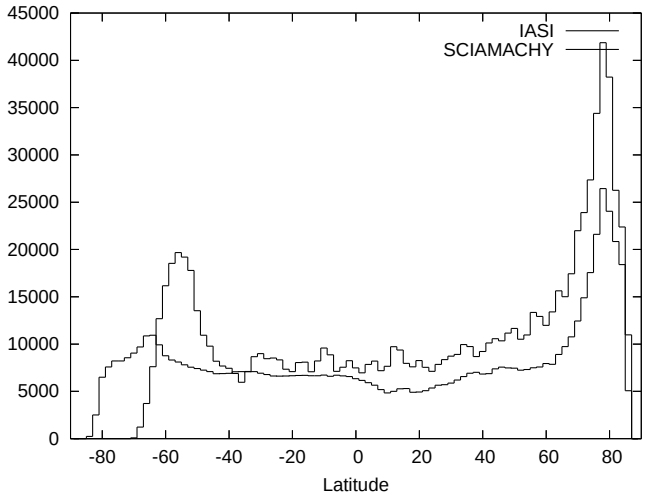


**Fig. 1.** Number of monthly IASI (black line) and SCIAMACHY (dotted line) super-observations for August 2007 as a function of latitude.



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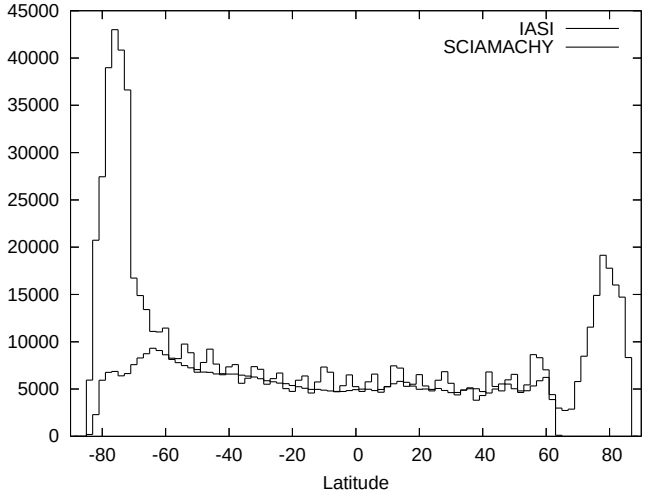


**Fig. 2.** Number of monthly IASI (black line) and SCIAMACHY (dotted line) super-observations for December 2007 as a function of latitude.



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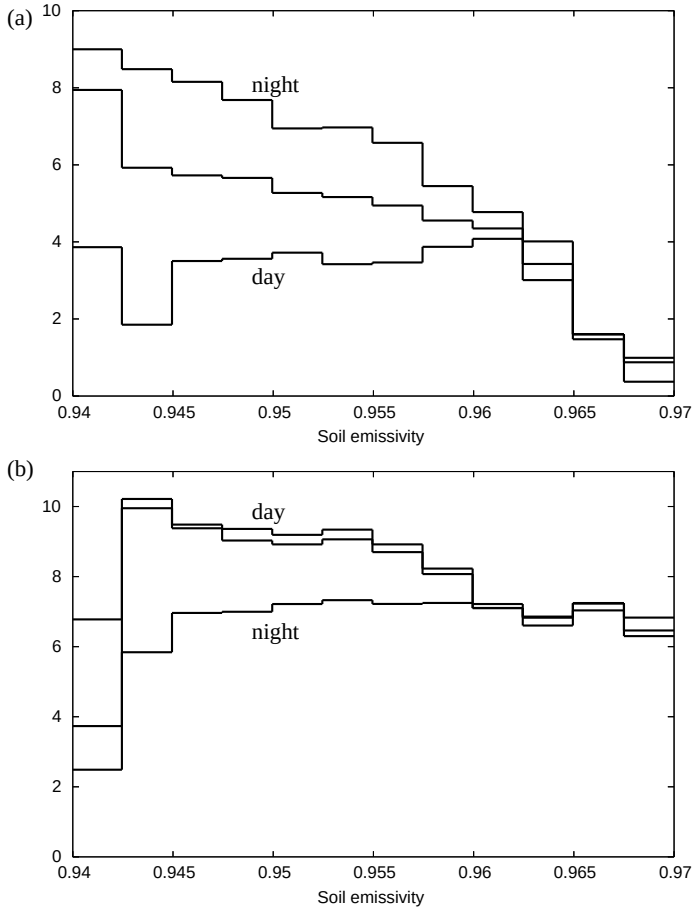


**Fig. 3.** Mean over the globe of the **(a)** average and **(b)** standard deviation of the difference in % between the LATMOS-IASI ozone data minus the co-located combined MLS and SCIAMACHY ozone analysis normalized by the IASI measurement, as a function of the ground emissivity, for the period 1 August 2007 to 31 December 2007. Continuous line : daytime and nighttime data ; dotted line : nighttime data ; dash line : daytime data.



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