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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.

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This paper presents a thorough and extensive evaluation of IASI ozone data. This analysis is based on a well-designed and well-performing DA system (MOCAGE-PALM). Five independent data sets (MLS, SCIAMACHY, OMI, ozone sondes and IASI) combined with the analyses of the assimilation system have been used, which provides wide-ranging opportunities to learn about the measurement data sets. The separate SCIAMACHY/MLS assimilation performance discussion leads to a long paper, but I found this nevertheless useful to document the quality of the analyses which is subsequently used to evaluate IASI. It is my opinion that the results presented deserve to be published.

Nevertheless it is my opinion that the paper can be improved, especially in response

to the major remarks given below.

Major remarks:

p6713, bottom: " .. the combined MLS and LATMOS-IASI analysis seem to bring significant improvements .." To my opinion this is a strange and possibly wrong conclusion. First, a bias is removed from the IASI data based on the MLS/SCIAMACHY analysis. This with the aim to bring this analysis in synch with SCIAMACHY/MLS. Then the analysis with the bias-corrected IASI data is compared with the old analysis based on SCIAMACHY and mean differences are discussed. The authors draw definite conclusions for these "residual differences" observed, as compared with OMI and sondes. One should be very careful to discuss the biases of a bias-corrected data set! I suggest that this is made very clear. Conclusions should be weakened or should be discussed carefully to convince the reader that they are real, and not some spurious feature.

The part which I found most interesting are the bias and standard deviation plots, figure 10. It is my opinion that the authors missed an opportunity here. Because of the good reference (analysis based on SCIAMACHY/MLS), the IASI data quality could have been analysed in much more detail. This may be done as a function of several properties of relevance to the retrievals, such as ice/snow, sand and surface emissivity, cloud cover/type, night or day, observation geometry. Also the added value of the ISAI data to improve the analysis where assimilation/model uncertainties are particularly large may be discussed. Such an extended analysis could bring much useful information.

The lack of IASI ozone kernel and a-priori information is mentioned in the discussion as a topic for the future. Since the data product does not contain kernel data (I assume) it is a valid approach to compare the ozone values with analysed columns. However, the IASI sensitivities and kernels should be discussed in section 2.1! It is important for the reader to be able to interpret the results. What are the vertical sensitivity profiles of IASI? At what altitude is IASI most sensitive? For what altitudes does IASI rely on

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a-priori? What a-priori is used? How does this influence the results.

The introduction is very limited and does not really provide the context of this work and relation to other ozone assimilation activities, see below.

The formulation can be improved. I would recommend a proof-read by a native English speaker.

Detailed comments:

Abstract: The first lines in the abstract can be improved. The statement "... (IASI) is one of the five European new generation instruments ..." is not useful in an abstract. A general statement like "Data assimilation is a powerful tool to combine these data with a numerical model." is also not useful in an abstract. Concerning the phrase "... first steps made towards the assimilation ...": Why are the authors so modest?

p6692, l22: "Onboarding" ?

p6693, l20: "The assimilation of the IASI trace gases measurements by a CTM is therefore an efficient way to combine information brought by the instrument with the information brought by the model". This is a strange conclusion. One could argue the opposite: the mismatch in resolutions could lead to all sorts of (representativeness) problems.

p6693, l28: "an improving phase" -> development stage (e.g.)

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?

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

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sensitivity of IASI vs altitude, and dependence on a-priori.

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.

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

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

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?

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)?

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

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?

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).

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Fig 2: Please tidy-up the notation of the y axis (soˆ2, sbˆ2).

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.

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

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

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.

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

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?

p6708, l22: "..which fairly agree the independent.." please correct

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

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.

p6709, l7: " .. rather good ..". The authors use this kind of subjective and meaningless statements more often in the text. Please remove. Stick to the numbers please.

p6709, l10: "to this quality ozone fields" - please correct

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

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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.

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

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.

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

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!

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.

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.

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