

Interactive comment on “Comparison between the assimilation of IASI Level 2 retrievals and Level 1 radiances for ozone reanalyses” by E. Emili et al.

Anonymous Referee #2

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Main conclusion:

The authors compare the assimilation of IASI ozone L2 retrievals with the assimilation of IASI radiances, with as much as possible the same settings and the same forward RTM. The equivalence between L1 and L2 assimilation depends on e.g. the non-linearity of the relation between radiation and ozone and on the realism of the linearisation point, or a-priori profile used. I agree with the authors that the degree of similarity between L2 and L1 assimilation depends on details and should be studied for real observations. I find this topic very interesting and relevant, and this paper is unique as far as atmospheric trace gas assimilation is concerned. Therefore I am in favour of publishing the results.

However, I have several general and specific comments which require significant ad-

justments to the present draft. I suggest the responses to these comments are incorporated in a modified manuscript before publishing.

General comments:

The authors document clear differences between L2 and L1 assimilation, and seem to suggest that L1 assimilation should be considered. However, it seems to me that L2 with an improved a-priori may be a possible alternative to reach a similar performance of the analysis. Would running the IASI L2 retrievals with a varying a-priori, for instance taken from the Copernicus Atmosphere Monitoring Service daily analyses, be a feasible option? Would that solve part of the problem with L2 compared to L1? Could the authors discuss this in a more balanced way in the conclusion section (and maybe in the abstract as well)?

Although the L1 and L2 experiments are set up with as much as possible equal inputs and RTM (but different a-priori), there are still subtle differences as discussed in the text. I am wondering how much those differences may also result in differences in performance as documented in the paper? Especially since the differences documented between L1 and L2 are quite small. This leaves me with a bit an uneasy feeling that the results are maybe not fully understood.

The relative differences in Fig. 2 seem to indicate persistent biases. Maybe it is a lot of extra work, but I wonder how the difference plot of L2 retrievals for the climatological a-priori (presented in the paper), compared to L2 retrievals with MOCAGE profiles would look? Such a plot would be a valuable addition. Would that show similar features as in Fig.2, at around 300-500 hPa ?

It would be helpful if the authors could add an image of the IASI averaging kernels, typical examples or averages, for NH, tropics and SH. In this way the reader can better understand at which pressures one may expect an impact of IASI.

The impact of IASI in both the L1 and L2 experiments seems to be relatively small,

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with also negative impacts. Especially when MLS is included as well, which already removes most of the bias around 200 hPa. This baseline

Detailed comments:

Title: "for ozone reanalyses": upon first glance this seems to suggest that the paper presents results of a multi-year reanalysis, which is not the case. Is it necessary to include the word "reanalysis" in the title?

Abstract, I9: "significant differences". The abstract does not give a very firm conclusion. Does the work presented justify the stronger statement that the non-linearity in the retrievals in combination with unrealistic a-priori profiles are the cause of the L1-L2 differences?

Abstract: Is there a clear recommendation from this work? Would L1 assimilation be preferred? A more clear statement would be helpful.

page 2

I5: Useful to mention the averaging kernels as well: .. and DOF linked to the averaging kernels ...

I9: "First atmospheric composition models": Reformulate

I19: "However, some aspects of the Data Assimilation (DA) approach differ between the chemistry and meteorology communities." Please be more specific, or remove the sentence.

I21: "Since long time" Please be more specific, by e.g. providing the year when this became common practice.

I23: "This resulted necessary to avoid"; please reformulate.

I24: About the historical background: I was wondering about the problems encountered when assimilating L2 retrievals in NWP (paper of Eyre)? Is it the non-linearity and a-

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priori dependence (as suggested by the text), or is it a simplification of the retrieval results? The latter could arise when e.g. kernels and full covariance matrices are not used in the assimilation, or not provided by the retrieval teams, which would clearly lead to strong a-priori dependence of the analyses.

I27: I would suggest to refer to the book of Rodgers as well. Also the paper of Migliorini 2012 is relevant here.

Page 3

I29: "Both are based" It is not so clear what "both" refers to: the two studies, or SOFRID and MOCAGE.

page 4

Section 2.1: METOP also has the GOME2 instrument. Has the synergy IASI-GOME2 been considered? Why the choice to use MLS?

page 5

I4: "increased biases": what does "increased" refer to?

I10: "LA" ?

page 7

I11: "ECMWF NWP model" Please replace the word "model" by e.g. "NWP model and assimilation system".

I19: The RTTOV versions for the L1 and L2 experiments are different, see table 1. Can the authors be sure that this does not significantly influence the results/conclusions?

I29: "... and was extended ..."

page 8

I19: "we assimilate here directly the full L2 profiles (43 levels)". Migliorini wrote a paper

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(2007) to discuss an efficient interface between L2 retrievals and data assimilation which is relevant in this context. Because the DOF is quite low, this implies that a lot of noise (43-DOF) is presented to the assimilation when all 43 levels are included. In principle I agree that this avoids any loss of information, but in practice I wonder if the full information may introduce numerical issues (randomness) in the system, especially when this is combined with vertical interpolations? Please comment.

I22: "The steps for the computation of modeled radiances are equal to the profiles ones until the vertical interpolation." Please reformulate.

I27: "climatological profile" -> "climatological profiles"

I29: "as it is done within SOFRID retrieval scheme" please reformulate

I29: Does this mean that the SST is treated differently in the L1 vs L2 assimilation experiments?

page 9

I8: "initialized on 1st June 2010"; replace by "initialized on 1 June 2010"

I11: "a diagonal matrix (i.e. with no inter-channel correlation) is used". Is the same diagonal matrix used in the retrievals that produce the L2 dataset?

p9, bottom to p10, top: I got a bit lost with the numbers provided for the background standard deviation, also in comparison with Fig. 1. I understand that a background standard deviation during assimilation is often smaller than the std of a free model run, but I do not manage to connect the numbers with e.g. Fig.1 in combination with Fig.3?! What is the motivation to go from 5% to 2% in the stratosphere, which seems like a big step and does not seem justified given Fig.1? Does this choice lead to very small stratospheric increments? What is the justification for a step between stratosphere and troposphere? The standard deviation should depend on the data assimilated. Normally these kind of numbers are optimised with e.g. a chi-square test.

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p10, l15: One would expect that features in the boundary layer, and, to a lesser extent, the free troposphere show vertical correlations because of e.g. vertical mixing and convection. This in contrast to the stratosphere.

p10, first part: I think the B matrix discussion can be shortened somewhat, because optimising it is not so important for the topic of the paper.

page 11

l5: "are in generally ", remove "in"

l7: "are found in correspondence of tropical latitudes". Please reformulate.

l14: "equivalence between L1 and L2 assimilation is not verified for O3". I suggest to explicitly add "for O3 retrievals in the thermal infrared".

page 12

l2: "The assimilation increases the RMSE of the tropospheric profile at northern latitudes (60°N-90°N)." I guess you mean in the range 350-1000 hPa.

l8: "the other way round". Replace by "around".

l17: "Hence, we expect a stronger impact of the prior in the retrieval results,". I do not understand this. It means that the DOF is smaller, which is clearly observed at altitudes around 200hPa, where the improvement with IASI data is much more limited in the SH. But a-priori plays only a role through non-linear effects. Why would these non-linear effects be larger in the SH?

l27: "The only exceptions are a lower RMSE degradation at 50 hPa". Should we believe the sondes or MLS here? How many sonde launches are included, and confirm the 60 hPa bias?

l32: "total computing time is 3.9 CPU hours". Is this on a single core/node ??

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Fig.5: The figure seems to prove that the analysis of MLS and of IASI are more consistent in the case of L1, while the L2 plot indicates biases between the instruments, especially in the tropics. This could be discussed a bit more explicitly.

page 14

I16: "mixed elsewhere"? Do you mean to say "mixed results are obtained elsewhere"?

I22: "which are differences" please correct the English.

I23: "reanalyses". I suggest to broaden this to e.g. "analyses and reanalyses".

I24: "between the L2 retrieval and the assimilation algorithm ". I suggest to change to " between the L2 retrieval and the L1 observation operator" or something like that

I24: "using the same RTM". But the version of the RTM is different ?!

I28: "between each other". I suggest "against each other".

I30: "Main findings suggest". I suggest "The results suggest ..."

page 15

I6: "We could imagine". I could as well, but is this a recommendation?

I6: The non-linearity of the retrieval may be very different for different species and spectral ranges. Which ones would be candidates to show significant differences between L1 and L2?

I9: I was wondering if we may expect positive synergies between IASI and GOME2? They are both on the same platform. Please discuss.

I15: "Level 2 products can be aggregated". This useful remark could be phrased more generally by referring to "Use of the Information Content in Satellite Measurements for an Efficient Interface to Data Assimilation" by Migliorini et al, 2007. Through L2 the number of useful observations presented to the assimilation may be optimised (to ultimately match the DOF).

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