

Interactive comment on “Combined assimilation of IASI and MLS observations to constrain tropospheric and stratospheric ozone in a global chemical transport model” by E. Emili et al.

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This study looks at the assimilation of IASI tropospheric ozone columns into a model with very simple tropospheric chemistry and no modelling of surface ozone sources. Stratospheric ozone is constrained by assimilating MLS profiles. Analyses of free tropospheric ozone can be improved by assimilating IASI observations, but mainly only in the tropics, where the main zonal patterns coming from biomass burning can be reproduced reasonably well. However, it is not possible to improve the boundary layer anywhere, or the higher latitudes.

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Overall, I think this study has been well-conducted and the presentation is good. The combination of experiments (free-run, MLS, IASI, MLS+IASI) with the independent validation (ozone sondes, total column) makes it easy for the reader to get a good idea of the quality of the assimilation. However, I would like to see a few changes before final publication.

Minor issues

1 - The abstract needs to be improved. It contains detail that need not be there (e.g. removal of the MLS 215 hPa layer) and it does not give a succinct enough summary of the overall picture. I have tried to give a top-level overview in the first paragraph of this review: these to me are the essential details that need to be conveyed in the abstract. Also I'm not comfortable with the way the results from Fig. 12 are given in terms of the global UTLS and TOC uncertainty reduction. The global figures are not that interesting given that the real story is about the tropics vs the extratropics. I would suggest the abstract should just give the RMSE changes in the tropics and the extratropics separately. this would also remove the need to say "The positive effect of assimilating IASI ... is very significant ..." which is glossy and non-scientific.

2 - Introduction, p. 21459, line 21 - "the degree of complexity of the CTM .. might become less relevant" / section 3.1, page 21465, line 21: " the resulting relaxation rate is about 4% of the difference between the climatological profile and the model profile .. which is negligible compared to the frequency of satellite measurements". The latter sentence is nonsensical, given that a rate in ppbv h⁻¹ cannot be compared to a frequency. But more than this, I'd like to see more justification and explanation of these statements that the observation density in time and space is high enough that the tropospheric Carioille chemistry is mostly irrelevant. Even a figure or two might be helpful. Surely if the exponential relaxation time is 24h, that is quite comparable to the frequency of the observations, which is perhaps every 12h?

3 - Section 2.1 - it would be useful if the authors could mention in the text the time

C8017

availability of the IASI and MLS data. Is it suitable for near-real time operations?

4 - Generally, "TOC" seems a pretty confusing acronym - total ozone column or tropospheric ozone column? The authors have introduced it in the text as the former but it is still easy to get confused.

5 - Section 2.2.1, page 21462, line 19-21. This sentence needs to be reworded. I don't understand the argument.

6 - Page 21465, line 11. I suspect Geer et al. (2007) found the accuracy was good specifically in the context of a data assimilation system. The text here implies that Cariole chemistry can really do better than a complex tropospheric chemistry model.

7 - Page 21466, line 2-4. Most of these references are only tangentially relevant to the current work and should be removed. If they really are relevant to the current work, the text should say why.

8 - Page 21474, line 2-3 "...able to correct most of the deficiencies of the direct model ..." - this is going too far. "able to partially compensate for the deficiencies" would be a more reasonable statement. This also needs to be reflected in the conclusion on p. 21480, line 24.

9 - Page 21474. line, 19-20. "Aircraft obs are ignored hereafter". The text should clearly state if the aircraft obs fully support the results from ozonesonde observations or if there are some areas where they don't; it's not quite clear as currently phrased.

10 - Section 4.3.1 - discussion of B and R. It's fair enough to compare R to O-B statistics (e.g. IASI and MLS FG departures) but a good alternative way to determine the B variance may be available: ozonesonde verification of the 4D-Var background in the IASI+MLS experiment will give B (admittedly plus a little sonde error). The authors should include that in the discussion.

11 - Figure 1 (and other figures) have a very hard-to-use y axis. Given the scale is log, there need to be intermediate division marks (e.g. 200, 500, 800 hPa) to help the

C8018

reader identify the levels between the only marked levels at "10²" and "10³".

12 - Figure 14 and discussion in the text. The day-to-day variability of ozone at Mauna Loa needs to be better understood and explained in the text, e.g. why the low ozone episodes on 14 - 18 August, 4 November and 23 November? An investigation into the sources of this low-ozone air would be straightforward to perform (at least roughly: where did these airmasses come from?) and it would shed a lot of light on why neither the free model nor the assimilation does a good job in reproducing the low ozone episodes. This would be quite an interesting counterpoint to the generally positive results from the ozonesonde verification.

Typos and grammar

1 - Abstract line 23: "little sensitive" -> insensitive

2 - Everywhere: where they have written "sensible" the authors mean "sensitive"

3 - Page 21470: line 7: "enhanced" erroneously suggests an improvement. "increased" would have more neutral connotations line 28: "arbitrarily reduced" - not really! The authors have reasonable justification for this reduction, given earlier in the text I think.

4 - Page 21472, line 15 "supposed to be exiguous": "supposed to" doesn't have the intended meaning and "exiguous" is a rarely used word - "expected to be small" would be clearer.

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