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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Deriving tropospheric ozone from assimilated profiles" *by* Jacob C. A. van Peet and Ronald J. van der A

Anonymous Referee #1

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Deriving tropospheric ozone from assimilated profiles:

van Peet and van der A. aim at improving the TM5 simulation by assimilating OMI and GOME-2 ozone into the model. The results of the paper show the clear positive impact of the assimilation making it very interesting and worthy of being published. In general the figures are clear and well done but the syntax and the grammar can be improved. My main comments for the paper are that it lacks context; that is, it is not well discussed in context of what is done so far for ozone assimilation and how this work fits in the bigger picture. I recommend the publication of this paper after the following comments and suggestions are addressed:

General comments

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Discussion paper

- The averaging kernels of OMI and GOME2-A can help you understand and analyze your results (or at least a discussion of AKs e.g. the recent publication by Keppens et al., 2018 that shows the OMI-GOME2 AK and profiles), and can help the reader understand the added information content from the satellite measurements. For example, in Figure 2, is the largest improvement when you use the assimilated profiles at altitudes where the AKs peak? This can be added in the methodology section when you present GOME2 and OMI.

- What is the added value of using GOME2 + OMI versus using one or the other separately? In other words did you try a simulation with GOME2 alone and with OMI alone and see the effect on the comparison with sondes?

- The discussion in section 4 comes very late as the info (in particular on TM5 chemistry and MSR need to be mentioned earlier.

Detailed comments:

Page 2 L1: you can add also the NOx contribution to ozone formation

Page 2 L10-L15: the 0-6 km column is also chosen because satellite measurements are not very sensitive close to the surface. So the 0-6 km column has been (historically) chosen as a compromise for a "tropospheric column" that has some DOFS$\sim$1 (although many times less than that). If you have the AK plotted, you can see that.

P2 L28: add reference for IASI ozone (Boynard et al., 2018).

P2 L35: "Since UV-VIS" instruments are not very sensitive to the height of tropospheric ozone": this sentence has no meaning, please rephrase.

P3 line 7: remind the reader here one more time that the FAT column is [0-6km]

P3 L9: you introduce MSR without further info. What is MSR and why are you using it in particular; Please move P4 L4-5 here.

P3 L27: correlation in the ozone distribution. . . Correlation with what?

P4 L 12-14: higher ozone is “a general artifact of parametrization” what does this even mean? “without any further constraints to the model” makes also no sense. Can you please explain why the model shows higher ozone in the lower troposphere. Are we overestimating precursor emissions? Is photochemistry to blame? While you cite the chemistry scheme authors (Cariolle), you can mention here how the free model simulation was previously (if any) validated, especially for ozone.

P4 L29: (same as before): were these model “artefacts” seen in other publications, were they discussed before? Any suggested reason for their source (definition of tropopause etc.)

P4 L33: “with the exception of the UTLS (around 15 km)..”: Why? Maybe the AK can help you?

P5/Fig1: since the current figure already occupies the whole page, you can add a difference plot so we see the clear contribution of the assimilated profiles.

P6 L5: the error bars on your figure are quite large and you attempt to show each station contribution in Fig 4 so I suggest to move Fig 4 and make it Fig 3. Unfortunately you don’t tell us why for example in the northern hemisphere the assimilated O3 has smaller rms. Please attempt a more in depth discussion.

(current) Fig3: NP and NML are indistinguishable in color (pink and red), make it orange? Same applies to other figures

Again Figure 5: need to discuss the figure more and put it in context with the previous figures. I don’t understand why the largest differences are in winter. I think you should present this by bands of latitude to understand where it is coming from. Please discuss more the reasons behind the assimilated O3 performance.

Figure 6 can be moved to before (after the discussion of Fig 2, or can be put in supplementary materials).

Grammatical/other minor edits:

Page 1: Abstract: change opening sentence to: “we derived global tropospheric ozone (O3) columns from assimilated O3 profiles of GOME-2A and OMI into the TM5 global chemistry transport model.

P1 L3: The horizontal model resolution is increased by a factor of six for more accurate results. To reduce. . .

P1 L6: assimilate->assimilated ozone fields

P1 L9: it turned out that -> Our results show that the residual method has large variations. . .

P2 L7,8,9 are not relevant to your study, they can be removed.

P 3 L2: [. . .] averaging kernels and the chemical ->averaging kernels in a chemical

P4 L1: change to: TM5 was used in two runs: a free model . . ..

P4 L18: and anthropogenic *precursors* emissions

P4 L20: NO2->NOx

P6 L7: ozone sonde station *location*.

Please read carefully the rest of the paper for other mistakes...

References :

Boynard, A., Hurtmans, D., Garane, K., Goutail, F., Hadji-Lazaro, J., Koukouli, M. E., Wespes, C., Vigouroux, C., Keppens, A., Pommereau, J.-P., Pazmino, A., Balis, D., Loyola, D., Valks, P., Sussmann, R., Smale, D., Coheur, P.-F., and Clerbaux, C.: Validation of the IASI FORLI/EUMETSAT ozone products using satellite (GOME-2), ground-based (Brewer–Dobson, SAOZ, FTIR) and ozonesonde measurements, Atmos. Meas. Tech., 11, 5125-5152, https://doi.org/10.5194/amt-11-5125-2018, 2018.

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