Atmos. Chem. Phys. Discuss., 9, C3187–C3193, 2009
www.atmos-chem-phys-discuss.net/9/C3187/2009/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Evaluation of tropospheric ozone columns derived from assimilated GOME ozone profile observations" *by* A. T. J. de Laat et al.

J. de Laat

laatdej@knmi.nl

Received and published: 23 July 2009

Response to referee #1 report.

Response: We thank the referee for his or her constructive comments, and have prepared a detailed response.

The paper makes several statements that need to be clarified and/or quantified. These are detailed in the specific issues below.

Furthermore, I think it would be useful to readers if the authors could provide some discussion on whether the data assimilation procedure "adds value" or not: viz., discuss

the differences between using/not using assimilation. My reading of the paper is that it is not generally the case that assimilation improves on the model. I think this is worthy of discussion.

Response: This is a very good suggestion by the author. We felt that for discussing the added value of the assimilation the summary/discussion section is the most appropriate section, rather than discussing the added value in other sections. We added a discussion about the added value of assimilation of GOM3 O3 profile, noting that with the GOME O3 profiles the only real improvement occurs for the long term changes in stratospheric O3 outside of the tropics, which typically for transport models show a tendency to accumulate O3 due to an artificially enhanced stratospheric circulation. No added value occurs elsewhere, neither in the tropics – which is an indication that the CARIOLLE scheme does - not unexpectedly – a good job in simulating tropical stratospheric O3, nor outside of the tropics where only improvement could be expected from observations that can resolve the spatial variations of UTLS O3, something which the GOME O3 profiles are incapable of due to a too coarse horizontal resolution. We added one line about the added value of the assimilation in the abstract.

P. 11812, Abstract: Mention the issue of "added value".

Response: A brief discussion of the added value of the assimilation has been added.

Specific points: L. 21: "improvements" in what?

Response: "modeling the vertical distribution of O3 . . .". Text modified accordingly

P. 11813, L. 20: I think the statement about "spatially dense" data is misleading. Satellite observations can have quasi-global coverage, but may not be spatially dense, in particular if the horizontal spatial resolution is coarse. Perhaps the authors could clarify what they mean.

Response: We meant a global observational network. Text modified accordingly.

P. 11814, L. 11: define level-2.

Response: We removed the reference to “level-2”, which is superfluous.

Line 19-20: Where is this “relevant”? The UTLS? Does the dominance of dynamical processes in the UTLS mean that the use of the CARIOLLE scheme is justified?

Response: Photochemical processes are also important in the lower troposphere and dominate O3 in the stratosphere above 25-30 km. This is now mentioned in the text. This indeed justifies the use of the CARIOLLE scheme in this application, which we emphasized a bit more at the beginning of section 4.1.1

Line 23-24: How do the weather analyses provide the best information about the separation between the stratosphere and the troposphere?

Response: There is actually no way to tell, hence this remark is superfluous and has been removed.

P. 11815, L. 1: Provide references describing the TORA method.

Response: There is a slight misunderstanding. We simply defined the method whereby the assimilated stratospheric O3 column is subtracted from total column observations as the TORA method. To clarify, this sentence is put directly after lines 24-26 on page 11814, so that is clear where the expression ‘TORA’ refers to.

P. 11817, L. 13: Quantify “good agreement”.

Response: This is an error from our side. The comparison in the troposphere between GOME and sonde/microwave observations is worse, in particular in the tropics. Sentence was rephrased:

“In the troposphere the agreement is reasonable outside of the (sub)tropics with biases of up to 25% and yet worse within the (sub)tropics. After 1998 effects of the instrument degradation as described above further degrade the comparison in the (sub)tropics.”

Line 14: Give latitude range for the “tropics”.

Response: The latitude range has been added.

P. 11818, L. 4: This comment about the analysis is in principle true, but shortcomings in the data assimilation methodology generally mean that the solution is sub-optimal. Perhaps the statement could be qualified?

Response: The referee is correct. What was meant to state here is that the assimilation assures the availability of a stratospheric O3 column everywhere even if no O3 profile observations was present. Whether or not the stratospheric column is accurate enough is a priori not known and should be evaluated. Hence this sentence has been rephrased.

Response: “The assimilation ensures that for every total O3 column observation a value for the stratospheric O3 column can be derived even in case no co-located O3 profile observation is available.”

P. 11820, L. 14: Quantify “good agreement”.

Response: The quantification of the agreement is later in this section extensively discussed (see also Table 1). For clarity, we modified the sentence to indicate based on what variations we use for determining the agreement.

“The phase and amplitude of seasonal cycles – as also reflected in the monthly means - and synoptical scale short-term variability are similar.”

Line 16: Identify the individual days where these “considerable differences” occur. If the list is long, perhaps provide a few examples to aid the reader.

Response: We removed the phrase “considerable”, rather noting that for individual days differences do occur, like for example for Payerne at the beginning of 1999 (as example added to the document). It is beyond the scope of the paper to discuss all day-to-day differences in detail. Rather, we discuss them in order to explain that one could expect to find certain day-to-day differences related to the effects described later on.

Line 20-24: Please quantify statements or, at least, refer to (I think) Table 1.

Response: This sentence has been moved up to follow explanation of what we use for determining agreement (see previous questions). Reference has been made to table 1.

P. 11821, L. 3: Quantify the "positive model bias".

Response: Clarified ($\sim$ 5 DU), reference to table 1 included.

Line 12: By "near perfect" do you mean near 1?

Response: Yes, has been clarified in the text.

P. 11822, L. 24: Quantify "a bit worse".

Response: Correlations are between 0 and 0.1 smaller. Included in the text.

Sections 4.3/4.4 would be a good place to discuss the issue of whether the assimilation "adds value" to the model.

Response: We decided that a discussion about the added value of the assimilation would be more appropriate for the summary and discussion (see begin of our response), rather than discussing it within the sections describing the results.

P. 11824, L. 25-26: Note that the assimilation does not actually correct the bias in the model, as this bias still remains.

Response: Noted. Although we did express this in terms of corrections, we nevertheless changed the phrase to "the assimilation counteracts the model drift", which we feel is a more accurate description of what is happening.

P. 11825, L. 8: Quantify "very well".

Response: The phrase "very well" is not used in quantitative manner here. To avoid confusion we deleted it.

P. 11826, L. 3-6: I think it would help the reader if the authors commented on the results when using the assimilated products (does the assimilation "add value"?).

Response: See previous discussion on "added value".

P. 11829, L. 7: Can you comment on the implications of Schoeberl's results on the results of your paper?

Response: Yes. The main differences between Schoeberl's method and the TORA method is the stratospheric observations used and the spatial resolution of the model, which in case of Schoeberl's method is 1.25x1 degree lon-lat. Note that the actual transport in the Schoeberl method uses trajectories and thus can have a much finer spatial resolution. Schoeberl's results suggest that valuable information about tropospheric O3 using residual methods can be derived as long as the spatial resolution of the model is sufficiently small to resolve synoptical scale variability, for which the 3x2 TM5 grid may be too coarse. We expect that we can improve the results by going to a higher spatial resolution, preferably combined with O3 profile observations with a smaller spatial footprint. We added a sentence to the discussion on this issue. This section has been modified according to the discussion above.

Line 15-20: Is the number of observations to be assimilated really for a problem for what is a CTM-based assimilation? Can you quantify your statements, e.g., provide CPU time, wall-clock time for, e.g., 1 month assimilation?

Response: There is actually a more critical point with regard to computing capacity. We have done some profiling of the entire assimilation chain, and the inversion of the covariance matrix for what we call the "super observation" consumes about half of the CPU time. Increasing the model resolution by a factor will increase the size of the covariance matrix quadratically, hence it could be that going from 3x2 to 1x1 degrees TM5 model resolution increases inversion of the covariance matrix accordingly (36 times), although it is a priori not clear if this really would occur since the inversion is solved iteratively (using standard mathematical packages), which may be rather efficient in

doing this inversion. The effect of more observational data is confined to the reading of the data, which only scales linearly (but still is rather time consuming) Nevertheless, while we felt that it was interesting to discuss some drawbacks, it was too technical for this paper. Hence, we removed the discussion.

P. 11838, Fig. 2: Difficult to see text inside the figures. The caption needs rewording to identify clearly which panels refer to the monthly means and which ones do not. Do the panels not describing monthly means describe daily means/values at 1200UTC?

Response: The figures have been enlarged, and the figure captions have been adjusted.

P. 11839, Fig. 3: It would be helpful to mention in the caption what do the positive/ negative differences indicate.

Response: The figure caption has been adjusted.

P. 11840, Fig. 4: Indicate in the caption which panels correspond to Payerne (left?) and which correspond to Ascension (right?).

Response: The figure caption has been adjusted.

P. 11841, Fig. 5: It would be helpful to mention in the caption what do the positive/negative differences indicate.

Response: Figure 5 does not show differences but absolute values. This has been clarified.

Technical points:

P. 11815, L.16-17: Should be: “Medium-Range” P. 11826, L. 27: “According” is superfluous.

Response: both have been corrected.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 11811, 2009.