Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2017-734-RC1, 2017

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Simultaneous assimilation of ozone profiles from multiple UV-VIS satellite instruments" *by* Jacob van Peet et al.

Anonymous Referee #2

Received and published: 9 October 2017

General comment

The manuscript describes the creation of a data set with 3D ozone concentration fields by assimilation of satellite retrieved ozone profiles in a global CTM.

As mentioned in the manuscript, ozone is regarded an "Essential Climate Variable" by the WMO, and therefore each method that provides better insight in the global distribution and temporal variability of ozone is relevant. Although assimilation of satellite retrieved ozone observations is not new, the study presented here has a number of novel aspects. These include the focus on ozone profiles rather than total columns, and the combined assimilation of profiles from two different instruments.

Printer-friendly version

Discussion paper

The description of the assimilation method takes most of the manuscript. This description seems complete: it briefly describes all relevant aspects such as the ozone profiles, the simulation model, and the assimilation method. The parameterization of the covariance matrices needed for the assimilation are then described more extensively, which is relevant since these dominate the result of the assimilation. The plots shows various intersections of the correlations are very illustrative.

To my opinion, the main deficiency in the presented study is that the subject of representation error is not taken into account enough. The simulation has grid cells of about 300x200 km (at the equator), which is about 2 (GOME-2) or 20 (OMI) times larger than the satellite footprints. Therefore, one would expect less variation in the grid cell concentrations since these are more smoothed compared to the satellite observations; this should explain part of the difference between model and measurements. Also the difference in footprint size is important for this study: the OMI pixels are about 10 times smaller than the GOME-2 pixels, and therefore one could expect the difference between OMI and model to be larger than between GOME-2 and model as far as related to pixel size. The issue is to some extend addressed:

- Page 5, line 27: *"The model grid cells are $3^o \times 2^o$, much larger than the satellite ground pixels and therefore no horizontal interpolation is needed"*. But a contribution to the representation error would make sense. And what does the model assume for the sub-gridcell concentration distribution, just completely mixed in the cell?
- Page 9, lines 14-15: *"These results include a representation error due to the grid cell size of the model. . . "*. How does error growth take representation error into account? Representation depends on the observation type (GOME-2, OMI), while there seems only a single error growth.

- Page 15, line 3: *"For these narrow-swath observations, the model is closer to the retrieved profiles"*. The feels counter intuitive, unless the observation error in narrow-swath mode is much smaller.
- Page 20, line 15-16: *"Both instruments have different horizontal resolutions, . . . "*. This issue should not be left for the discussion section only.
- Page 20, line 18-19: *"The representation error of OMI will increase towards the edges of the swath"*. But if the footprint is in better agreement with the model grid size, the representation error will be smaller. See also the comment on Page 7, line 1.

Because it is important for the result how GOME-2 and OMI observations are weighted in the assimilation, I think the representation error deserves a more extended discussion, for example as a new section 5.2 . It is not necessary to re-run the experiments, but could the authors at least give an idea of how other representation error formulations might change the results? And what would be a proper way to improve on this; could super observations help?

Finally, the case study is described very short. Why was this event chosen, is it a common test case for ozone? A bit more text would be nice, otherwise this section does not contribute much.

Specific comments

- Page 2, line 32-33: *"Second, it does not produce an estimate of the uncertainty . . . "*. Think this is formulated too strong. Depending on the optimization method also 4D-var could produce an estimate of the uncertainty in terms of the inverse

Hessian of the cost function. Also ensemble methods might produce an uncertainty estimate.

- Page 4, line 16. What are typical DFS values for GOME-2 and OMI here? On page 6, line 5 a value of *"about 5 to 6"* is mentioned, is that the same for both instruments?
- Page 6, line 7: Is the threshold 0.1 an absolute number? Or relative to the largest singular value?
- Page 7, line 1: *"... the outermost pixels are neglected, because of the large are of these pixels"*. Larger pixels would actually match better with the grid cell size, so that would be an argument to neglected the pixels in center of the field-of-view. So why neglecting the outermost pixels, higher retrieval errors maybe?
- Page 9, line 3. How is the error growth applied, as factor to the std.dev. field? What is the time t, a time step? Then better use Δt. In the formula on line 9 I see that for $t \rightarrow \infty$ then $e(t) \rightarrow a$, which from Table 1 seems to be in a range 0.22-0.34. If the error growth is a factor I would expect a value above 1.0, so that means it is an absolute value?
- Page 9, lines 15-16: *"Therefore, all collocations that are more than 3σ from the mean are discarded"*. This looks more like an outlier test? The reason for discarding is not clear: is it to reduce the standard deviation because it also includes a contribution from the representation error? But that is not taken into at all. Please clarify.
- Page 10: Table 1 would be more clear as figure.
- Page 11, Figure 3: Couldn't this be used to parameterize the error growth?

- Page 11, lines 8-10: How are the soundings extended to the top of the atmosphere?
- Page 11, line 19: Number of sondes, or number of sonde observations?
- Page 11, lines 11-14: The values presented here depend on the layer thickness, and do not make much sense therefore. Only relative numbers would be useful, DU/km or DU/Pa. Same holds for Figure 5.
- Page 14, Figure 6: OmF seems always positive, is it absolute bias maybe?
- Page 15, line 11: *"... which changes its correction parameters at the start of each year"*. This would be easy to solve, as mentioned later on page 21 at line 8.
- Page 17, lines 3-6: Text mentions specific features for GOME-2, while Figure 10 shows results for combined assimilation. How do we see the specific features?
- Page 18, line 6: *"... somewhat closer to the 1-to-1 line ..."*. I don't really see this back in the figure.
- Page 19, line 15: Is the representation error bigger on higher altitudes? But Figure 4 suggests longer length scales at higher altitudes.
- Page 21, line 16: *"... due to lack of time and resources"*. Although probably true, this remark makes more sense in a project report than a scientific journal; please reformulate.

Minor corrections

- Page 3, line 22: Start new paragraph at *"GOME-2 ..."*.

- Page 8, line 21: Start new sentence at *"Instead we parameterise . . . "*
- Page 9, lines 11-17: symbols 'a' should be in Italic font.
- Page 13, Figure 5: names "GOME-2" and "OMI" in the title would be useful.
- Page 13, line 8: pressure does not has km as units . . .
- Page 19, Figure 12: caption should mention *"validation with ozone soundings"*.

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2017-734, 2017.