

Interactive comment on “Validation of 10-year SAO OMI Ozone Profile (PROFOZ) Product Using Aura MLS Measurements” by Guanyu Huang et al.

Anonymous Referee #1

Received and published: 6 June 2017

I. General impression:

As a follow-up to initial validation work by Liu et al. in 2010, this work reports on the comparative validation with respect to AURA MLS measurements of 10 years of SAO OMI nadir ozone profile data. It thus nicely complements a recent validation exercise of the same OMI data with respect to ozonesonde measurements (Huang et al., 2017). The impact of the occurrence of a serious OMI row anomaly in January 2009 is well addressed, and the comparative analysis is insightfully adopted accordingly. The only major thing missing seems to be a clear motivation for the comparison grid that has been used (see details below). Additionally, it is believed that the clarity of the presentation of the results could be improved by slightly adopting some of the figures, and possibly by including a summary table.

C1

II. Specific comments:

In abstract/introduction and throughout the text, please mention the validated SAO algorithm/product version, as is done for the MLS data, for traceability and for comparison with the results presented in Liu et al. (2010).

Introduction, page 2, lines 35-36: The mentioning of a “suggestion that the possible affection of OMI measurements at shorter wavelengths in the UV-1 channel may have been affected by the RA at all cross-track positions” lacks any notion on how the affection could take place. This seems important however for the succeeding validation motivation. Please provide an appropriate indication of the ‘affection’ source.

Section 2: The major motivation for the use of MLS data for comparison is its location on the same platform. This instrument however measures microwave thermal emission, whereas OMI is a UV/VIS spectrometer. Please provide a brief sketch of how this wavelength mismatch might affect the comparison results.

Section 3, page 6, lines 1-3: The apparent mismatch between the 0.35 hPa to TOA (line 2) and 0.22 to 261 hPa (line 3) ranges is only explained in lines 23-28 on the same page. Please combine these paragraphs for clarity.

Section 3, page 6, lines 4-22: Although in agreement with Liu et al. (2010), the OMI and MLS profile matching and comparison approaches (still) raise some questions, especially regarding the choice of the MLS grid as the comparison grid: (1) Why opt for the MLS grid as the comparison grid, when OMI is the instrument to be validated? (2) The complementary validation paper using ozonesonde data (Huang et al., 2017) makes use of the OMI grid as the comparison grid, so comparison of results in both papers would be simplified by using the same grid (using the sonde grids would indeed not make sense). (3) Opting for the finer-resolution grid seems to contradict the OMI DFS, which is of the order of 6-7. This means that all grids with a higher resolution than six to seven layers introduce information that is not in the measurement (i.e. coming from the prior). Why then make the grid even finer than the OMI retrieval grid? (4)

C2

Taking the MLS grid as the comparison grid increases the number of operations that has to be performed on the data, hence also the uncertainties: - OMI grid comparison: MLS data to OMI grid, AK-smoothing, Comparison - MLS grid comparison: MLS data to OMI grid, AK-smoothing, MLS data to MLS grid, OMI data to MLS grid, OMI prior to MLS grid, Comparison (5) Returning to the MLS grid after smoothing might indeed make the calculation of relative differences using the MLS data as a reference less stable. The authors' choice for the OMI prior profile as reference in the relative difference denominator is legitimate but somewhat unfortunate. Therefore, (a) please provide a strong motivation for the use of the MLS grid as the comparison grid, taking into account the consequences thereof as described above, and (b) possibly add a plot that clarifies the different vertical grids and comparison ranges (as e.g. in Hubert et al. (2016) Fig. 1). An indication of the approximate OMI unit-DFS layers in this plot would be a very nice surplus.

Section 4.1, Fig. 3(e): Please provide an indication of why the OMI bias exceeds the prior bias within the 1-10 hPa range.

Section 5, page 12, lines 20-21: "The impact of using different coincidence criteria on the ozone profile comparison is negligible" is indeed correct for the different criteria considered in this work, but to what extent (spatio-temporal distance) is this true? Please briefly clarify.

Section 5, page 12, lines 24-26: "The much worse comparison at higher altitudes indicates that RA significantly affects the UV-1 channel of the OMI measurements, although they are not flagged as RA pixels." Based on these results, have you considered introducing an additional L1/L2 flagging/filtering criterion? If so, it would be helpful to mention this.

III. Technical corrections:

Introduction, page 2, line 13: Move link to references and provide access details to capture future changes?

C3

Section 2.1, page 3, lines 31-32: Please use consistent spelling for "cross track-positions" or "cross-track positions" throughout the text.

Section 3, page 6 line 6: Please provide reference to "a procedure provided by the MLS team"

Section 5, page 12, line 30: "straylight" instead of "staylight"

Presentation of results: (1) It might be helpful for the reader to include a comparison result overview table next to the plots. Rows: bias, spread for pre-RA, post-RA for pre-RA period, post-RA Columns: profiles, 100 hPa SOC, 215 hPa SOC, 261 hPa SOC And possibly: Do these numbers comply with the OMI instrument targets? (2) Figure 1: Horizontal lines could indicate the ideal collocation windows and enforced post-RA windows for clarity. By use of interpolated colors, the discreteness of the cross-track position is flawed: E.g. "red" values appear through interpolation, but are not in the data. (3) Figure 2: Vertical lines or gray areas that mark the periods that have been left out of the analysis would be helpful for interpretation of the later plots. (4) Figures 7 and 8: Standard deviations are often plotted with respect to the mean biases, but are plotted with the zero-line as a reference here. Please mention this in the figure captions. (5) Figure 9: The gray areas are not specified in the figure caption. (6) Figure 10: Please in the text or in the figure caption provide an indication of the OOM of the P-values that are now indicated as "0.00".

Interactive comment on Atmos. Meas. Tech. Discuss., doi:10.5194/amt-2017-92, 2017.

C4