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Atmospheric Measurement Techniques Discussions

Interactive comment on "Comparison of GOME-2/Metop-A ozone profiles with GOMOS, OSIRIS and MLS measurements" *by* A. Määttä et al.

Anonymous Referee #1

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General comments:

The authors present a comparison of GOME-2A ozone profiles with GOMOS, OSIRIS, and MLS atmospheric state observations, all retrieved from satellite measurements. The AMTD manuscript in its current form has a good scientific significance and scientific quality, but the presentation quality could be substantially improved: References and detailed data (manipulation) specifications often seem to be missing (see specific comments for details). The manuscript's content nevertheless fits well within the AMT journal scope, so it can be published after some important additions have been made.

Specific comments:

1. The abstract already has a very strong focus on results (second paragraph), in contrast with more common practice. It would be recommended to include a notion of the comparison studies that are addressed, cf. subsection titles of section 5, instead.

2. The authors should motivate (probably in the introduction) why only limb and occultation measurements are considered for comparison, and not any other nadir viewing instruments, which have a more similar geometry and vertical resolution.

3. One or more references are missing for the LIDORTA radiative transfer model. Consider Spurr et al. (JQSR, 2001) or van Oss and Spurr (JQSR, 2002).

4. One or more references are missing for the vertical resolution of the nadir ozone profiles retrieved by OPERA. Look for example at van Peet et al. (AMT, 2014) – mentioning a resolution of 7 to 15 km – and references therein.

5. In Section 3, no motivation whatsoever has been provided for the spatial and temporal collocation criteria that have been applied for reference measurement selection. The authors should indicate (a) whether these criteria have been taken from literature (including proper references) or own considerations, (b) what geometrical, instrumental, statistical, or other considerations these criteria are based on, (c) thereby explaining why different criteria are used for the different reference instruments, and (d) whether the resulting spatio-temporal data sampling allows for an appropriate interpretation of the comparison results. For the latter an indication of the latitude-time collocation distribution would be most instructive, accompanied by either sampling plots or references to Figs. 1 and 5-7 that are informative on this aspect as well.

6. The MLS data discussion in Section 3.3 seems somewhat disproportionate with respect to the GOMOS and OSIRIS descriptions in Sections 3.1 and 3.2, respectively. The MLS section could therefore be extended, especially on the retrieval technique and settings.

7. The comparison methodology as currently outlined in Section 4 looks rather poor: (a) The first sentence is vague, whereas critical information on unit conversion and regridding operations should be included for all reference instruments, e.g. see Keppens et al. (AMT, 2015); (b) Despite the reference to Rodgers and Connor (JGR, 2003), the prior correction for retrieval comparisons as discussed therein has not been mentioned. The authors should indicate whether this correction method is applicable and why it has (not) been used; (c) The second equality in Eq. (1) is mathematically trivial and could be omitted. It seems more appropriate to limit Eq. (1) to the expression that has actually been implemented (the other might yield slightly different results due to computational limitations); (d) No indication has been provided on the satellite data screening, whether before or after collocation. This should be added either here or in Sections 2 and 3.

8. In Section 5.1 it could be helpful and instructive to make reference to the (meridian dependence of) the tropopause and ozone maximum in the discussion of the dependence of the differences on latitude.

9. Figs. 2-4 of Section 5.2 contain many plots that seem to contain more information than what is currently discussed: The difference between AK smoothed results and direct comparisons is only mentioned in one sentence (page 7673, lines 18-19), whereas the standard deviation of the differences is not handled. It might therefore be useful to combine Figs. 2-4 into a single figure, showing the mean AK smoothed differences for all three reference instruments. Such approach also allows the reader to more easily compare the three mean difference statistics. Brief indications of the comparison spread or the effect of comparison without smoothing could then be included in the text without specific reference to a plot.

10. It would be very clarifying and therefore recommended to draw vertical lines and add version numbers in Figs. 5-7 in agreement with the OPERA retrieval algorithm version updates as mentioned at the top of page 7674.

11. Similarly, in Fig. 8 it is not obvious from the plot and caption only which grey dashed line relates to which instrument. The overpass times of the other two instruments could be added as well for completeness.

12. Could the authors provide any reason for the exceptionally high relative differences and their sharp cut-off values discussed in the last paragraph of Section 5.3? If not, that should also be mentioned, e.g. with reference to future work.

13. The last sentence of Section 5.4 raises the question of significance for the comparison results: (a) Nowhere in the text have the relative differences been compared with the (remaining) error budget of the satellite and comparison uncertainties. It is highly recommended to do so at least in Section 5.2, 5.4, or 5.5, and to add the respective random satellite errors in Table 1 for quick reference; (b) The manuscript does not make mention of the satellites' vertical sensitivities (AK matrix row sums), which could be of importance for the comparative analysis, especially for the nadir viewing instrument under study (see Rodgers (2000) book). Again, the vertical ranges of relevance, i.e. with sensitivity close to unity, could be indicated in Section 5 and Table 1. Fig. 11 for example reveals a sensitivity close to zero for the GOME-2 levels below about 10 km altitude.

14. A reference should be provided for the sentence on lines 19-20 of page 7676.

Technical corrections:

1. Neither the title nor the abstract mention that this work is on space-based "satellite" observations. Although this is clear for people from within the field who are familiar with the instrument names, it should also be so for other interested readers.

2. The MetOp-A acronym should be spelled out in the abstract and at first use within the text.

3. In the abstract on line 11 it is not clear whether the "non-degradation corrected" also applies to the previous sentence or not. This is clear for the remainder of the text, but

should also (or especially) be so for the abstract.

4. Page 7664, line 26: “since the 1970s”

5. On page 7665, line 19; the O3M SAF abbreviation should be fully spelled out at first use; the O3M part is missing.

6. Page 7665, line 29: “validation with ozonesondes shows better agreement”

7. Page 7667, line 4-5: Rewrite “to the fitting the ozone profile to the”

8. Page 7667, sentence starting at line 11: “two types of products available” and “consisting of data blocks”

9. Mention version numbers for the OPERA and MOP retrieval algorithms in Table 1. If multiple versions have been used (as for the OPERA updates), they should be indicated as well, both in the table and the relevant text parts (e.g. first two lines of page 7674).

10. Page 7668, line 23: “have been validated”

11. Page 7670, line 6: The indication of the spectral region for retrieval requires two frequencies as edges.

12. Page 7673, lines 25-26: The statement that “In the tropics, there are NO seasonal variations in the GOME-2 biases, as expected.” seems too strong and should be relaxed. Some seasonal variation can even be discerned from the plots.

13. Page 7676, line 28: Rewrite “in coarse resolution horizontal resolution”

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