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Atmospheric Measurement Techniques Discussions

Interactive comment on "Improved GOMOS/Envisat ozone retrievals in the upper troposphere and the lower stratosphere" *by* Viktoria F. Sofieva et al.

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The manuscript describes an improved version of GOMOS/Envisat ozone retrieval algorithm resulted in a significant improvement of the retrieval quality in the UTLS region in comparison to the previous retrieval version (V6). The improved data quality achieved with the new retrieval technique is clearly of a great scientific importance for UTLS studies, where significant disagreement between the measurements from different satellite instruments still persists. I absolutely agree with the statement made by the authors in the beginning of summary section that "The satellite data with a good quality in the UTLS are very important for the studies of the complex processes and long-term changes in the UTLS". In this respect, however, the authors clearly miss

the point. While a great improvement with respect to the previous retrieval version is clearly demonstrated, no attempt is made to quantify the quality of the new retrieval. To my opinion, this issue significantly reduces scientific importance of the paper. All comparisons with satellite measurements are qualitative providing no possibility to the reader to estimate the quality of the new retrieval version. While the comparisons for the older version are made for a statistical ensemble of the sonde measurements, only a couple of examples are presented for the new retrieval version. Even for the previous version the sonde comparison is strongly biased to the middle and high northern latitudes including only one sonde station in the inner tropics, where the issues in UTLS seem to be strongest. For the reasons listed above I recommend a major revision of the manuscript to include a quantitative estimation of the quality of the new retrieval version in UTLS using a representative set of ozone sonde stations.

Detailed comments:

- Page 1, line 23 (and throughout the text): The notation "aerosol-insensitive" used with respect to the new retrieval version is confusing. Actually the authors mean that the new version does not depend on the aerosol parameterization used in the retrieval rather than the fact that the retrieval is insensitive to the presence of the aerosols in UTLS. To my knowledge, the experience of the University of Saskatchewan group with both OSIRIS and OMPS data shows that the triplet method is non-negligibly sensitive to the aerosol extinction. In this sense the notation "aerosol-insensitive" is wrong.
- Page 1, line 26: The notation "horizontal column ozone densities" is not common and thus should not be used in the abstract without any additional explanations.
- Page 1, line 27: The notation "triplet ozone profiles" is not common and thus should not be used in the abstract without any additional explanations.
- Section 2.1: The selection of sonde stations is too much biased to the middle

and high northern latitudes, only one station is used in the inner tropics, no stations are used in the southern mid-latitudes. Some additional stations need to be added in these regions.

- Fig. 3: It would be interesting to see these plots for absolute altitudes rather than only relative to the tropopause.
- Fig. 3: It should be discussed why median rather than more common mean values are plotted.
- Page 6, line 26: "No ground-based measurements are available at SH middle latitudes." - Why? There is a bunch of stations at these latitudes, e.g., Broadmeadows, Lauder, Macquarie Island, Ushuaia.
- Page 9, line 20: "the aerosol extinction is linear in a relatively narrow wavelength band" - I am not really sure that 525-675 nm band can be referred to as "narrow".
- Section 4: for a quick comparison it would be nice to know which wavelengths were used in V6 and if a differential or absolute radiance was used for a spectral fit. Furthermore, a rough idea of the vertical retrieval method (2-3 sentences) would be also helpful (even if it has been already discussed in details in previous publications).
- Fig. 5 is not really necessary.
- Page 12, line 12: What is "ozone line density", is it the same as "horizontal column ozone densities" used before?
- Page 13, Eq. (6): "N" is not defined
- Page 13, Eq. (6): To my opinion only the systematic uncertainties which are different between the individual absorbing channels can be accounted for in this

way. If you agree please include the corresponding remark in text. Otherwise please explain why the systematic uncertainties are accounted for by using this formula.

- Page 15, line 10: "For tropical stations, the dramatic reduction of biases is observed." - actually only Paramaribo is a real tropical station, the other two are already in a transition region. It is clearly seen in the comparisons showing clearly different results for Paramaribo in comparison with other two stations. As the improvements are strongest in the tropical region, the robustness of the conclusions would certainly benefit if more tropical stations are included.
- Caption of Fig. 9: please explain the meaning of "16th and 84 th percentiles"
- Fig. 11: Relative deviations between the satellite measurements need to be plotted to give a quantitative estimation for the quality of the new dataset.
- Page 19, line 3: Please provide approximate altitude for 100 hPa
- Fig. 12: An additional altitude grid should be provided or pressure grid should be replaced to have the same vertical axes as in previous plots.
- Fig. 12: The goal of the figure is not clear. Indeed, one sees a clear positive bias of V6 at lover altitudes. One also sees, however, that OSIRIS and new GOMOS data are still quite different. No further conclusions can be drawn from this plot. My suggestion is to extend/replace it by a couple of 2D plots for different latitudes showing both data sets as functions of time at a particular altitude level.

Technical corrections:

- Page 18, line 15: "ITLS" $\longrightarrow$ "UTLS"
- Page 20, line 12: "NO2" $\longrightarrow$ "NO_2"

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