Dear Alexey,

Thank you very much for your valuable comments on our paper amt-2016-219 “Improved GOMOS/Envisat ozone retrievals in the upper troposphere and the lower stratosphere”, which are taken into account in the revised version.

Below we present the detailed replies to your comments. Your comments are in blue, replies are in black font.

## Major corrections

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.

The objective of the original manuscript was introducing the new GOMOS retrieval method. Therefore, the validation in the original manuscript was not extensive as possible, but informative (to identify main problems and demonstrate improvements). The V6 biases in the tropical UTLS are large and evident, as well as improvements with the new algorithm.

However, we do agree that as extensive as possible validation would be definitely beneficial and would increase the scientific importance of the paper and the corresponding dataset. Therefore, we decided to use all available ozone sonde measurements from NDACC, WOUDC and SHADOZ network (the dataset, which is used in Ozone_cci project and in

(Hubert et al., 2016)). Daan Hubert (BIRA) kindly provided the collocated dataset of ozonesonde measurements and he is now the co-author of the paper.

In the revised manuscript, the text in the sections related to ozonesonde validation (Sect. 2.2 and 5.1) is modified, and the figures in Sect 2.2. are replaced by new ones, and 2 new figures are added to Sect. 5.1.

We also included more quantitative comparisons with satellite measurements, as suggested.

However, the results and conclusions are the same as in the original manuscript (as expected).

**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.

In the revised version, the term “aerosol-insensitive” retrieval is not used.

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.

We have modified slightly the text in the abstract so that these specific terms are not used in the revised abstract.

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.

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.

As explained above, now all available ozonesonde data are used for validation.

Fig. 3: It would be interesting to see these plots for absolute altitudes rather than only relative to the tropopause.

The figure is replaced with the global distribution of biases, as a function of latitude and altitude. The distributions are presented for both absolute and relative to tropopause altitudes.

Fig. 3: It should be discussed why median rather than more common mean values are plotted.

A note is added in the beginning of Section 2.2 of the revised version.

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”.

In the content of the sentence, it is narrow compared to the whole UV-VIS wavelength range used in IPF V6 retrievals. To avoid misinterpreting, we changed “relatively narrow wavelength band” into “wavelength band of ~150 nm”

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).

We include a paragraph with more details about the GOMOS IFP V6 spectral and vertical inversion, with corresponding references, into the introduction (Sect. 1). In section 4, we refer to this description.

Fig. 5 is not really necessary.

We would prefer keeping this figure, for illustration of position of the triplet wavelength and the relation to ozone cross-sections.

Page 12, line 12: What is “ozone line density”, is it the same as “horizontal column ozone densities” used before?

Yes. The term “horizontal column density” is now used throughout the paper.

Page 13, Eq. (6): “N” is not defined

The variable is now explained.

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.

Of course, Eq. (6) assumes that systematic uncertainties are different for absorbing channels (not a constant systematic bias). In the considered case, such uncertainties might result from incomplete chromatic scintillation correction. We included a corresponding remark in the revised version.

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.

As explained above, now all available ozonesonde data are used for validation. The results and conclusions are the same.

Caption of Fig. 9: please explain the meaning of “16th and 84 th percentiles”

An explanation is added.

Fig. 11: Relative deviations between the satellite measurements need to be plotted to give a quantitative estimation for the quality of the new dataset.

The relative deviation between the satellite measurements are shown in the rightmost panel of the updated Fig.11 (Fig.13 in the revised manuscript). The previous rightmost panel has been removed according to the suggestion by Reviewer#1.

• Page 19, line 3: Please provide approximate altitude for 100 hPa

Added.

• 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.

The altitude grid is also provided in the updated version of Fig. 12 (Fig.14 in the revised manuscript).

• Fig. 12: The goal of the figure is not clear. Indeed, one sees a clear positive bias of V6 at lower 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.

Fig. 12 of the original manuscript was included in order to observe weather the seasonal cycle in tropical UTLS ozone is seen in GOMOS data. The presented figure answers fully this question. Additionally, it illustrates also the limited coverage of the UTLS by GOMOS. The presentation as 2D plots would be less visible, also because of very large bias of the V6 ozone data in the UTLS.

## Technical corrections:

• Page 18, line 15: “ITLS” →“UTLS”

• Page 20, line 12: “NO2”→ “$NO_2$”

Corrected.