Replies to Referee #2 on the manuscript 'Retrieval of ozone profiles from OMPS limb scattering observations' by C. Arosio et al.

We thank the reviewer for the time she/he spent reading the manuscript and constructively commenting on the paper. In the text below, we address the comments from the Referee #2. Referee's comments are shown in italicized font and authors' responses are highlighted in blue.

Major comments *The manuscript presents two interesting new merged satellite data sets of ozone vertical distribution, based on SAGE II, SCIAMACHY and OMPS observations. Two different methods are used for merging the data sets. The first one uses MLS data as a transfer function to evaluate the bias between SCIAMACHY and OMPS data sets, which overlap for only 2.5 months, while the second merges deseasonalized anomalies. Both data sets have the advantage of being longitudinally resolved, which is generally not the case for similar merged records except for the SWOOSH data set. Yet, the deseasonalized anomalies record when extended with SAGE II observations is zonally averaged. Ozone trends are then computed from the merged data sets using classical multilinear regression over the 2003 – 2018 and 1985 – 2018 periods for the SCIAMACHY – OMPS and SAGE II – SCIAMACHY – OMPS records respectively. The paper is well written and reference to previous work is adequate. It is suitable for publication in AMT provided that following important comments and recommendations are taken into account.*

General Comment

1. The paper is lacking an assessment by the authors themselves of which SCIAMACHY – OMPS merged record they think is best suited for their initial objective of ozone trend evaluation. Comparisons are displayed with MLS data in Figure 3 and 4 of the article, but this record is used as transfer function in both records. What is the advantage for potential users to of using one record over the other one?

There are two points raised by the reviewer.
The first one, regarding which SCIAMACHY/ OMPS-LP merged record is best suited for trend studies, was also addressed by Reviewer #1 and doesn't have a simple answer: we showed that both methods give similar results in terms of ozone trends. The advantage of the plain-debiasing approach is that it maintains the original data sets as they are, so that the resulting merged time series is expressed in terms of ozone number density and preserves the original seasonal cycle. This can be useful for example for data assimilation and model studies, for which a time series in terms of number density or VMR is more valuable than ozone anomalies. The second method involves the subtraction of the seasonal cycle and it is more suitable at altitudes and latitudes (like polar region) where the seasonality of the instruments differ more strongly. The subtraction of the seasonal cycle is a common procedure before the merging, especially when considering several satellite data sets with different observation geometry and latitude coverage. In our case SCIAMACHY and OMPS-LP observe the atmospheric scenes at a very similar scattering angle, the latitude coverage is comparable (as reported in Table 1) and the overpass time differs by 3.5 h. As a consequence we believe that the plain-debiasing method is in this case also suitable and reliable. A sentence was added in the conclusions about this point: '[The anomaly] approach is a standard procedure followed in many studies when merging several data sets; in this case, since SCIAMACHY and OMPS-LP observe the atmosphere with a very similar sampling and geometry (in terms of scattering angle), we showed that the plain-debiasing approach is also valid, with the advantage of providing a merged time series expressed in terms of ozone number density and preserving the original seasonal cycle.'
The second point relates to the use of MLS as transfer function and for comparison/validation: since MLS data set was not included in the merged record and no drift correction has been

applied, we think that it can be considered an 'independent' data set for validation. The MLS data in the merging procedure is only used to remove the systematic mean bias between the instruments. After the debiasing procedure, the mean levels of the time series are the same for the 3 instruments (at each altitude, latitude and longitude) but the ozone variability and the seasonal structure remain independent. That's why the computation of correlation and relative differences between MLS and the merged time series is justified.

2. An assessment of both records could be provided by comparing them to other independent merged records that have been produced recently, e.g. GOZCARDS, SWOOSH, and others.

Several recent merged data sets such as GOZCARDS and SWOOSH for the period after 2005 are determined by MLS observations. As a consequence, we consider the usage of these data sets for further validation not relevant, since the comparison would lead to very similar results as the ones we got using MLS only. Previous works like Harris et al., 2015 and Steinbrecht et al., 2017 already compared trends from several merged satellite data sets and illustrated the differences.

3. The issue of diurnal variation of ozone deserves some more attention in the article. It is mentioned in page 9 that diurnal ozone variation has to be accounted for above 50 km. However, Sakazaki et al (2013) found significant diurnal variation of ozone well below 50 km and down to 30 km in some latitude ranges.

The reviewer is right, Sakazaki et al., (2012) showed that also at 30–40 km daily variations of ozone play a non-marginal role. However, we did not take them into account for the following reasons. First of all, the equatorial overpass times of SCIAMACHY and OMPS are both around the noon, namely at 10:00 and 13:30 respectively. This was not clearly stated in the manuscript and this information has been added in Table 1 and remarked in a sentence in Sect. 3: 'This [considering diurnal variations] was not done in our study, because the equatorial crossing time of the two instruments is around noon and differs by only 3.5 h: this would lead to a systematic discrepancy in ozone that we estimate to be about 1-2 % at 30–40 km. Furthermore, the expected systematic bias between the two instruments is largely removed by the debiasing procedure, even though not completely, because variations with time of this systematic discrepancy may not be accounted for by a 'plain-debiasing' procedure.' Indeed, according to the mentioned paper, at altitudes between 30–40 km the ozone concentration has a minimum in the morning after dawn and a maximum in the afternoon, with an amplitude in terms of VMR of 0.15 ppmv, which corresponds to 2–3 % of the ozone at these altitudes. So, considering the satellite equatorial crossing times we expect a difference in the order of 1–2 %. Anyway, the important point to be stressed is that the effect of such a diurnal variation leads to a systematic effect on the whole time series, since the overpass time remains constant; such an offset is then largely removed by debiasing the data sets. We therefore believe that the debiasing procedure to a large extent eliminates this systematic difference between the two instruments, as long as it remains constant with time.

4. More precision is needed on the use of MLS as a transfer function for both records. What is the processing of MLS data in equations 1 and 2? Are they interpolated to the location of SCIAMACHY and OMPS observations? Similarly, not enough attention is given to differences in vertical resolution between the various data sets. Could it be an issue for the merging? In addition, ERA-Interim is used for the MLS data conversion to number density versus altitude. Did the authors test the sensitivity to other reanalyses such as MERRA2?

In Eq. 1 and 2 MLS time series as well as SCIAMACHY and OMPS-LP ones are already con-

sidered as monthly mean profiles, binned into the regularly spaced grid in terms of latitude and longitudes. Before computing monthly averages and binning the data, each single MLS profile has been converted into number density vs. altitude and interpolated onto the common vertical grid (equally spaced every 3.3 km). A sentence was added in the paper to clarify this point: 'In these and following equations, ozone profiles from each instrument are considered as binned monthly averages, interpolated to a common altitude grid.'
The issue related to the vertical resolution is interesting but difficult to remove. It has to be noted that the vertical resolution of the three sensors is similar: about 3 km for all instruments. The vertical sampling is however higher for OMPS-LP and MLS, whose ozone profiles are provided every 1 km, and is equal to 3.3 km for SCIAMACHY. This information has been added to the paper. We interpolated all single profiles from each instrument in the same vertical grid using a linear interpolation scheme. This procedure may indeed introduce artificial discrepancies between OMPS-LP and SCIAMACHY, especially at altitudes where the seasonal cycle significantly changes. To minimize this problem, we chose to perform the interpolation of the 1 km-spaced OMPS-LP and MLS profiles onto the SCIAMACHY lower vertically-resolved grid and not vice versa.
The reviewer raises another interesting point here. It was indeed not properly explained in Sect. 3 that for the conversion from MLS VMR vs. pressure profiles to number density vs. altitude, only pressure profiles from ECMWF are considered, while the temperature profiles are taken from MLS retrievals. We updated the description: 'Volume mixing ratio ozone profiles from MLS on a pressure grid are converted to geometric altitude vs. number density using collocated pressure information from the ECMWF ERA-Interim database and temperature profiles retrieved by MLS.' No issues are known by the authors about ERA-Interim pressure, so that we think that the usage of a different reanalysis product would lead to non-relevant changes. However, a comparison in terms of relative differences between MLS ozone profiles converted using ECMWF and MERRA-2 has been performed and the results have been included in the paper, in Appendix A, Fig. A1. Computing the relative difference between number density MLS zonally averaged ozone distributions over 2016 as a function of altitude and latitude, computed using the two reanalysis, we see (panel (a) of the picture) that the discrepancy increases with altitude, up to 3-5 % above 55 km, while in the lower stratosphere differences are within 1 %. In addition, the sensitivity of the ozone trends to the MLS conversion have been studied as well: the right panel of Fig. A1 in the paper reports the differences in terms of % per decade between the trends computed from the merged data set (plain-debiased approach) when using ECMWF or MERRA-2 for conversion. The differences are small even though not always negligible in the upper stratosphere: values are within -0.25 and +0.5 % at most altitudes and latitudes, approaching +1 % above 45 km at some latitudes.

Specific comments

P2-l7: The CFCs have been banned by 2010 in Article 5 developing countries.

The sentence have been reformulated as: 'the London amendment in 1990 called for a complete phase out of CFCs production by the year 2000, ...'

P2-l23: Sentence starting with N2O is a long-life GHG needs to be rephrased.

The sentence has been split in two parts for better readability.

P3-l13: What is the reference for the NASA LORE/SOLSE instrument?

Thanks, a reference was included also for LORE/SOLSE: McPeters et al. 2000.

P3-l20: Mention instruments on board SCISAT that use the solar occultation technique.

Thanks, instead of mentioning the SCISAT satellite we directly referred to the ACE-FTS and MAESTRO instruments, as we mention SAGE III instrument in the same sentence.

P3-l24: It is an improper description of the Harris et al. (2015) paper. In this paper, merged satellite records are used for trend studies but the merging is not made by the authors. Intercomparison of the merged records is made in Tummon et al. (2015).

We thank the reviewer for this note. The description of Harris et al. (2015) was changed from 'the authors considered several satellite data sets, merged them over the period 1979–2012 and examined separately...' to 'the authors considered several existing merged satellite data sets and examined separately...'

P3-l26-27: In general, provide trend results from published studies with error bars.

A better characterization of the values has been provided for each cited work, except for Harris et. al (2015), for which several uncertainties were discussed and it was difficult to summarize the results in few sentences.

P3-l30: Ozone-CCI is not the name of a record but the name of a project. More generally in this paragraph it would be better to distinguish articles that describe merged records with those retrieving ozone trends from those records.

Thanks, we changed the terminology regarding Ozone-CCI: from 'merged measurements from SAGE II with Ozone-cci and OMPS satellite data sets' to 'merged measurements from SAGE II with several other data sets homogenized within the Ozone-CCI project including OMPS-LP'. We also improved the distinction between papers merging data sets from articles retrieving trends from the merged data sets.

P4-l1: Mention the name of the method used in Ball et al. (2018).

Yes, the name dynamic linear method has been included.

P4-l23: The SWOOSH record is resolved in longitude.

Thanks for the note, "except for SWOOSH" was added.

P4-l24: Sentence starting with 'In addition': Explain why it is better not to extract the seasonal cycle.

Generally speaking it is better to extract the seasonal cycle to remove discrepancies between data sets related to the sampling, geometry, seasonality and overpassing time; in this case we performed also this approach since we consider only 2 instruments with similar characteristics in terms of geometry and sampling. No further explanation has been added at this point but we expanded the discussion of the two approaches in Sect. 3.

P5- Table1: typo on the unit of the spectral resolution. The latitude coverage could be added in the table as additional information.

The table was corrected and the information about the latitude coverage for both instruments included.

P6-l10: A short summary of validation results of OMPS and SCIAMACHY should be added here.

We agree with the reviewer. We included at this point of the paper a couple of sentences about the results of SCIAMACHY validation against ozonesondes and IUP-OMPS validation against MLS and sondes.

P7-l15: Figure 1 does not include altitude information.

Yes, it doesn't include altitude information because it just shows the number of available (retrieved) profiles from the two instruments as a function of time, without considering it as a function of altitude.

P8-Figure 2: at 28.3 km in the 40°SS-20°SS latitude range, the SCIAMACHY seasonal cycle looks very different. Can the authors comment on this discrepancy?

We studied more carefully SCIAMACHY seasonal cycle in this region and the main discrepancies are found at [40°S, 30°S] latitude, where its seasonal cycle is pretty flat after the maximum in February-March, in comparison with MLS seasonal cycle in the same period. We don't know the reason for this difference.

P9-l8-9: Equations 1 and 2 should include indices linked to latitude, longitude and altitude.

We included the indexes '(lat, lon, z)' also in equation 1.

P13-l6: Sentence starting with 'For the 50-60°SN latitude': please clarify. Why is seasonal variation handled differently in this latitude range?

The usage of heat fluxes in this latitude range was done following Gebhardt et al. 2014. At these latitudes the seasonal cycle is found to have a strong inter-annual variability that can be insufficiently modeled employing the harmonic terms. Eddy heat fluxes are related to the wave forcing influencing in turn the BDC. The explanation was added to the paper.

P13-l14: Several studies are mentioned but only one (Park et al., 2017) is cited.

Yes, we started the sentence with Park et al., avoiding the inconsistency.

P13-l21: The solar cycle is also used as a proxy in MLR regression of total ozone for trend retrieval in various studies including that from Weber et al. (2018). The solar activity has thus an impact on ozone also in the lower stratosphere. This is worth mentioning.

The discussion of this proxy has been extended as requested, including the studies of Soukharev and Hood (2006) and of Maycock et al. (2016), investigating the ozone response to the 11-year solar cycle as a function of altitude and latitude from several satellite data sets.

P17-Fig. 8: Mention for which merged data set are the trends displayed. Trend results should be restricted to the range of validity of the data.

Thanks, we specified that we are using the anomalies data set and plot the trends up to ± 70° latitude, in agreement with the specified range of validity.