Preprint amt-2022-212, RC1

Response to Giovanni Muscari (RC1):

We would like to thanks Giovanni Muscari for his positive feedback and valuable comments. Please find below the original comments, the authors' response (in blue) and the amendment made to the manuscript (*in italic*). Note that the figure and line numbers refer to the original manuscript.

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General comments:

The paper is well written and describes two important middle atmospheric ozone time series which need to be reconciled in order to understand whether they can be useful for estimating long-term ozone trends. As the authors state, more work needs to be done in order to fully establish their long term stability but I think the paper successfully proves that the two time series are extremely valuable and they are worth additional efforts to resolve the few discrepancies left. This manuscript shows the accuracy and care for the smallest details that are required when dealing with long time series and their use for monitoring small long term trends.

This manuscript should be published and I think AMT is the correct journal where it should appear. In what follows I suggest very minor revisions which might slightly improve the manuscript.

Following the suggestion of the two reviewers, the authors would like to suggest the two following main changes to the original manuscript:

Manuscript structure:

Reduction of the number of figures and reorganisation of the figure order. In particular, Fig. 3 and 4 have now been integrated into one single figure to show the error and resolution from both instrument together. The uncertainties budget figures have also been merged to keep only 2 figures, one for the low opacity case and one for the high opacity case. Last, Fig. 10 has been moved upward to avoid introducing it before Fig. 8 and 9.

Opacity:

Addition of a new figure (Figure A) in the Appendix to show the difference in opacities between the two sites. In addition, the authors would suggest to add more discussion on the opacity and its potential role in the stratospheric ozone differences between the two sites. In particular, the authors would suggest to rewrite the opacity discussion in section 4.1 as follows:

During the summertime the warmer and wetter troposphere results in a higher opacity. This attenuates the ozone spectral line and thus decreases the retrieval sensitivity during summer. As discussed in section 3.3, a higher tropospheric opacity also results in larger uncertainties in the retrieved ozone profile. In case of very hot and humid conditions, the troposphere can become optically thick at 142 GHz which can prevent the retrieval of ozone profiles. It is confirmed by Fig. A1 which shows higher tropospheric opacity in summertime than during the other seasons.

However, Fig. A1 also shows that the difference in tropospheric opacity at the two sites remains constant, independent of the season. In addition, we investigated the correlations between GROMOS and SOMORA considering only profiles measured at low tropospheric opacity ($\tau \leqslant 1$)

and did not see any significant changes in the results. For these reasons, we believe that the summer bias does not result from the higher tropospheric opacities affecting this season.

The reasons for the summer seasonal bias remains unclear but we assume that they result from seasonal temperature and humidity cycle in the troposphere. Indeed, despite controlled room temperature for both instruments, the higher summer temperature still influences room and window temperatures and consequently the instruments (e.g. receiver noise temperature). We believe that the hardware components of GROMOS and SOMORA have different sensitivity to such influences, which could explain the seasonal patterns observed in their relative differences and the lower correlation of the ozone profiles during summer.

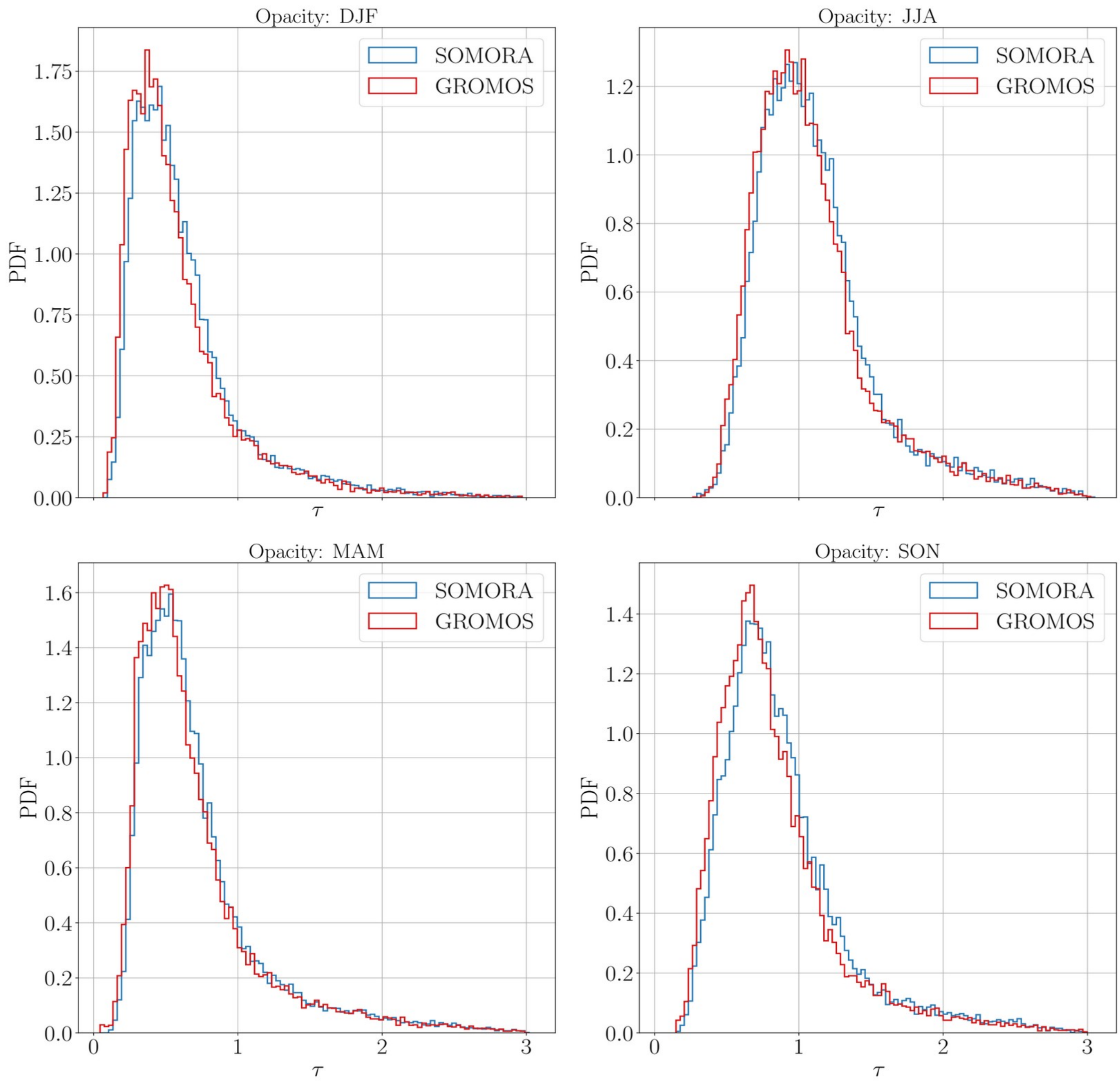

Figure A: PDF of tropospheric opacities at the 2 sites.

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Specific comments:

Line 43: It would be useful to compare opacity measurements to evaluate whether the two instruments look at portions of sky that have significant orographic/water vapor content differences

See opacity discussion above.

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Line 53: This statement would be unclear to most readers. Please clarify

We have modified this sentence and deleted the part referring to the spectrometer as it already appears later in Section 2.1. The sentence would now be:

We present and validate here the new harmonized time series for GROMOS and SOMORA focusing on the time period from the month of September 2009 until December 2021.

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Line 68: please, cite at least Parrish et al., 1988, https://doi.org/10.1029/RS023i002p00106

We have added the suggested citation.

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Line 70: this is not correct. The retrievals accuracy is highly depend on the atmospheric opacity, as it is also stated later on in this manuscript

We agree that this sentence is misleading and we have therefore rewritten this paragraph as follows:

Passive microwave radiometry uses the electromagnetic radiation emitted and transmitted in the microwave frequency region to derive geophysical quantities of interest. It makes this technique suitable for both earth's surface observation from space and sounding of atmospheric trace gases, temperature or winds from satellites or ground-based instruments. Unlike other techniques, MWRs do not require UV/VIS emitting sources (e.g. sun or stars) and are able to measure during day and night. In addition, the pressure broadening effect at microwave frequencies enables to retrieve vertical profiles of temperature, winds and abundances (e.g. Parrish et al., 1988; Connor et al., 1994; Rüfenacht et al., 2012; Krochin et al., 2022).

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Figure 1: I think it would be useful to look into the opacity measurements at the two sites, to see whether they differ significantly especially in summer, due possibly to the orography or/and local atmospheric conditions.

See opacity discussion above.

Line 93: I am surprised that the relatively small difference in Trec makes up for the large difference in spectral resolution.

It is an interesting point. Actually, the large difference in spectral resolution does not automatically comes with a significant improvement of the ozone profile retrievals. The retrieval can be limited by other factors like the receiver noise or the pressure broadening effect. In the case of these two MWRs, we believe that the sensitivity of the retrievals is limited by the receiver noise, which explains why the GROMOS higher spectral resolution does not lead to significant difference in the retrieval sensitivity.

In that sense, the sentence on line 93 was misleading and we would suggest to change it to:

As a result, GROMOS could be more sensitive to ozone at higher altitude. However, we do not see any significant difference in vertical sensitivity compared to SOMORA, possibly because of the high receiver noise, which could act as a limiting factor for extending the altitude coverage of the two instruments.

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Lines 102-107: This paragraph (lines 102-107) would be useful in the introduction of the manuscript.

We agree that part of it could be used in the introduction. We also think that part of it would introduce too much of a repetition and we therefore moved part of the paragraph in the introduction and adapted the rest to provide a small introduction to Section 3: Harmonization project.

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Line 176: where is this response described? Please, cite.

We have added the following citation where the AC240 channel response have been measured and compared to other digital spectrometers:

Murk, Axel, and Mikko Kotiranta. "Characterization of digital real-time spectrometers for radio astronomy and atmospheric remote sensing." Proceedings of the International Symposium on Space THz Technology, Gothenburg, Sweden. Vol. 15. 2019.

https://www.nrao.edu/meetings/isstt/papers/2019/2019139142.pdf

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Line 241: therefore there is dependency on atmospheric conditions

Yes, this should now be clearer in the revised manuscript.

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Line 248-249: it is not clear the difference between cross validation and direct comparison.

The original sentence was not very clear. What the authors meant, is that as the two instruments have similar observing geometries, sensitivity etc, they can be used for cross-validation without smoothing or other special comparison techniques. From there the “direct comparison”. We suggest to modify the sentence as follows:

GROMOS and SOMORA are located close to each other, have similar viewing direction, altitude range and sensitivity. Therefore, they can be used for direct cross-validation of their time series.

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Line 250: This jump to Fig. 10 before introducing and discussing figg. 8 and 9 should be avoided.

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We agree and have reorganized the manuscript in this regard.

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Line 252: do you mean 5 hPa instead of 50?

No, it was meant to be 50 hPa as the relative difference are quite consistent on this pressure range. However, the preceeding sentence was misleading and we have modified it as:

In general, GROMOS and SOMORA agree well in most of the middle atmosphere, with relative differences mostly lower than 10 % in the stratosphere and lower mesosphere (from ~50 to 0.1 hPa), increasing towards lower and higher altitudes.

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Line 267-268: one more proof that this technique is not independent from atm conditions

Yes, this should now be clearer in the revised manuscript.

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Line 270: maybe this should be a 4th region to insert in table 4

We agree and have added a line to Table 4.

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Line 271: A summer correlation of about 0.5 doesn't seem particularly good.

We have rephrased this sentence as follows:

In the stratosphere and lower mesosphere, the ozone profile are well correlated with Pearson's R coefficients mostly above 0.7 at most pressure levels and seasons. However, this is not the case during summer where we find significantly lower correlation between GROMOS and SOMORA ozone profiles.

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Line 275: this statement implies that the better correlation in winter is due to the larger variability. I think this conclusion is incorrect or at least not at all proven by fig. 9. The poor correlation of the summer period is most likely due to the high opacity of summer measurements and maybe different atmospheric conditions at the two sites. This is also why I think it would be useful to compare opacity measurements to evaluate whether the two sets of ozone measurements are carried out in similar atm conditions.

As mentioned earlier in this document we have now extended the discussion on the summer bias and the lower correlation in this section. We do not think that the higher opacity during summertime is the reason for the summer bias because even by considering only measurements performed under low opacitiy conditions, we see that same bias during summer. It can be seen in the figure below, which is essentially the same as the one in the manuscript but filtered to keep only low opacity values ($\tau < 1$):

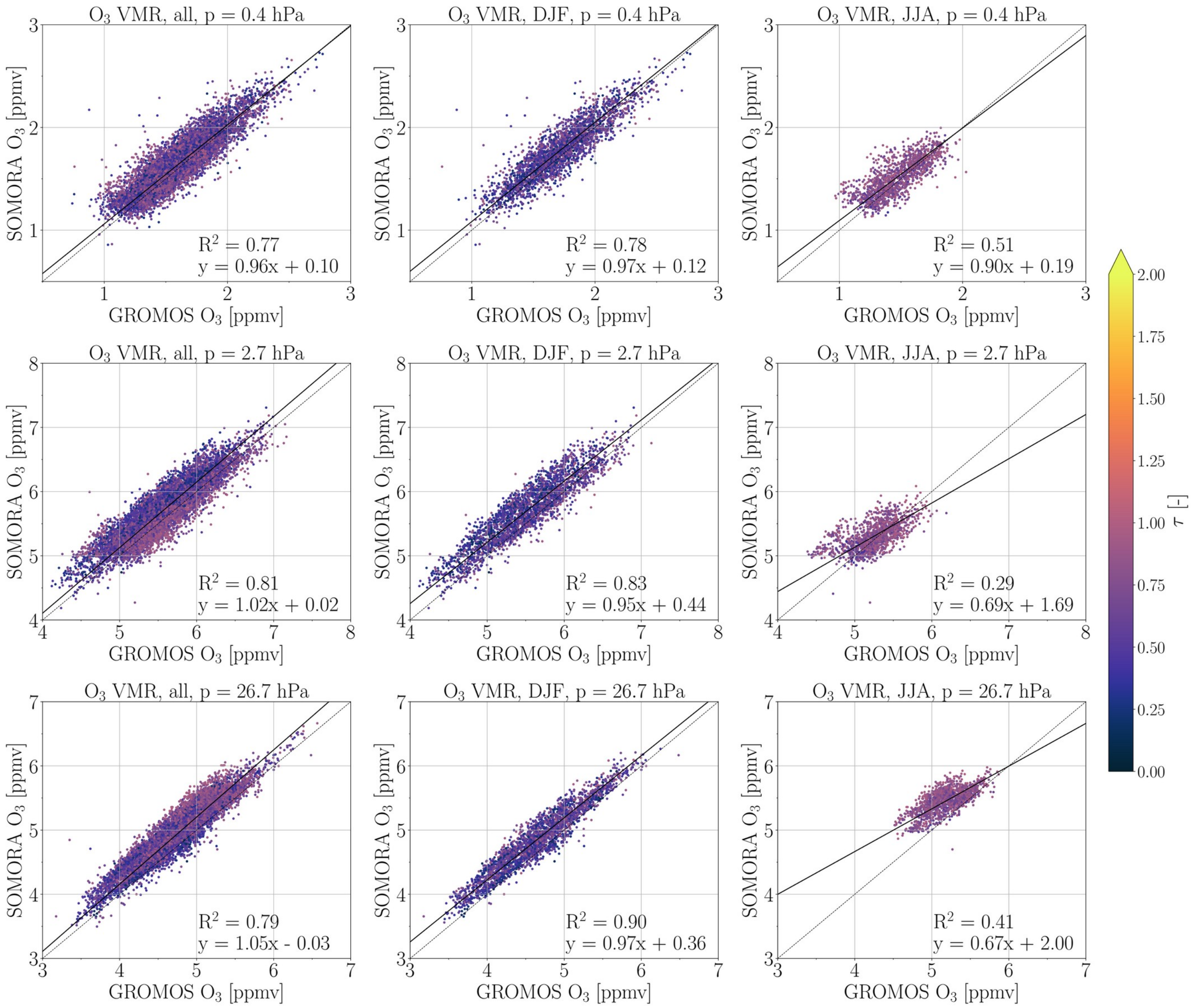

Figure B: same as Fig. 9 but filtered to keep only value measured when τ < 1

Note that the mean seasonal differences also show similar structure when we remove the high opacity measurements.

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Line 290: Here it is late in the manuscript to "introduce" Fig. 10 which has already been called several times before. The position/number of this figure should be reconsidered.

We agree and have rearranged the position of the figures in the manuscript (also following the second reviewer suggestions).

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Line 294: this sentence is unclear, please rephrase.

We have rephrased it as follows:

The differences between the previous series also showed a quite strong seasonal signal. As the processing was very different for the two instruments, in particular the way it was treating the tropospheric attenuation, it gives confidence that the remaining seasonal bias in the new data series is not an artifact introduced by the new retrieval method.

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Figure 10: I agree the new retrievals have greatly reduced the differences between the two time series. However, in the upper stratosphere the two series still show a significant difference in summer.

We would agree that a significant seasonal bias remain in the mesosphere (above 0.1 hPa) in summer but we do not think that the differences are that large below this pressure. At least they are within the uncertainties of the two datasets.

Regarding the remaining bias in the mesosphere, we are currently working at understanding their origin and believe it could originate from a GROMOS spectrometer issue because we see similar differences when we compare the GROMOS FFT dataset to the older filter banks dataset.

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Line 301: Looking at Figg. 9 and 10 I would argue that the summer periods still show large differences which will likely prevent the estimation of consistent trends. Consider removing measurements with "high" (say > 1.00) opacity values from trend calculations

We agree on this point. Actually, unlike the previous ozone retrieval routines, we chose to remove almost no spectra because of high tropospheric opacity and perform retrieval for all cases. The only filter we apply is removing 10 min calibrated spectra with opacity higher than 3 during the calibration step to avoid contamination of hourly integrated spectra. The idea was to keep the maximum amount of data from these 2 series and leave it to the final user on how to deal with these type of cases. Especially, this limits the seasonal uneven sampling obtained when filtering data by tropospheric opacity and as mentioned above, this is maybe not the main cause for the bias between the two instruments.

We agree though that this needs to be stated, maybe with screening recommendation, not only in the manuscript but also when providing the new dataset to NDACC or other databases.

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Figure 11: would it make sense to compute these trends separating winter from summer periods or remove measurements performed during high opacity conditions ?

When the time will come to compute the actual trends for the two instrument, the authors believe that it could make sense to filter out the measurement performed during high opacity conditions. However, there are also many more steps to be taken to get new trends from GROMOS and SOMORA and we believe that it it not the point of this paper to go too far into this direction. Therefore, if the reviewer agrees, the authors would prefer to keep this figure like it is.

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Line 326-329: Did you check whether your comparisons vary considerably when changing the comparison criteria?

We did not investigate thoroughly the comparison criteria in this study because it has been done already for SOMORA and MLS and we have chosen similar values as the one considered in:

Hocke, Klemens, et al. "Comparison and synergy of stratospheric ozone measurements by satellite limb sounders and the ground-based microwave radiometer SOMORA." Atmospheric chemistry and physics 7.15 (2007): 4117-4131.

We did however, investigate the day-night differences and did not see significant changes in our comparisons between MRWs and MLS, except at $p < 0.1$ hPa, where the error from both MLS and the MWRs becomes quite large.

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Line 354: This behavior cannot be evaluated from fig. 12, the time scale is too large to look at short term variations. From Fig. 12 the reader cannot observe diurnal variations nor variations that are smaller than the diurnal cycle.

We agree that it was not appropriate to mention short-term variability with this figure and we have changed the sentence as follows:

In the stratosphere, clear seasonal pattern are well captured by all dataset and the higher winter ozone variability is clearly visible at all pressure levels. On time scale of a few weeks, we can see that all four dataset are able to capture well the larger ozone variations not only in the stratosphere, but also in the mesosphere where these variations become relatively small compared to the amplitude of the ozone diurnal cycle.

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Line 372: specify not for somora in summer

We have modified the sentence as:

Both GROMOS and SOMORA show very good agreement with MLS at all seasons and altitudes, with the exception of SOMORA during summertime.

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Figure 13: Could you clarify why the GROMOS and Somora convolution processes have a different impact on MLS profiles? Does the difference depend on the selection of compared profiles or the AVKs of the two datasets are different?

Unfortunately, this is a question that we have discussed many times during our study and to which we have not yet found a satisfying answer. This is not entirely clear though, how different is the impact of the AVKs convolution on the MWRs-MLS comparisons as it seems to mostly increase the existing bias on the "direct differences".

On singles profiles, they are indeed sometimes differences between GROMOS and SOMORA averaging kernels but we did never spot any systematic differences between their AVKs. Also, after averaging of so many profiles we do not think that it can introduce significant differences as the mean seasonal AVKs between GROMOS and SOMORA are very similar.

Regarding the selection of compared profiles, we have also analyzed what happened when only collocated profiles for the two sites are taken and the results are essentially the same so this is not the issue. We have now added the following to discuss this matter at the end of Section 5.4:

It is not entirely clear why these differences are larger with the convolved MLS profiles but it does not result from sampling differences (not shown). As it seems especially visible on SOMORA in the lower stratosphere, it could potentially arise from instrumental baselines impacting the AVKs.

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Line 389-390: This sentence is not very meaningful in my opinion.

Line 391: Please specify that this is an average over the entire period, differently from figg. 13 and 14.

We agree that the sentence was not very good and according to these two comments, we have modified the paragraph as follows:

Similar comparisons between MWR and MLS has been performed at various locations (e.g. Boyd et al., 2007; Palm et al., 2010; Ryan et al., 2016) and showed similar results with the ones obtained in our study. This is confirmed by the mean ozone VMR relative differences between MWR and MLS given in Table 5 for the three pressure ranges. Averaged over these pressure ranges and on the entire time period, the differences between MLS and the MWRs are less than 5 % in the as defined stratosphere and lower mesosphere.

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Technical and typos corrections:

We have corrected the following typos identified by the reviewer:

Lines 6, 10, 57: incorrect use of "both"? Indeed, we have replaced "both" with "the two".

Lines 214 and 215: ~~its~~ corresponding altitude_ → their corresponding altitudes.

Line 246: "space-based" have been changed to "satellite-based"

Line 296: ~~Whereas~~ → Although

Line 349: *[…] and highlights the overall good agreement […]*

Line 373: "*[…] grow and show some oscillations.*"

Line 400: ~~both~~ → the two

Line 402: ~~It includes~~ →They include