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.

##
##

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.

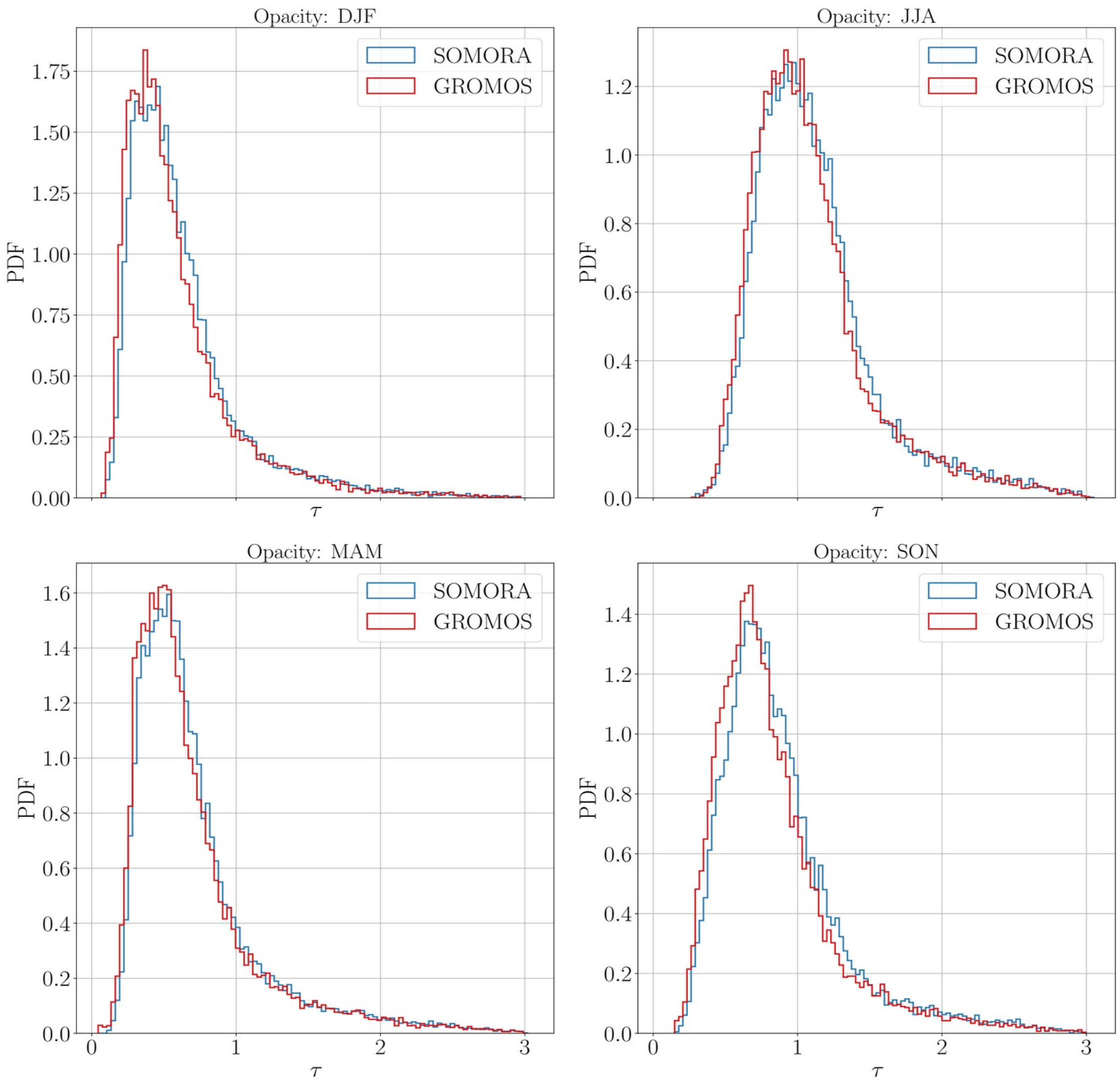

Figure A: PDF of tropospheric opacities at the 2 sites.

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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.

#####################

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.

#####################

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

We have added the suggested citation.

#####################

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).

#####################

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.

######################

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.

######################

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

######################

Line 241: therefore there is dependency on atmospheric conditions

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

######################

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.

######################

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.

######################

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.

######################

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.

######################

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

We agree and have added a line to Table 4.

######################

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.

######################

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 ($\tau < 1$):

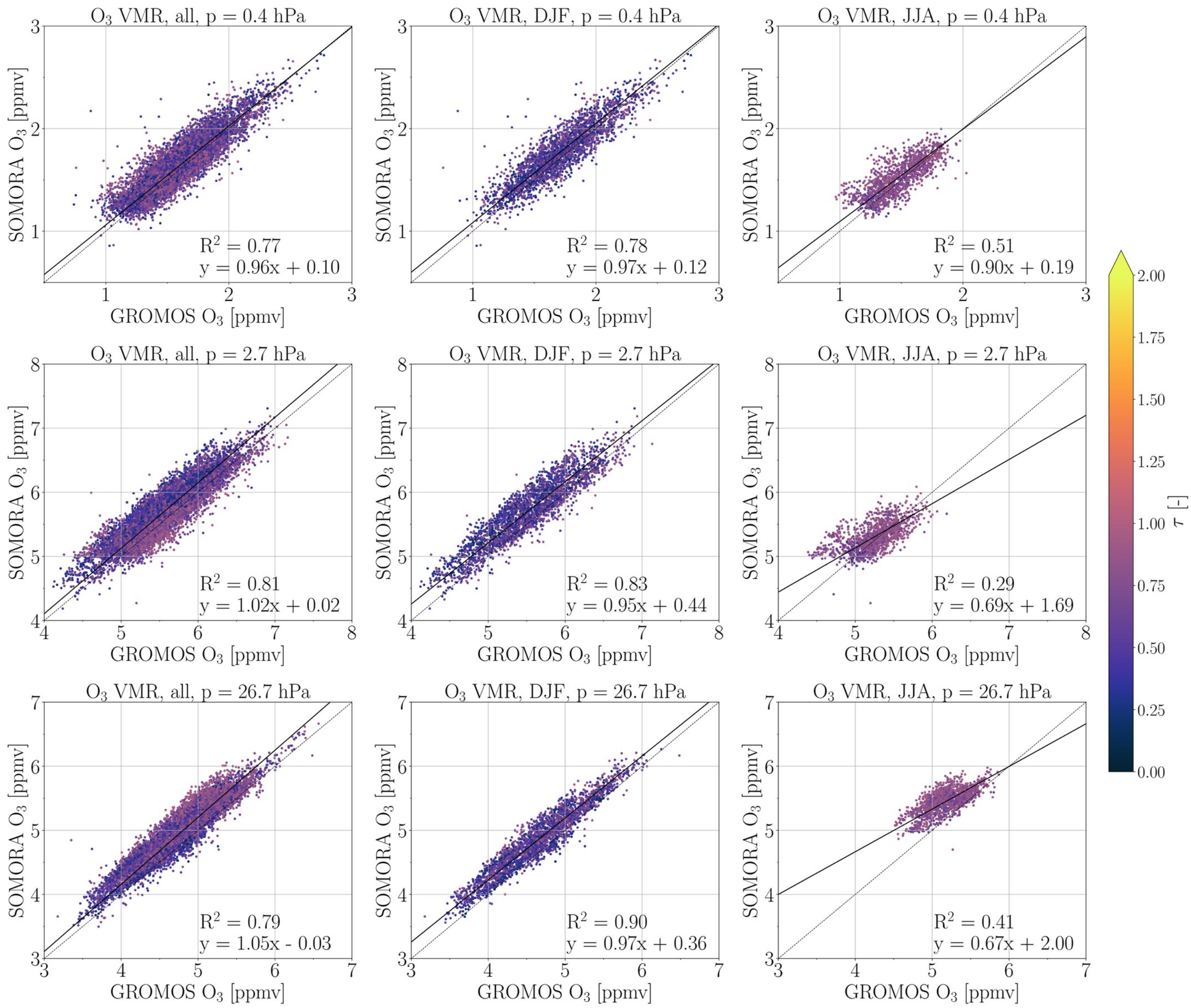

Figure B: same as Fig. 9 but filtered to keep only value measured when τ < 1

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

####################

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).

####################

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.

#####################

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.

#####################

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.

#####################

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.

#####################

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.

#####################

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.

#####################

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.

######################

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

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Response to Anonymous Referee #2 (RC2):

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

##
##

General comments:

Two similar microwave ozone instruments that have been measuring from nearby sites in Switzerland for decades, but the data has been processed with different retrieval codes. The authors do a nice job of harmonizing these datasets. The content is appropriate for publication in AMT. Below are a number of suggestions which I hope may improve the manuscript.

Throughout this study there is a disturbing emphasis on "reducing discrepancies" or "improving the agreement" between instruments. No scientific study should ever have this goal. The goal is to harmonize various stages of the data processing so that these stages do not, by themselves, introduce differences.

Regarding the reduction of discrepancies, the authors agree with the second referee on the substance. We believe that this impression arose from sentences found in the abstract, introduction and methods and we have modified a number of them. It should help to make clear that the goal of the study was focused on the harmonization of the data processing and that no further corrections were applied on the resulting ozone profiles to "improve their agreement".

The most obvious information missing in this manuscript is any comparison of the tropospheric opacities at each site. While the sites are physically close in stratospheric terms, and the altitudes are similar, the tropospheric conditions at the two sites may be very different, yet there is no information presented on this topic. Even if the tropospheric opacities are quite similar, a small figure making this point would be nice.

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 a single figure to show the error and resolution from both instrument together. The uncertainties budget figures have also been merged together 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 extend the discussion on the opacity and its potential role in the stratospheric ozone seasonal differences between the two sites. In particular, the authors would suggest to rewrite the opacity discussion in section 4.1 as follows:

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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.

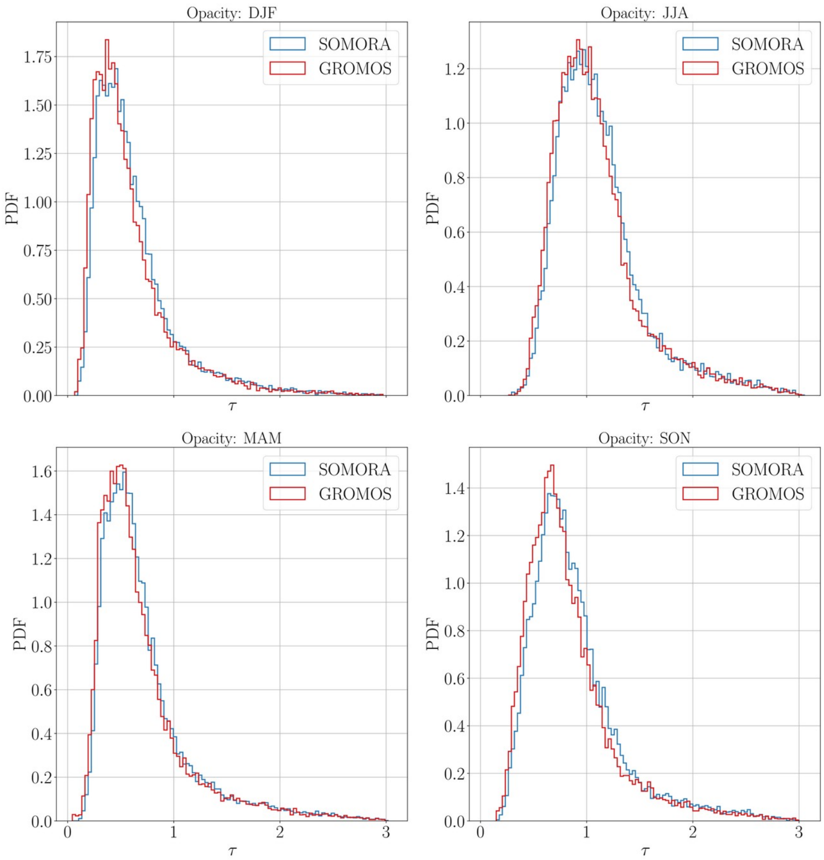

Figure C: PDF of tropospheric opacities at the 2 sites.

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Specific comments:

Line 40 – “close to each other”. Please give a physical distance somewhere in this paragraph.

We have added the physical distance between the two instruments as follows:

In Switzerland, two ozone MWRs are operated since more than 20 years in the vicinity of each other (ca. 40 km): the GROund-based Millimeter-wave Ozone Spectrometer (GROMOS) in Bern and the Stratospheric Ozone MOnitoring RAdiometer (SOMORA) in Payerne (Fig. 1).

######################

Line 69 and 70 – This sentence sounds rather odd. It sounds like you’re measuring the atmosphere with a measurement that is insensitive to the atmosphere. I assume the word troposphere belongs somewhere in here.

We agree and as suggested by both reviewer, we therefore modified the entire paragraph as:

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).

######################

Line 99 – “no way to confirm the amplitude of the effect of the bias”

We have changed this sentence as follows:

“no way to confirm the amplitude of the bias”

######################

Line 115 – “Due to their high sensitivity, the operation of microwave radiometers requires continuous calibration”.   I don’t understand this statement. Continuous calibration is required because the receivers are not perfectly stable, not because they are highly sensitive.

We agree that sensitivity was not the correct word to use here and have changed this sentence as follows:

The operation of microwave radiometers requires continuous calibration because their receivers are never perfectly stable [...]

######################

Line 134 – I assume what the authors are trying to say here is that they “provide good quality spectra for 87% and 89% of measurements”, but I’m not quite sure if that is what is meant. Please rephrase.

This sentence was indeed not very clear and we have rephrased as follows:

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Considering instrumental issues and technical interruptions for maintenance (e.g. for LN2 refilling or instrument repairs), GROMOS and SOMORA provided good quality hourly spectra for respectively 87 % and 89 % of the measurements performed between 2009 and 2021. It results in more than 80'000 hours of comparable retrieved ozone profiles.

######################

Line 186 – "a modulation"

Thank you, we corrected the typo.

######################

Paragraph starting at 195 – Have the fitted baselines been removed in the spectra shown in the following figures?

No, the baseline are still present in the measured spectrum and are included in the fitted spectrum. As a result, they are "suppressed" in the residuals. However, the examples shown in the paper did not contain high amplitude sine baselines. In Figure B below, the reviewer can find an example with higher amplitude sine baselines (with the blue line indicating the full baseline retrievals (sine + 2nd order polynomial). In this case, a sine baseline is still present in the residuals but much reduced compared to the case without baseline (Figure C)

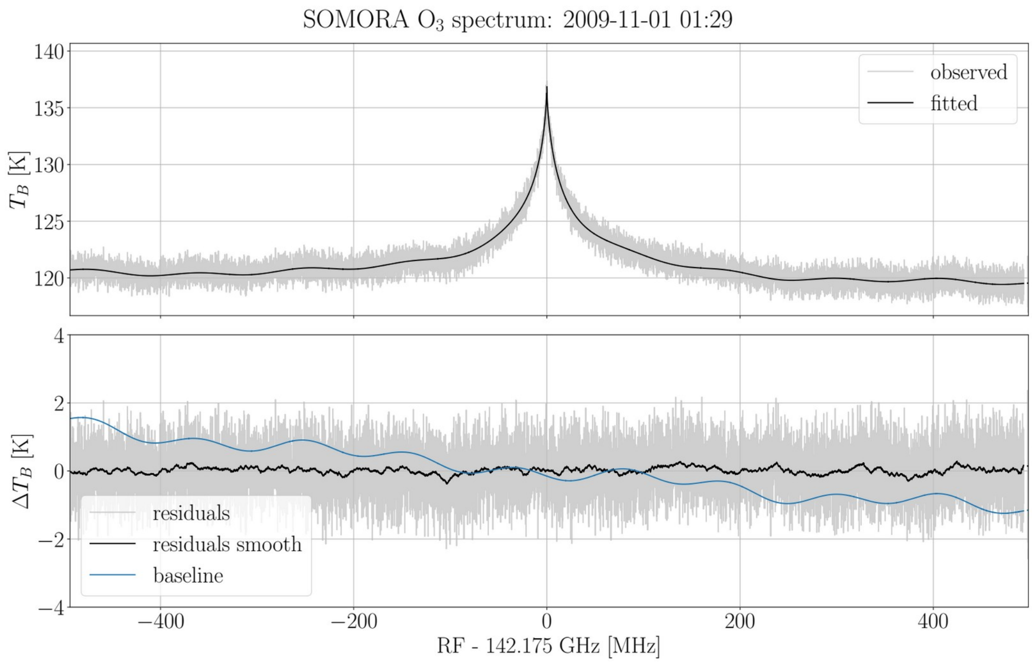

Figure D: Example of SOMORA spectrum with high sinusoidal baselines.

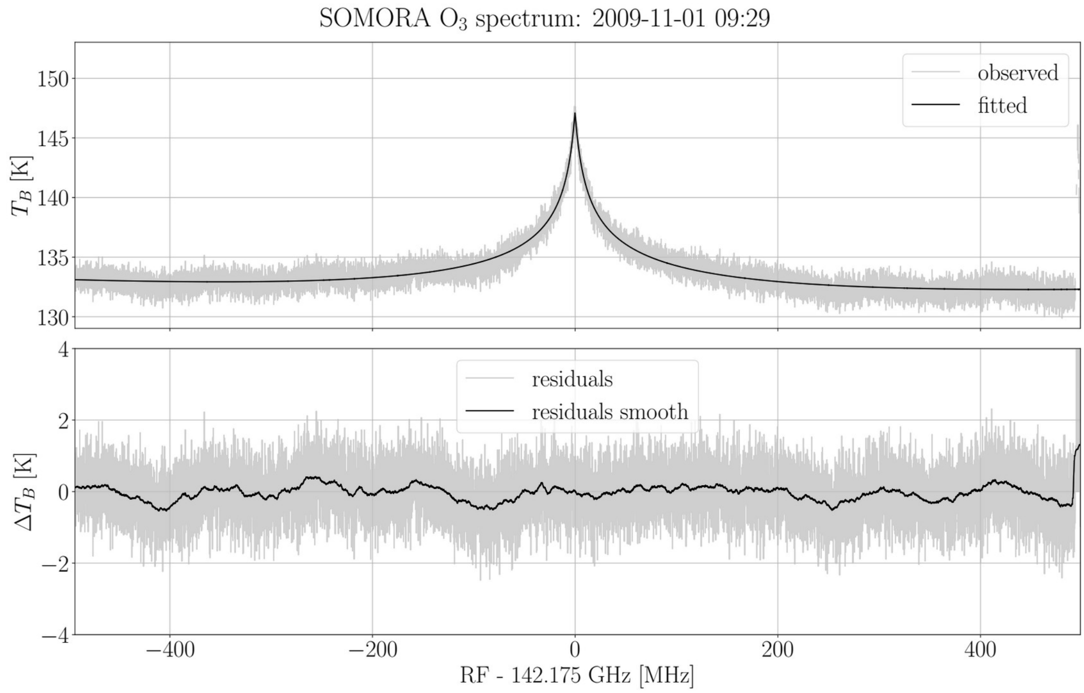

Figure E: Same as Figure B but without retrieval of the sine baselines.

######################

Figures 3 and 4 seem to be almost identical. While I realize that the authors are trying to make this point, there is no need for a two 4-panel figures to make this point. It would be nice to see the errors and resolutions of both instruments on the same plot (perhaps one with symbols and the other with a lines).

Thanks for this comment, we agree that this was not the most efficient way to visualize these data and that errors and resolutions should go into a single figure for the two instruments. We also think that it is important to show the AVKs for the two instruments even if, as mentioned later, they are more dependent on tropospheric conditions: this is a necessary condition so that we can compare the instrument directly in the rest of our manuscript.

In that sense, we would like to suggest Figure D as replacement of Figures 3 and 4:

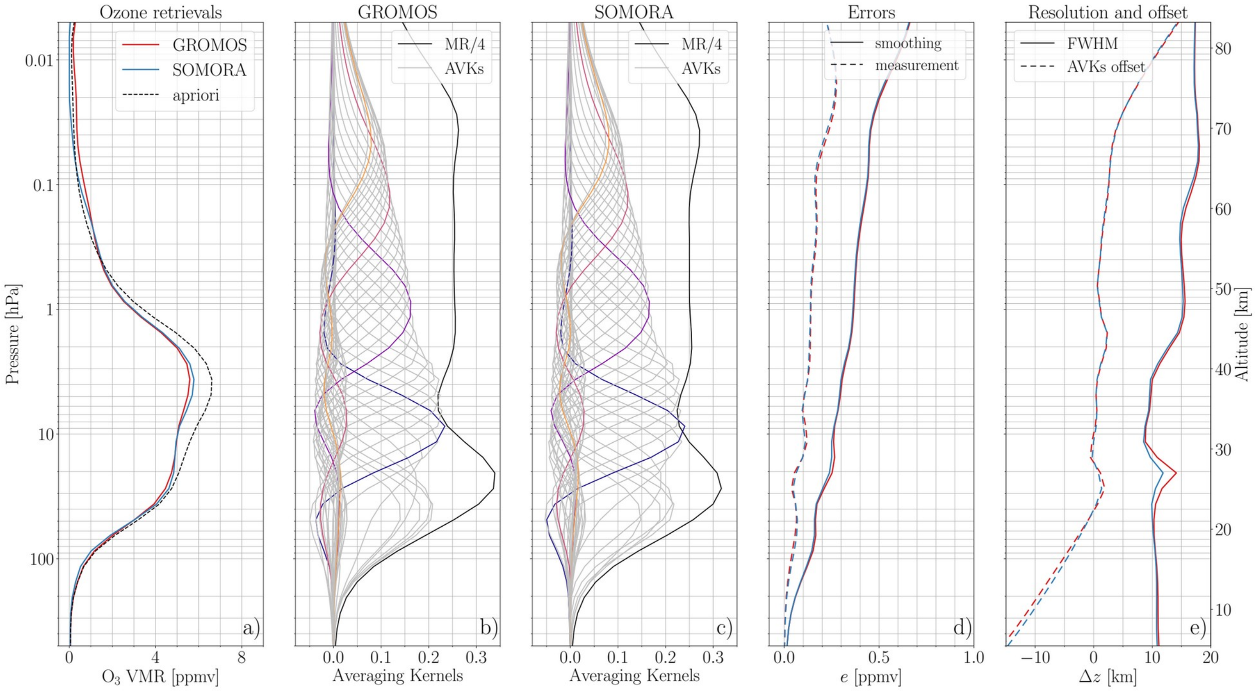

Figure F: Example of hourly retrievals diagnostics for GROMOS and SOMORA

Perhaps I've missed it, but why is there a large spectroscopy error at the top in Figure 5 but not in Figure 6?

This is hard to say why it appears on GROMOS only but it might be because the retrieved ozone profile for GROMOS is zero at these altitudes whereas the SOMORA profile still retrived a tiny amount of ozone there. Given the amplitude of the measurement and smoothing errors at this altitude, we believe that it is difficult to track the exact effect of spectroscopic parameters changes up there. In fact, the uncertainties become dominated by the measurement noise above approx. 2 hPa so that we do believe that the larger spectroscopy error appearing up there is an artifact from the GROMOS retrievals.

######################

The authors show high and low tropospheric opacity cases, but they do not mention the opacities of these cases, nor do they give any indication of how representative each case is. I assume that the difference in AVKs between high and low opacity cases is larger than that between the two instruments in the low opacity case. I don't think that there is any need to show the AVKs for both instruments in the low opacity case since the exact AVK is probably much more opacity-dependent than instrument-dependent.

We agree and have added opacities value to define our "low" and "high" opactity cases. Combined with the new figure (Figure A), it should provide a context on how representative are these two opacity cases for GROMOS and SOMORA. In addition, we suggest to extend the discussion in Section 4.1 to discuss the opacities at the two sites and the implication of this on the seasonal bias.

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Regarding the AVKs, it is correct that they are opacity dependent but we still believe that it is important to show the AVKs from the two instruments. In the original manuscript, we showed two examples of AVKs corresponding to rather low opacity (τ=0.4). We did not mention the opacity of the cases shown and did not discuss the effect of higher opacities on the AVKs. Therefore, we also suggest to add the opacity value to the retrieval diagnostics (Figure D) and discuss the implication of higher opacity on the AVKs and the retrievals in general in section 3.3.

#####################

Figure 12 would be much more helpful if it were deseasonalized. Or perhaps just shrink the y-axes ranges a bit to make it easier to distinguish the lines.

We agree that the original figure was a bit difficult to read and as recommended, we suggest to shrink the y-axis and modify the temporal resolution (1 week instead of 2 days) of the time series to get Figure E as a replacement for Fig. 12. We also did a deseasonalized version (Figure F) but we think that the good capture of the seasonal cycle by the two instruments is important and therefore we would prefer to keep Figure E.

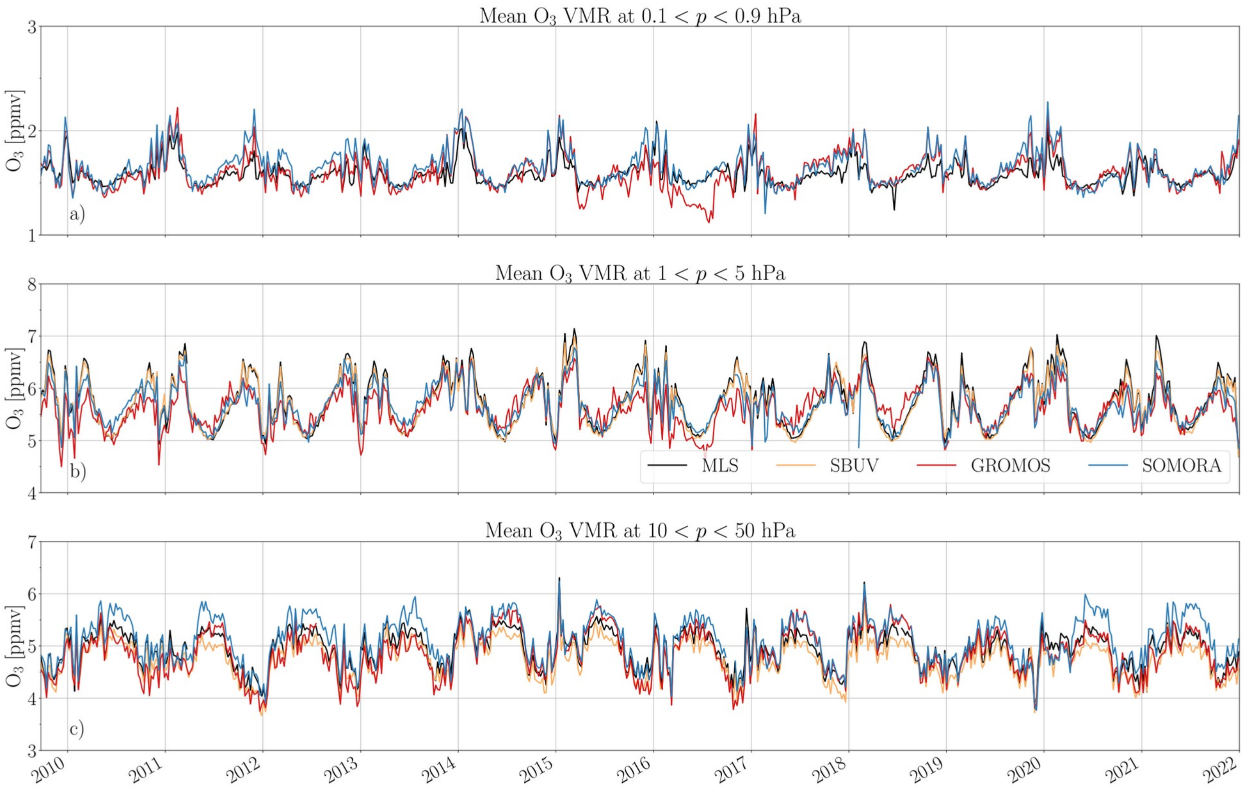

Figure G: Ozone time series, now weekly averaged for clarity.

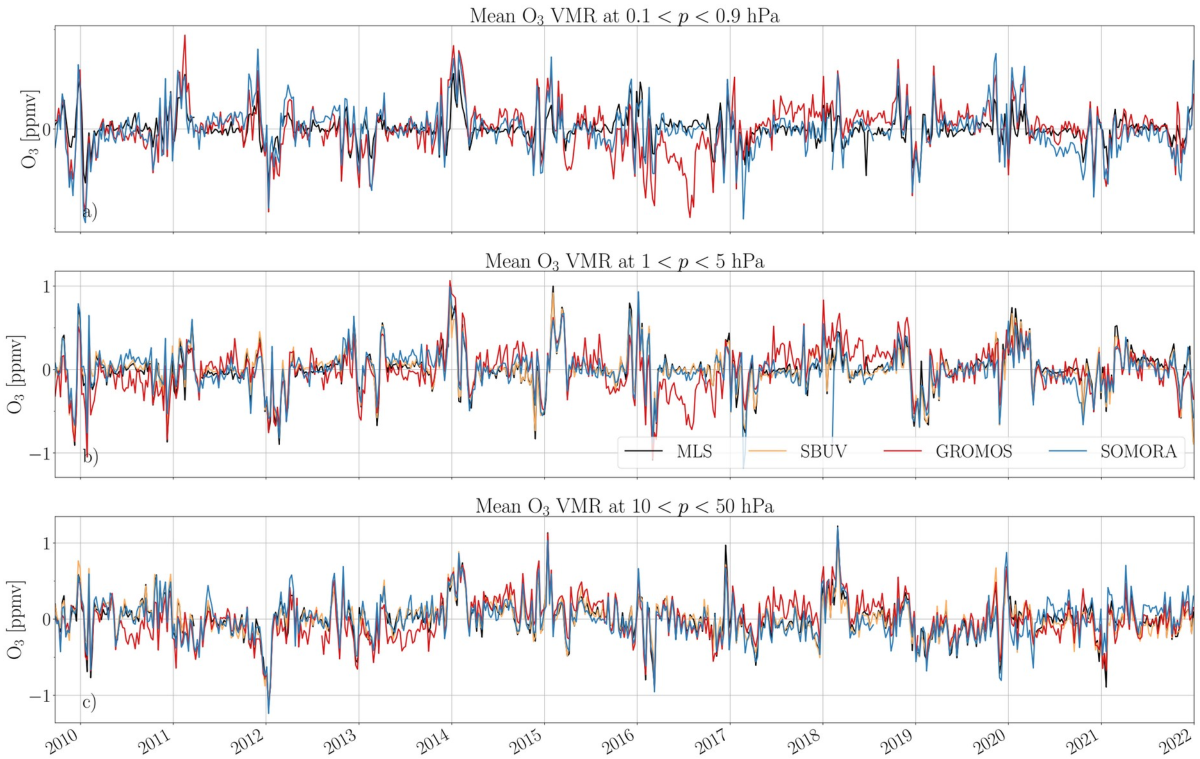

Figure H: Same as Figure D but deseasonalized.

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Are the comparisons in Figure 13 with both the daytime and nighttime MLS overpasses? Are the differences the same for both? Why are there more profiles in the unconvolved case than then convolved case?

Yes, Fig. 13, 14, B1 and B2 do not differentiate between daytime and nighttime MLS overpasses. We initially did the analysis with daytime and nighttime differences but did not see large differences, except at $p < 0.1$ hPa, where the error from both MLS and the MWRs becomes quite large.

The fact that there are more profiles available in the unconvolved cases results from numerical errors in some of the averaging kernels, producing extremely large single values at 1 or 2 altitudes. While this is not a problem for the basic comparisons, it becomes a problem during the convolution of the AVK with the corresponding MLS profile. A solution could be the interpolation of the concerned AVK but given the number of available profiles, the authors took the decision to simply filter these out which explains why the number of MLS convolved comparisons is slightly lower than the direct comparisons.

######################

Given that the error in Tprofile is the dominant error in Figures A1 and A2, some explanation about this would be of interest. I assume it has something to do with the calculation of opacity?

The authors agree and would suggest to rewrite as follows the paragraph of Section 3.3 where the differences between the low and high opacity cases are discussed:

In the case of high tropospheric opacity, the ozone emission line gets more attenuated by the tropospheric water vapor absorption. The AVKs gets degraded, reducing the sensitivity of the retrievals and leading to higher uncertainties than at lower opacities. As can be seen on Fig. B1, the atmospheric temperature profile becomes the dominant contribution to the uncertainties below 1 hPa at higher opacity. It is likely due to the increased importance of the water vapor continuum retrieval, which is itself strongly dependent on tropospheric humidity and temperature. In the higher opacity case, the total relative uncertainty on GROMOS in the stratosphere is 12-15% respectively 10-12% for SOMORA.