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Response to reviewers comments on: Interrelated variations of O_3, CO and deep convection in the tropical/subtropical upper troposphere observed by the Aura Microwave Limb Sounder (MLS) during 2004 – 2011

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1 Introduction

We thank the reviewers for their careful read of our manuscript and for their helpful comments and suggestions for improvement. As detailed below we have enacted changes based on their comments.

2 Notes about the figures

2.1 "Timeseries" plots

One area where we have opted to vary a little from the reviewers' understandable suggestions for improvement, while hopefully keeping in the spirit of their requests, is in the layout of Figures 1 – 4 of the manuscript. We completely agree that the figures, as presented in the ACPD version of the paper, are hard to navigate and could be better organized. This was an inevitable consequence of the layout imposed by the ACPD page format. Our intent has always been that, in the final manuscript, the Figures are presented full page with all three panels together, ideally placed on facing pages in any printed version. To illustrate we have included a (highly reduced) mock up of the first pair as Figure 1 in this response. We hope the reviewers agree that this presents a clearer arrangement of the figures that will alleviate many of the concerns they justifiably express.

2.2 "Scatter" plots

Both reviewers highlighted the challenges of interpreting our discussion of inter-species correlations. This is both in the context of the timeseries plots (discussed in sections 4.1 to 4.3 of the manuscript, and in section 2.1 of this reply, above) and of the "scatter" plots (discussed in manuscript section 4.4).

For the scatter plots themselves, we agree with both reviewers that the use of symbol size to denote IWC is not informative. Accordingly, we have split these figures so that O_3/IWC and O_3/CO correlations are shown separately as their own x/y scatter plots, avoiding the use of symbol size as a third "axis". In light of that, and in response to their related comments, we have introduced a specific discussion of quantitative O_3/IWC correlations as a new paragraph (the second) in section 4.4 of the paper.

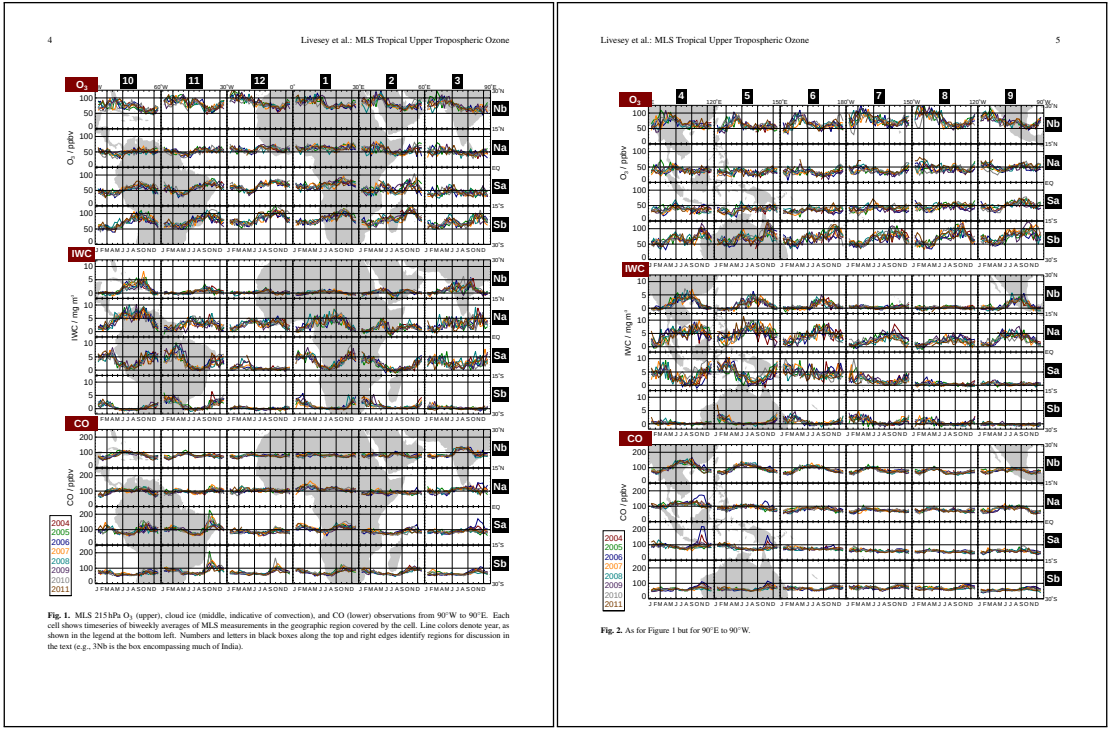

Fig. 1. Figures 1 and 2 of the paper, highly reduced, as they are intended to appear on facing pages of the final print version of the paper.

3 Response to specific reviewer comments

In the following discussions, the reviewers' comments are reprinted in their entirety *in italics* with our responses interspersed in upright font.

3.1 Response to reviewer #1

Interrelated variations of ozone, CO, and deep convection in the tropical/subtropical upper troposphere observed by the Aura MLS during 2004–2001 (Livesey, 2012)

Author presents 7 years of 215 hpa MLS ozone, CO, and IWC data in a compact fashion through the use of novel plots. Possible causes of interannual and seasonal variations in regional amounts are discussed with plentiful references to more detailed analyses. Data set is of great interest to scientific community.

18672L26: Lofting of ozone poor lower $\rightarrow$ *Lofting of lower*

Done.

18674L10: Why are Microwave signals less affected by clouds?

This is because the wavelength is longer than the typical particle size, so scattering is significantly reduced. Words to that effect have been added in the text.

18675L1: "Identifies some behavior not previously reported" These new findings do not stand out in the text. Consider recapping any new findings in a discussion section or the final summary. This "unanswered questions" section can also be used to advertise this data set.

We agree that such a discussion would help. Rather than put it in a separate section, we have recapped these points in a new paragraph at the end of the summary section.

18675L19: 10–20% agreement $\rightarrow$ *10–20% biases*

Done.

18676-18677: Different terminology is used in describing the vertical resolution of the ozone, CO, and IWC values. Be consistent. What do you mean by "full width at half maximum of 3 km" for ozone, 5 km vertical resolution for CO, and "effective vertical resolution of 4 km" for IWC.

This inconsistency was deliberate as averaging kernels are not produced for the cloud ice product, so the same metrics are not applicable. We agree that the text was unclear on this point and the discussion has been revised.

18677: What an exciting data set! Are these bi-weekly data sets available to the scientific community? If yes, make this clear, if no, double check and make sure enough information is available in this paper and its predecessor for others to reconstruct this data set

This derived biweekly-average dataset is not currently distributed to the wider MLS user community as an MLS "standard product". It has been shared upon request, and will continue to be so. The description given in this paper and in the cited data users' guide does indeed give sufficient information to reproduce the results.

Figures 1–4. Flipping between the various Figures was tedious. I suggest you change Figures 1–4 into Figures 1–6 with Figure 1a/1b and Figure 4a/4b being ozone, Figure 2a/2b and Figure 5a/5b being CO etc. This change would make it much easier to look at wave one variations for eg. Of course it would make it harder to compare crossspecies variations but most of these comparisons are also shown with the correlation plots.

This issue is discussed in section 2.1 of our response, above.

18681: When comparing with sondes, you do not apply an averaging kernel to the sondes. What should users do when comparing to upper tropospheric model output?

As described in the text, application of the kernels makes little difference to the sonde comparisons. While, strictly speaking, users should apply the averaging kernel matrix in comparisons to sondes/models etc., in our experience this makes little difference in this region. At higher altitudes, and/or for other species, where the resolution is coarser (e.g., $>$5 km), the kernels have a more significant impact and should ideally be factored in to the analysis. We have added a brief mention of the application of the averaging kernels in the summary section at the end of the paper, where using these data for model validation is suggested.

18678L14-18: Why do you compare the sub-tropical sonde observations of Thompson to tropical MLS data here? Where were the sub-tropical observations taken? Why do they show a different timing?

The sondes are indeed more 'tropical' than 'subtropical' in location, we have changed the wording accordingly. The Thompson figure uses data from nine sonde stations, six of which are within our 'Sa' band (15°S to the equator). The exceptions are Reunion (21°S) and Fiji and Tahiti (both at 18°S).
Also, apologies for incorrectly describing the sonde-reported seasonality. The minimum observed by Thompson et al. is during March to May, (much more consistent with the MLS findings) not September to November as stated in our submitted manuscript. We have corrected the text. Many thanks for highlighting the inconsistency.

Are MLS data available at higher pressures (lower altitudes) than 215 hPa or is this the highest pressure for which reasonably accurate values are possible?

For MLS v3.3 CO and IWC, 215 hPa is the largest pressure (lowest altitude) for which the data are valid, while MLS v3.3 O_3 are valid at pressures of 261 hPa and less ($\sim$1.5 km lower in altitude than for CO/IWC). We have clarified this point in the manuscript in the discussion of each product.

18684: Anti-correlations between ozone and convection. Might be useful to summarize

these regional correlations with a plot or table. Tedious work moving back and forth between the plots.

A new figure quantifying the O_3/IWC correlations has been added that, along with its accompanying discussion and the final layout of the timeseries figures, hopefully addresses this comment. Please also see the comments in section 2.2 of this reply, above.

18684: Difference in correlations between adjacent regions. You binned the MLS observations into rigid longitude/latitude regions. How much additional insight could be obtained by more judicious partitioning? Thoughts?

It is certainly possible that, in some cases, more focused, customized, regions might elucidate such correlations more. We feel that such analysis is beyond the scope of this paper, but agree that the point should be made. We have included it at the end of the discussion in section 4.4 on quantitative correlations.

18685: Doesnt 6Nb show non-negligible values of IWC?

Good point, though only in summer, not in spring. The text has been modified to clarify this point.

18687L5: This presumably new finding is an example of something that could be revisited in discussion/summary section.

Agreed, discussion of this has been added to the summary as described above.

18690L2: The IWC ellipse in 3SB is mostly horizontal. Doesnt that contradict your comment?

Apologies, we meant to say 3Sa! This has been corrected.

Figure 6/7. I would suggest removing cloud ice information from this plot. It is not discussed in the text and adds confusion.

Agreed, this did make the figure overly complicated, and obfuscated the issues. Instead we have a separate plot relating O_3 to IWC. See the discussion on these plots above, in section 2.2 of this reply.

3.2 Response to reviewer #2

Review of "Interrelated variations of O_3, CO and deep convection in the tropical/subtropical upper troposphere observed by the Aura Microwave Limb Sounder (MLS) during 2004–2011" by N. J. Livesey et al.

In this paper MLS observations of upper tropospheric O_3, CO and IWC are presented in form of biweekly regional (15° latitude, 30° longitude) averages. The geographical and temporal variability of the data are described in detail with an emphasis on seasonal and interannual variations. The presented data set may provide a valuable tool for the evaluation of Chemical Transport Models (CTMs). In general the paper is well written and of interest for the scientific community. Publication of the manuscript is recommended after addressing the following comments.

Page 18674, Line 7: What about limb scattering (e.g. OSIRIS)?

Apologies for the omission. While OSIRIS or OMPS-LP profiles do not (as yet?) extend into the troposphere, those from SCIAMACHY do. Accordingly we have added a mention of that technique in the manuscript.

Page 18677, Line 7–8: Please explain what "iterative fitting. . . cloud ice product" means.

This is a somewhat complex procedure that is well explained in the MLS document cited in the previous sentence. We have added a brief description of it here.

Page 18680, Line 3–4: "and summer over the North Atlantic. . . " not clear which panel this statement refers to. Please add information.

Added a reference to cells 10Nb and 11Nb.

Page 18680, Line 23: There is no 8Nb in Figure 1.

Corrected the reference to Figure 2.

Page 18680, Line 21: Does this mean that regions with a strong seasonal signal also show strong interannual variability? Or that interannual variability is pronounced during times of largest ozone abundance?

Both are generally true. We had intended to refer to the former, but have taken the opportunity to revise the text to encompass both aspects.

Page 18681, Line 18: Which long term average is used here? The same as for the MLS data? In this case wouldnt it make more sense to compare the individual years where sufficient sonde data is available?

The dates of the long-term averages of the sonde data are given on each panel of Figure 5 (e.g., 1/98–12/11), which we have updated to include more recent sonde data, as available. The default period we chose for the sonde monthly means is from the start of the SHADOZ record in 1998 to the end of 2011. However, the records for some stations start later and some end earlier than these dates, so only 6 of 14 stations have data for January 1998 to December 2011. We decided not to restrict the analysis to the overlap period with MLS (August 2004 to December 2011) as some stations make only bi-weekly measurements, some have gaps, and five stations end before 2011, so the statistics would not be as good as using the longer period. We do not compare individual years, as there are at most 4 sonde profiles per month, which is not adequate to resolve interannual variability in the upper troposphere.

Section 4.1 and 4.4: The relation between ozone and convection is not explored in a clear way and the reader is left somewhat confused about the fact if and how well they are anticorrelated. Section 4.1 states that there is an anticorrelation between ozone and IWC MLS data and refers to some regions displayed in individual panels. However, this argument is hard to follow (and check) since the species are shown in different panels. The correlations in Figure 6 and 7 (indicated by the symbol size versus ozone on y-axis) are not easy to interpret either (however, for most panels there seems to be no real anticorrelation). A clearer presentation of this issue is needed. This could either happen through improved Figures (maybe also show correlation between O_3 and IWC directly) or through additional information on the correlation coefficients (enlarge Figures 6 and 7 and add correlation coefficients between O_3 and IWC where statistically significant) or through presentation of O_3 and IWC seasonal cycles in the same panels.

We agree that the correlation discussion was hard to follow, in part because of the issue with the ACPD layout of the timeseries plots (see section 2.1 above), and also because of the lack of an O_3 / IWC correlation plot. As discussed in section 2.2 of this reply, we have added that plot and updated our discussion accordingly.

Summary (or elsewhere): Has a similar analysis been conducted for other levels above and below? Which ones of the presented features are valid for a deeper altitude range and which ones are only found in the 215 hPa level? Even though it will be impossible to give a complete answer to this question some information will help to connect results presented here to other studies.

In the early stages of the analysis described in this paper, similar plots to those shown in the manuscript were produced for the higher altitude MLS observations, in particular at 146 hPa. We have revisited these in order to answer this question. While quantitative findings differ from those at 215 hPa, as would be expected, the majority of the qualitative findings (wave-one, double peaks, relationships among species, departures from the norms etc.) are broadly similar. The one place where significant qualitative

differences are seen is the analysis in section 5 associated with Figure 8. Accordingly, a discussion of these differences has been added in this section, along with a note to the effect that no other qualitative findings are significantly different.
We note that the analysis tools developed here are being applied to MLS observations of CH_3Cl at 146 hPa, and to the relationship between CH_3Cl and CO at this altitude. These issues will be described in a manuscript on this topic (Santee et al., in preparation).

Figures 1–4: While the presented Figures contain a large amount of detailed information they are hard to read. Red lines in Figures 3–4 are not really necessary since the interannual variability is shown by the standard deviation. The numbers (S and V) need to be larger and more easily readable. Maybe it is better to group different areas instead of different species in one Figure.

Please refer to the discussion of these figures in section 2.1 above. As suggested by the reviewer we have enlarged the size of the S and V annotations in Figures 3 and 4, changed the line color to help distinguish the line from the text, and removed the uppermost horizontal grid line. We have opted to keep the red line as it allows the reader to see month-to-month changes in variability more easily than can be done with an error bar that moves up and down according to changes in the mean value. While Figures 1 and 2 contain a lot of different colored lines, the fact that they can sometimes be hard to distinguish is, to some degree, the point as this indicates cases where there is little interannual variability, so noting the "average" behavior is sufficient.

Figures 6–7: Varying symbol size indicating the IWC does not help to understand the relation between IWC and O_3 or CO and complicates the plot unnecessarily.

Agreed. As discussed above, we have removed this 'dimension' and instead included a separate plot of O_3 vs. IWC comparisons, and updated the discussion accordingly.