Atmospheric
Chemistry
and Physics
Discussions

Interactive comment on "Comparison of ozone profiles and influences from the tertiary ozone maximum in the night-to-day ratio above Switzerland" *by* Lorena Moreira et al.

Anonymous Referee #3

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General comments.

This paper discusses comparisons of observations of mesospheric ozone between two established sensors, one ground-based, the other in low Earth orbit. Particular focus is given to the diurnal cycle seen around 70km in both datasets. The paper seems ideally suited to the special issue for NDACC. However, I'm less convinced that this paper belongs in ACP rather than AMT. Indeed, this manuscript feels like it sits exactly in the grey zone between them. If it were more of a "GROMOS v150 validation" paper it would clearly belong in AMT, but it is too lacking in detail to be that. If the focus was more on trying to understand why the two different sensors (plus the one at Lindau) report different behavior for the mesospheric diurnal cycle in ozone, then it

might be more clearly aimed at ACP (though, as many of those differences may well be instrumental in nature, such a discussion would still retain suitability for AMT). In either case, I feel important detail is lacking.

As it is, the paper presents differences between GROMOS and Aura MLS observations, but makes little attempt to explore their origins, nor even to comment on whether the magnitude of these differences is reasonable, given the differences in approach/performance between the two instruments. I feel greater effort needs to be made to explore these issues further for this paper to be a valuable addition to the field. For example, it's possible the discrepancy relates to differences in latitudinal sampling between MLS and GROMOS (see note on this issue below). A study of the latitudinal variability in the amplitude, based on MLS observations, could be used to quantify the degree to which the latitudinal sampling differences can account for the different amplitudes observed. I suggest the authors strengthen their analysis with some more consideration of such factors and an attempt to quantify (or at least bound) the potential contributors.

In addition, the paper is lacking in detail in several areas (some quite key) as discussed below.

The writing would benefit from some attention by a copy editor, as some of the choices of phraseology are awkward. I've pointed out some, but not all of these, and made suggestions for improvement in some places.

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More specific comments (of varying degrees of import).

(As noted by other reviewers, the line numbers in the manuscript are incorrect in some places. In contrast with the other reviewer, however, I'm going to continue to use them to index lines, for convenience. So in my [the authors] numbering scheme, the first lines on each page vary from 1 to -2.)

—- Page 1

Global note, I believe that it should be "Aura MLS" rather than "Aura/MLS"

line 4: "for the retrieval of" is odd wording: "A new version of the ozone profile retrievals..."

Line 8: Shouldn't it be "GROMOS and Aura MLS profiles agree within 3% on average for ..", or "Average GROMOS and ..." or "On average, GROMOS and ..."?

Lines 12/13: The sentence that spans these lines is poorly worded. "This behavior is related to..." is probably better. Also "On the other hand" is an inappropriate way in which to begin the sentence that follows.

Line 19: "its" -> "their"

Line 22: The assertion that this family of measurements have been indispensable would benefit from some citations that back that point up.

—- Page 2

Line 2: This sentence would also benefit from citations also (e.g., to some of the foundation documents for NDACC, or to GCOS [or similar] reports).

Line 10: "Furthermore" is inappropriate here. It's generally used when introducing a third or greater point, not for a second point. I suggest "In addition, we have ..." or "We have also,..."

Line 11: Badly constructed sentence. As written it sounds like there are two diurnal variations, one unspecified one, and one in mesospheric ozone, the amplitude of which you investigated.

Line 13/14. This explanation could be more complete, specifically, it would be good to give the timescale for the recombination. Presumably its ∼hours not ∼minutes, but needs to be made clear.

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Line 14: "Moreover" feels like the wrong word here. "In addition..." might be better.

Line 18: "an effect occuring at" -> "a phenomenon that occurs at"

Line 22: comma needed between "and" and "since"

Lines 23/24: Badly worded sentence. Suggest: "The lack of odd-hydrogen needed for the catalytic depletion of odd-oxygen, in conjunction with an unchanged rate of odd oxygen production, leads to an increase in odd-oxygen"

Regarding the discussion in this section of the paper, the more conventional way to frame it is to list some relevant reactions and then talk about the processes that give rise to maxima and diurnal cycles etc. in terms of those reactions. So we'd have sentences along the lines of "Lack of sunlight inhibits generation of odd hydrogen via reaction X, leading to enhancement in odd oxygen abundances due to continued production by reaction Y", or something similar. The authors might want to consider taking that approach.

—- Page 3

Section 2.1. This section would benefit from having a few more details concerning the instrument. In particular, no information is given on the bandwidth of the observed spectrum, the spectral resolution, or the receiver noise temperature etc. These are all key parameters needed to get a sense of the measurement system. A plot showing a sample spectrum and associated error bars would be most welcome. For example, there's little point talking about adding 0.5K to the noise here or there without giving the reader a sense of how big the $T_{rec}/sqrt(B\ tau)$ number is. At what altitude does Doppler broadening start to dominate over pressure broadening for this line?

Also, presumably the retrievals need to assume a temperature (and height?) profile. Some information on where that is taken from, and the sensitivity of the result to it would be useful to give.

Line 8: Is the ozone a priori really taken from the ECMWF analysis? How useful is that

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up to 70km, what is it based on. A reference would be good.

Line 13: You tell us that v150 has a constant a priori, but don't say how it behaved in 2021, it would be useful to know.

Line 13: "optimizing" in what sense, what were you trying to optimize? The vertical range, resolution, what? [Or should you change the "and" on the same line to "by"?]

Line 15: This discussion is a little confusing. Earlier parts of the paper give the impression that this study of the diurnal cycle was, at least partly, enabled by the new GROMOS data version. However, here you talk about the new version being focused on improvements in the lower stratosphere. If there were improvements in the mesosphere, it would be best to be more specific about what they are and which of the changes (presumably among those discussed above) brought those improvements about.

Lines 17/18: You need to define all of the terms in these equations, and give us the numbers for T_{rec}, B and tau.

Line 23: "The AVKs are multiplied by 4 in figure 1 in order to..."

Line 24: AVK -> AVKs

—- Page 4

Line 5 (your numbers): "our location" -> "Bern" or "the GROMOS measurement location" or similar.

Line 13: Suggest you make this a "displayed" equation rather than an "inline" one. Also, conventionally vectors are in lower case. If using LaTeX suggest _{\text{GROMOS}} (amsmath.sty) rather than _{GROMOS}, it give more suitable letter spacing (similarly for MLS).

Line 15: Surely Tsou is not the first such reference. Cite others, or at least put "e.g.," in front.

Line 19: More major point here. 8 degrees / 800km is a very large coincidence window, particularly given the ~165km along track spacing for MLS measurements. While you might need this on some days, when GROMOS falls in the gaps between the MLS orbits, on other days you'll get ~5 coincident observations. However, you do not tell us what you do in such circumstances. Do you compare your one GROMOS profile to all five? Do you pick the closest one? Do you average the five profiles together to give one comparison? What are the impacts of your choice on the subsequent analyses? More detail is needed here if readers are to be able to correctly interpret the results that follow.

Line 30: I'm a little bit wary of using the term absolute difference, more particularly in the caption for Figure 2, where you use the term "mean absolute difference". It could be taken to mean the mean of the unsigned difference, |a-b|. Perhaps simply say "mixing ratio difference"?

—- Page 5

Lines 2 and 3 (counting from -2): At face value, the 30-day smoothing and 4-day filtering appear to be contradictory. If the 30 data points are for 30 days worth of observations, then surely such a smoothing is going to filter far more aggressively than 4 days? Are there more than 30 points per day? Is this related to the issue of having more multiple MLS matches to a single GROMOS measurement? If so, this needs to be made much clearer. Plus, the impact of this smoothing is going to vary quite significantly depending on how many points there are on a given day. Why not simply smooth on a daily rather than a point-by-point basis (average of all differences within an n-day window)? Again, all this needs to be much more clearly described.

Line 8: "almost perfect" is very much in the eye of the beholder, and in my eye your scatter plots are far from it. To me "almost perfect" is at the >0.999 level of correlation, where the points are all but indistinguishable from the 1:1 line, with perhaps just one or two strays. I suggest you use more measured language.

Line 9: Odd way to phrase it, simply say that the black line is close to the green one to one line.

Line 21: "variation is also expected"

—- Page 6

Lines -2 to 2: As discussed above, more discussion is needed here. Some more investigation is needed as to why the amplitudes of the cycles are so different. You don't even tell us if we should be surprised by this level of disagreement. Note that the MLS averaging kernels imply not insignificant vertical smoothing at these altitudes for this instrument too. When taken in conjunction with the possible latitudinal gradient, are there plausible reasons to explain the differences based on sampling etc. alone, or is the only feasible explanation some instrumental/calibration difference? If nothing else, raise these questions and identify a route to answering them. Could the diurnal cycle in temperature (and thus the pressure/height relationship) play any role in this (from a measurement characteristics point of view rather than an atmospheric science one)? This manuscript would greatly benefit from an analysis, or at least an identification, of all the potential factors involved.

Lines 13-15: This discussion is unclear, at least to me. If the orange points are smoothed by 10 points, is that 10 days? How does this number related to the ∼7 years between 2009 and 2016. I don't get how the 10-point and 30-point smoothings are related.

—- Figures

In general, all the figures use overly heavy line thicknesses. While it may be OK for the lines themselves (though rather on the heavy side), the linewidth used is far to heavy for the axes. Also the font should be slightly (∼20-50%) larger, and perhaps not bold, for greater clarity.

Figure 2: Suggest "mean absolute difference" -> "mean mixing ratio difference". Also,

how is "its uncertainty" (last line) defined? Do you mean standard deviation?