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Author(s): S. Tukiainen et al.
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1 Authors' response to referees

We thank all three referees for very insightful questions and comments. They have helped to improve the quality of the paper. In the following we answer to the individual questions posed by the referees.

2 Referee 1

2.1 Major comments

1. There is NO mention of tangent height registration. A discussion on this needs to be added: how tangent heights are obtain, their accuracy and precision. This is a large potential source of uncertainty.

For GOMOS, the tangent altitude registration is not really a problem. GOMOS always follows a star, which leads to very small uncertainties (see Tamminen et. al. 2010 and the references in it). We now discuss this matter in the revised text, in Section 2.1.

There is also no error analysis. There can be no validation without a discussion of error, systematic and random. I am not going so far as to insist that an error analysis needs to be included, but I do insist that without it there can only be 'comparisons', and not a validation.

We agree that an error analysis would be needed for validation. However, as the error analysis is a complex and lengthy issue, this could easily be a topic for a separate study. For this reason, this paper does not include a proper validation and error analysis but is concentrated on the inversion method itself. In the text, we now talk about comparison instead of validation.

I found the section on the removal of stray light hard to understand. First off, define what you mean by stray light (I assume you do not mean spectral stray light but rather off-axis light entering due to lack of a baffle?). Provide a physical basis for why this algorithm was used. Surely it can be tested in a model. This appears to be critical to the quality of the profiles and yet there is no justification and details provided. Is the fitted polynomial the amount of signal that is removed? In Figure 2, what is the point at 20 km? This would seem to be critical for the stray light algorithm. I could find no mention of where this comes from.

We rewrote the stray light part (section 2.3) with special emphasis on making it easier to understand.

2.2 Minor comments

Section 2.2 Stray Light Page 4359, Line 4: give an example of some sources

The two main sources of stray light in GOMOS are the scattering from some Envisat hardware and from the limb itself. We now discuss these in Section 2.3.

Table 2: Is the MS:SS ratio not a function of ozone in the UV, especially below 310 nm where your information on upper stratospheric ozone is coming from? I mean a function of ozone at it may depart via natural variability from climatological values (e.g., +/- 20%). Not capturing this would severely compromise the retrieval here. You need to show that this is not the case. This would easy enough to assess with synthetic data

The MS:SS (or actually TOTAL:SS) ratio is also a function of the atmosphere, but the dependence is actually very small. We studied this during the GBL project and it is now mentioned in the text: *"The sensitivity of the look-up table values to the parameters were also studied, and the ranges shown in Table 2 were found sufficient."*

Section 3.1: The GOMOS FWHM is 3 nm and OSIRIS is 1 nm. You might get improved comparisons by reducing the FWHM of the GOMOS slit function slightly to account for the finite resolution of the OSIRIS slit function.

This has only a negligible effect in practice.

Section 3.2: I noticed you have an OSIRIS person listed as an author. The OSIRIS absolute calibration was updated in the past year or so and I assume you are using this.

Yes we use the latest OSIRIS Level 1 data (with the updated absolute calibration). It was already stated in Section 3 (line 4).

Section 3.3: Why is the stray-light corrected ratio noisier?

This is because there is some noise in the spectra above 100km. If we use this spectra to extrapolate stray light for lower altitudes, the noise also penetrates and is visible after subtracting the stray light from the radiances. We now mention this in section 3.3.

English and grammar: A large number of grammatical errors need to be corrected. A few are here:

We have revised the text, correcting all errors found.

3 Referee 2

3.1 Major Comments

1- I think the main strength of the paper is the association of this team with ESA, and the potential implementation of this retrieval to process all GOMOS bright limb measurements and subsequent release of this data set to the scientific community. If this is the case, the authors should highlight it and inform the reader if there is there is such a plan to process all the bright limb measurements by ESA.

It is likely that the daytime GOMOS data will eventually be processed using the GBL method. However, the inclusion of GBL in the official ESA processor takes time because decisions will have to go through many levels of administration. An option would be to process the daytime data at FMI to make the GBL data set available sooner.

2-The paper could benefit from an error characterization of the retrieval so that the reader can understand the potential accuracy and precision of the measurement.

We agree that an error analysis would be needed for validation. However, as the error analysis is a complex and lengthy issue, this could easily be a topic for a separate study. For this reason, this paper does not include a proper validation and error analysis but is concentrated on the inversion method itself. In the text, we now talk about comparison instead of validation.

3- Section 5, I think the authors should rename it data comparison rather than validation, because of the limited number of profiles and instruments used.

Agreed, we did so.

4- As stated in section 2, GOMOS measures the bright limb above and below the star. Which one did you use? Did you perform separate retrievals or only retrievals using either the upper or lower band? How do the separate retrievals compare? Please discuss further the merits of the approach you used in this study.

We used the upper band. The difference between the upper and the lower band results were statistically very small below about 50 km. However, there can be large relative differences in individual profiles specially at high altitudes.

During the project, there was a study about consistency of the two bands. The results indicated that there are some quality issues to be aware of, and there were different issues in different bands. Therefore, mixing of the two bands is not recommended.

3.2 Minor Comments

1-Page 4356 line 6: 'We introduce an alternative technique' should be 'We introduce a retrieval that supplement GOMOS night-time measurements' or something similar.

OK, we changed this.

2-Page 4356 line 24: 'the limb-viewing technique can not achieve as good global coverage, but it yields superior vertical resolution', this is not exactly accurate, since the main advantage of the limb-viewing technique is that it can achieve similar good global coverage as well as superior vertical resolution.

You are right. It is better to state that "...the limb-viewing technique can not achieve as good horizontal spatial resolution, but it yields superior vertical resolution. "

3-Page 4357 the paragraph starting at line 4, can you establish relevance of this paragraph to the current study?

The first sentence is irrelevant and we removed it.

4- Page 4357 line 17; can you specify the number of bright limb measurements?

Yes we can, but we think that the exact number is not relevant, and a rough estimate is good enough (more GBL measurements come every day).

5-Page 4357 line 23; replace 'in similar way than' to 'in similar way to'

Corrected.

6- Page 4357 line 28: 'we present a new method to retrieve ozone' the retrieval is not a new method, rather than a new study. The technique is already used for OSIRS measurements in Tukiainen et al, 2008..

That is correct. We changed the text: "In this paper we present a study to retrieve..".

7- Page 4378 line 18; the sentence the flags are claiming the data to be good is confusing, since the authors said earlier that the flag are not correctly implemented, can you please clarify the use of the flags in this data set?

It should be now more clear. *"The upper panel is plotted using the current Level 1 data, and while the signal notably begins to saturate at 26 km, the version 5.00 flags are wrongly claiming the data to be good up to 24 km."*

Furthermore, It was already stated in the end of next paragraph *"In this study we ignored the whole 400–500 nm region to be sure that no saturated pixels were used in the retrieval."*

8- Page 4358 line 22 replace the whole UV optical region with the whole UV-Visible optical region

Corrected.

9- Section 2.2 stray light: the authors need to elaborate more on the source of stray light.

We now explain the two major sources of external stray light in GOMOS, in Section 2.3.

Page 4359 Line 6; the authors need to reference Rault, 2005 and Taha et al, 2008 for introducing similar empirical corrections model of the stray light. The scheme for straylight in this paper is rather similar to both papers, although not identical.

We now mention these two studies: *"In Taha et al (2008), the GOMOS stray light was modeled by assuming the measured radiances to be entirely stray light between 80 and 120 km and by doing a simple linear fit for each wavelength. A similar approach was also used by Rault (2005) for SAGE II."*

Figure 2: The figure would benefit from adding longer wavelength, the 500 and 600 nm which are used for the ozone retrieval. Also by showing the radiance before and after the corrections applied using log scale if needed.

We added 350 nm and 600 nm wavelengths.

The authors need to explain in the text the use of 20 km constraint value?

This corresponds to the lower limit of the GOMOS daytime scans. It is now explained in the text.

10-Page 4360 Line 9: OSIRIS radiances are relatively clean of stray light below 70 km is not exactly accurate, since OSIRS still exhibit straylight contamination depending on altitude and wavelength. Please add more information about the OSIRIS straylight using proper references.

That is true. We mention it now: *However, it should be noted that the stray light is not totally absent from the OSIRIS measurements either. But according to Llewellyn et. al and references in it, the contamination is rather small and within expected limits.*

11- Page 4380 Line 15: These observations are from a narrow narrow latitude band, use very narrow instead.

Corrected.

12- Page 4361 line 7: First, we analyze differences in the GOMOS and OSIRIS absolute radiances replace it with the differences between GOMOS and .....

Corrected.

13-Page 4361 line 20: Can you elaborate more about any possible reasons for the observed increase in the bias?

The reason for the bias is not clear at the moment.

14- Section 3.3: the authors use fixed altitude normalization at 50 then 47, however, the conclusion of which normalization height to use is not clear. I think they end up using the 47 km for normalization, but you can only read it in Fig 8 not in the text. Did you investigate the use of different normalization height for the UV than the VIS?

Yes this was a little confusing. Now we always use the 47km normalization height and show radiance comparisons for 55km and 30km. We have not tried different normalization heights for the UV and the VIS. This is something we could try in the future.

15- Page 4362, line 13: Did you use the reported error values of the GOMOS bright limb? Can you add more about accuracy of the error estimate?

We use the square root of the signal (plus the dark charge and an error of the stray light proportion) as a standard deviation estimate for the radiance. This is now explained in the inversion method section.

16- Page 4362, line 16: The reference altitude used in the retrieval is at 50 km, why not 47 km? It clearly shows better agreement with OSIRIS than 50 km. Again, did you try using different reference heights for UV and VIS? What is the advantage of using same reference altitude for all wavelengths?

We now use the first measured tangent height below 47km. I does not make significant difference in the profiles. That is good because the inversion method should *not* be very sensitive to the normalization height of choice.

17- Page 4363 line 7: while NO2 is taken from a climatology and kept fixed, is it one fixed profile or a location and time dependent profile? Depending on which climatology you use there might be an added error that needed to be discussed in section 6.

We use the same NO2 climatology that was used to build the TOT/SS look-up-table. It is dependent on the latitude (5 regions) but there is no time dependence.

18- Page 4363 line 5: Typically O3, aerosols and neutral air are inverted together, did you really invert aerosol and neutral air? If so you need to discuss the accuracy of the retrieval and the error associated with such retrieval on the ozone profile. How does you retrieval compare to using a climatology of aerosol and air density?

Yes we do get the aerosol and neutral air profiles. The quality of these products is a little open issue at the moment. The aerosol profile is very sensitive to the saturation, for example. Neutral air is usually very close to ECMWF values, though. We say in the text: *" In this study, we did retrieve neutral air and aerosols in the same peeling loop as ozone but the quality of these two products will be determined in later studies."*

19- Section 5 page 4364: Which version of OSIRIS ozone profile did you use? Can you add a comment about the quality of OSIRIS ozone?

We use the FMI own OSIRIS product (L2 version 3 with the latest L1 data). The quality of this data is very good at these (mid) latitudes between 20-50km. The difference between GOMOS and OSIRIS is only a few percents. Similar result with SAGE II. However, in this study we only compared the 14 profiles (with the same geolocation and solar angles).

We hesitate to comment the quality of the OSIRIS ozone profiles, because a good validation reference is missing. Therefore, we added the profile comparison against GOMOS which is a much better validated instrument.

Fig 9, can you add the 50% percentile to the right panel, similar to Fig 10. Also comment further on the 50% percentile in the text.

Fig.9: We added the 25th and 75th percentiles to the both medians.

20- Can you expand your comparison for the GOMOS bright vs. night measurements? Why only showing 30S-30N? Looking at Fig 11, there are far more coincidence between the two measurements outside the selected latitudinal band. I think the paper could benefit from expanding the comparison in this section and further discussion of the sources of random and systematic biases seen in comparison.

We are still working on the error analysis and validation, and plan to write another paper when these studies are ready. In this paper, which can be taken as a proof of concept, we consider the GBL method itself and opted to present only a preliminary comparison of the results with GOMOS (occultation) and OSIRIS data.

4 Referee 3

4.1 General comments

The paper presents interesting results on the ozone profile retrieval from measurements of scattered sunlight from the GOMOS instrument. Although not stated explicitely this seems to be the first publication on this topic. With no doubt the obtained results complement the stellar occultation dataset from GOMOS and are important for atmospheric studies. The presented comparison results are promising and I would like to encourage authors to perform an extensive validation in a follow-up study. Unfortunately the paper is written not carefully enough and some important contents are missing. The retrieval algorithm is insufficiently discussed and the description of the stray light correction method is unclear. It is also unclear if the results of very limited comparison with OSIRIS are also valid for other atmospheric or observation conditions. The abstract is wrongly focused and provide improper conclusion on the retrieval accuracy. No concise formulation of the conclusions is given. Nevertheless, my impression is that after a moderate revision according to my specific comments below the paper might be considered for publishing in AMT.

We rewrote the stray light part (section 2.3) with special emphasis on making it easier to understand. The inversion method is also described more extensively.

4.2 Specific comments

1. Abstract: The first two sentences of the abstract are not in line with the focus of the manuscript as they give an impression that GOMOS uses only the stellar occultation technique.

We now mention that GOMOS also records limb scattered spectra during the daytime.

2. Abstract, line 14: better than 10% at 2250 km - This estimation is true only for the comparison between GOMOS night OCC and GOMOS GBL. For the comparison between GOMOS and OSIRIS a statement better than 20% would be more appropriate. The abstract must not present only the best case conclusions.

We say in the abstract that the profiles were within 10% with GOMOS and within 15% with GOMOS. The bias is larger only if we use the UV region at low tangent heights.

3. Introduction, lines 22-24: Limb-viewing instruments can directly observe solar or stellar signal as it is occulted by the atmosphere, but limb scattered indirect sunlight, or radiance, can be used as well. - From this sentence one gets an impression that the scattered light measurement technique is something really new in comparison to well-established occultation geometry. This would have been true if it had been stated shortly after the launch of Odin and ENVISAT satellites 8-9 years ago. At present, the limb scattered light observation technique is widely approved and should not be introduced as a minor addition to occultation mode.

You are correct. We changed the text: "Limb-viewing instruments can directly observe solar or stellar signal as it is occulted by the atmosphere, or instead measure the scattered indirect sunlight (radiance) and various atmospheric emissions."

4. Introduction, page 4356, lines 24-25: Compared to the nadir looking instruments, the limb-viewing technique can not achieve as good global coverage, but it yields superior vertical resolution. - In principle, limb viewing technique can achieve the same global coverage as the nadir looking instruments. Most probably you mean horizontal resolution here.

Yes you are correct, we mean the horizontal spatial resolution. This was corrected in the text.

5. Introduction, page 4356, lines 25-26: A major advantage of the stellar occultation technique is the possibility of night-time observations which is essential for, e.g., polar night studies. - The topic of your paper is actually limb observation mode. What is a goal of discussing advantages of the stellar occultation technique?

Maybe it is a little irrelevant to discuss stellar occultation here. We took away this sentence and now focus more on the limb technique: *"The limb view also offers greater sensitivity to trace constituents than the nadir view. Cloud interference is often a disadvantage in the limb view, but on the other hand, it can be used to study e.g. polar mesospheric clouds."*

6. Introduction, page 4357, lines 2-3: I think this sentence would benefit from more recent references.

Shepherd (2002) was added.

7. Introduction, page 4356, lines 25-26: The key element of the middle atmosphere is ozone, but there are several other observable species such as NOx and HOx compounds, BrO, OClO, aerosols and even metals. It is typically the wavelength band of the instrument that restricts which species are possible to detect. - This two sentences have a relation neither to previous nor to subsequent text.

Agreed (we removed these sentences).

8. Introduction, page 4357, lines 11: I think a proper illustration of recent studies on ozone requires more references.

We added a few recent references.

9. Introduction, page 4357: The GOMOS (Global Ozone Monitoring by Occultation of Stars) instrument on board the Envisat satellite uses stellar occultation method to probe the atmosphere between 10 and 120 km.: - Alone from the title of your manuscript one suggests that GOMOS can also measure the scattered sun-light. This sentence seems to claim that it is not the case.

GOMOS is mainly a stellar occultation instrument. We mention this in the text now. The limb product was initially more like a side product to be used in the day occultation retrieval. GOMOS wasn't optimized for limb scatter retrievals which shows in weak radiance quality (compared e.g. to OSIRIS).

10. Sect. 2: A subsection containing a short general description of GOMOS measurements needs to be provided. It should contain such information as how the scanning is performed, vertical and horizontal resolution, vertical sampling, spectral resolution and sampling and so on.

There is now a short discussion about his.

11. Sect. 2.1: Please explain what exactly is referred as the CCD saturation. From the electronic point of view one would expect that the saturation means that the CCD reaches the maximum of its dynamic range. It this case however one might want to avoid the parts of the spectrum where the signal is high. As it follows from Fig 1, you reject rather the spectral parts where the signal is lower. Is there just a part of CCD which has an anomalously reduced dynamic range?

This *is* the part of the spectrum with the highest signal values. Fig 1 shows radiance ratio, not absolute radiances. When the signal saturates, it appears as a hollow in the ratio (the ratio converges to constant value, even if you go lower altitudes with increasing signal).

We could remove the whole saturated region of 400-500 nm from the retrieval and this is exactly what we did for this paper (Section 5). But this is unnecessary drastic operation because the spectrum is perfectly good above 30 km. We really should be able to trust

the Level 1 flags telling me which pixel is good and which is bad. Thankfully this is fixed with the new Level 1 data version.

12. Sect. 2.2, Eq. 1: Index -1 is ambiguous. It is better to write a fraction instead

Corrected.

13. Sect. 2.2, line 10: where $I(\lambda, j)$ are measured radiances and $\bar{I}(\lambda, j)$ are the radiances at 500 nm. - the formulation gives an impression that $\bar{I}(\lambda, j)$ are not the measured radiances. What are these? If this is a measured radiance you are weighting different tangent heights with a single point when averaging intensities. This puts a high weight to the noise at 500 nm. Is the averaging with noise-determined weights meaningful?

They are the measured radiances, it is now said in the text.

14. Sect. 2.2, line 11: altitude index $\rightarrow$ tangent height index

Corrected.

15. Sect. 2.2, line 11: goes through the measurements above 100 km - how many measurements and at which tangent heights are these?

It depends on the scan. Typically, a GOMOS measurement begins at around 130 km with around 1 km vertical sampling. It is now said in the text.

16. Sect. 2.2, lines 13 - 16: You write you fit the radiance with a polynomial at each wavelength. In this case you can easily extrapolate the radiance to the low altitudes but it is unclear how you are doing this with the stray light. Do you use the polynomials from the radiance fit to re-calculate the stray light? If yes you have to associate your stray light to any tangent hight. Which one is that? If it is not the case please explain how you do the extrapolation.

We rewrote the stray light part (section 2.3) with special emphasis on making it easier to understand.

17. Sect. 2.2, lines 13 - 16: Suppose you have somehow extrapolated your stray light to the low altitudes. How do you proceed to get back to the radiance from the dimensionless ratio, i.e., to obtain the red circle in Fig. 2?

We rewrote the stray light part (section 2.3) with special emphasis on making it easier to understand.

18. Sect. 2.2, lines 13 - 16: Why is not the stray light as given by Eq. (1) calculated starting from the same tangent height $\hat{z}$ as used later for the fit? What is a goal to fix the lowest altitude for the stray light at 100 km and use a dynamic value for the fit range?

Yes it is confusing that there is a different range for the second polynomial fit. It is not really needed and we changed that. It's now the same 100 km. We just wanted to get as many altitudes as possible for the second fit but there is a risk. If you take too low tangent heights you might be fitting also the true signal and not just stray light.

19. Sect. 2.2, lines 24 - 25: In reality, the amount of stray light may actually vary as a function of altitude leading to abnormal results from the extrapolation. - How are these abnormal results identified and how are they treated then?

At the moment we only look at the chi square values after the retrieval to identify problem cases. Majority (90% or so) of the retrievals seem to produce reasonable chi square values indicating that the stray light removal was also successful. Profiles retrieved with too large chi square values we can simply discard. We plan to look at these cases more carefully in the future to understand the reason behind the failure.

20. Sect. 2.2, page 4359 line 25 and page 4360 line 1: Stray light is dependent on the albedo below the satellite - I think you mean below the tangent point. The satellite is quite far away from the probed area and, thus, I do not think that albedo below the satellite makes any effect.

It is actually the zone between the tangent point and the satellite position. Clouds on this area affect the amount of stray light specially in the red end of the spectrum. We have investigated this issue but the results have not yet been published.

Now it says: *"As mentioned earlier, GOMOS stray light is dependent on the albedo of the zone between the tangent point and the satellite position. Thus, strong albedo gradients during the measurement can disturb the usually quite linear altitude dependence of stray light at high tangent heights."*

21. Sect. 2.2, Fig. 2: Please discuss physical/instrumental reasons why the stray light should decrease below 50 km.

In the stray light removal method we try to deduce the altitude dependence of the stray light by investigating >100 km values. Typically, the altitude dependence varies depending on the wavelength from very linear to slightly curved shapes. This would suggest that also the spectral shape of stray light is different at low tangent heights than at high altitudes (so the spectral shape constrain is not totally accurate). Not much is known about the stray light and the possible physical reason for stray light decrease below 50 km is not clear to us.

22. Sect. 3: OSIRIS is known to measure typically at large solar zenith angles. For this conditions one can expect the highest contamination due to the direct sunlight penetrating into the instrument but the lowest contamination due to the scattered light. As you highlight the influence of the surface albedo I guess the latter contribution is at least comparable to the first one. It is extremely important to investigate which source dominates. This is the only way to draw a conclusion if the considered coincidences represent best or worst case with respect to the stray light contamination of GOMOS measurements.

We agree that this is an important point. Unfortunately, not much is known about GOMOS stray light at this point. This kind of analysis should be included in the continuation paper with a proper error characterization and validation.

23. Sect. 3.1: This is because the slit of GOMOS is much larger than of OSIRIS. - The fact that a poor spectral resolution of a spectrometer is usually determined by a large entrance slit is broadly known. Thus, the sentence is unnecessary.

The sentence is now removed.

24. Sect. 3.1: The best estimate of the slit function is shown in Fig. 3. - How has this estimation been obtained?

By studying the 557 nm oxygen line. It is now mentioned: *"The GOMOS slit function was estimated from the GOMOS data by investigating the oxygen emission line at 557 nm which spectral characteristics are well known. "*

23. Sect. 3.1: weak spectral resolution → low spectral resolution

Corrected

25. Sect. 3.1: The following statements are in a contradiction: ... whose absorption fingerprint is small. and ... using the strong NO2 absorption features in the 430450 nm region.

It is now corrected: *"This makes it challenging to retrieve trace gases whose absorption fingerprints are relatively small compared to the ozone fingerprint. For example, NO_2 is routinely retrieved from the OSIRIS data using the NO_2 absorption features in the 430–450 nm region."*

4.2.1 26. Sect. 3.1: What is a reason for discussing NO2 in a paper dealing with ozone retrieval?

We are also working on NO2 retrieval but at this point the results are not satisfactory in quality. Therefore, we do show them in the paper. Nevertheless, we believe it is still

relevant to keep the short discussion about NO2 here. It would have been natural to also retrieve NO2 from GBL measurements.

27. Sect. 3.2, line 10: vertical sampling resolution - vertical sampling and vertical resolution are different characteristics. Which one do you mean here?

Yes it's just vertical sampling. Resolution is a different matter and not relevant here.

28. Sect. 3.2, Fig. 5: Further in the course of the manuscript you are focusing on the 30 km tangent height. It would be interesting to see the plot similar to Fig. 5 for this tangent height as well.

This kind of picture is not very interesting at 30km, because the relative amount of stray light is so much smaller. At 55km the difference between uncorrected and corrected spectra is dramatic.

29. Sect. 3.2 - 3.3, Figs. 5 - 8: I am wondering why the upper tangent height in the plots and/or ratios is always different. First you plot 50 km then go for 47 km when averaging and finally plot 55 km. I think it would be more consistent if you use always the same tangent height.

OK, now we always use 47km as a normalization altitude, and show radiance comparisons for 55km and 30km.

30. Sect. 4 lines 12 - 13 : For every measurement layer j ... - You do not measure any layers. May be you mean tangent height?

Corrected.

31. Sect. 4, Eq. (2): Does $\bar{I}_{ref}$ in the denominator include multiple scattering?

Yes it does (now said in the text too).

32. Sect. 4, Eq. (2): It is unclear how to make a least squares fit as there is no fitting parameter in the equation.

Fitting parameter is ρ (density vector of O3, air and aerosols for the current tangent height). Is it really unclear? Maybe the chi square formula clarifies this.

33. Sect. 4: Table 2 should be referenced already here. The solar zenith angle grid is suboptimal. The step size is unnecessary small for small solar zenith angles and too coarse close to 90 deg.

You are maybe right. This is the first version of the LUT and it can be corrected in the future.

34. Sect. 4: Provide the formula for the trace gas density fitting procedure

We added the formula for the chi square (which is minimized in the fit.)

35. Sect. 4: ... using tens (or hundreds) of wavelengths in the 280680 nm band - From this sentence reader might get an impression you do not know how many wavelengths you have in your fit. Please be more specific.

We have tried different wavelength sets but it is not straight forward to determine the optimal combination of wavelengths. Nevertheless, we state that we used 71 wavelengths.

36. Sect. 4: ... in the 280680 nm band - As you have shown in Sect. 3 the spectral signal below 350 nm is strongly contaminated by the stray light and the correction algorithm fails to provide reasonable results. What is the reason to include these wavelengths in the retrieval?

These wavelengths are necessary if we want to retrieve profiles above 40km. Otherwise we could use only the Chappuis band.

37. Page 4363: Section Radiative transfer model has no number.

This is AMT's error.

38. Page 4363, last line and Page 4364, first line: More multiple scattering means slower execution. Smaller contributions come from the solar angles, atmospheric composition and albedo. - The statements are misleading because the multiple scattering contribution is determined by the solar angle, atmospheric composition and albedo.

It is fixed now: *"More multiple scattering means slower execution. The multiple scattering contribution is determined by the solar angles, atmospheric composition and albedo."*

39. Sect. 5: Below 22 km the standard deviation of the GBL profiles increase significantly. - The standard deviation of GBL profiles is not shown.

It is better to comment on the large bias than the deviation: *"There is a large bias in the GBL profiles below 22 km compared with the night occultation profiles. This is probably because some saturated pixels were still used in the retrieval (outside of the 400–500 nm band).*

40. Sect. 5: It is difficult to follow occultating stars at low tangent altitudes, and especially when clouds enter the line of sight. - It should actually affect both GBL and night OCC measurements in a similar manner. Thus, it can not be an explanation for the differences in Fig. 10. Or may be you want to say something else with this sentence. Please clarify.

Yes, the cloud interference is not really the problem during daytime. It is the limb signal itself that easily confuses the star tracker. We now try to explain it: "During the daytime, when the limb scattering contribution is present, it is difficult to follow occultating stars at low tangent heights. The limb signal starts to overwhelm the star signal and the star tracker more easily loses control."

41. Sect. 6: The spectral resolution of GOMOS (bright limb signal) is poor, about 3 nm compared to the 1 nm resolution of OSIRIS. - this is actually not an issue for ozone retrieval which is the topic of the paper.

Right, but we would still like to include the note about the resolution, because it's relevant for (possible) NO2 studies in the future.

42. Sect. 6: Unfortunately, close matches between OSIRIS and GOMOS measurement times, tangent point locations, and solar angles are rare and only 13 cases were investigated in this paper. - This might be an indication that OSIRIS was not the best choice for your comparisons.

For radiance comparisons it should be the best reference. We only can think Sciamachy as another option. For profile comparisons, we could use much looser coincidence criterias, but we wanted to show also the profiles of the 14 cases we had used in the radiance comparison. We think it is natural also show how these profiles compare.

43. Sect. 6: The word validation is not applicable to the presented set of comparisons because it is quite sparse and limited to certain latitudes. The word verification would be much more appropriate.

This is true. We use "comparison" now.

44. Sect. 6: Finally, it should be noted that the validation for profiles shown in this paper are preliminary .... - Noun and verb are inconsistent : ... validation ... are ...

Corrected.

45. Sect. 6: ... and should be repeated using other reference instruments for all latitudes. - repeat means do exactly the same. I think you want to extend rather than repeat the comparisons.

Corrected.