Response to Interactive comments from Referee #2 on: "AerGOM, an improved algorithm for stratospheric aerosol retrieval from GOMOS observations. Part 2: Intercomparisons" by C. E. Robert

We would like to thank referee #2 for the useful comments made regarding the manuscript.

Text legend:

Referee comment
Author reply
Author addition to the original manuscript

1. General comment

1.1: My primary criticism is that the results of the data comparisons are presented for the most part as a simple catalog of observed differences; and often no coherent explanation is provided for the observed systematic differences between the AerGOM retrievals and the other measurements. Also, it is hard not to conclude from the results presented that the operational IPF data generally agree as well and often better with the correlative measurements than the AerGOM results. This issue should be addressed directly in the conclusions.

Both of these aspects have been addressed in this version of the manuscript. Section 5 now includes more references to accuracy and precision estimates from literature for various datasets, and provides tentative explanations for the differences seen between AerGOM and other datasets. Differences between AerGOM and IPF results are also discussed at much greater length in a section dedicated to the subject (section 5.2).

2. Specific comments

2.1 Page 2, lines 4-5 – there must be a standard reference to the current GOMOS operational processing algorithm (IPF v6.01). It should be used here.

I am not sure that there is a standard peer-reviewed publication that focuses solely on IPF6.01. The closest would be Kyrölä et al. (2010) but I think that the latest GOMOS ATBD would be a better reference, as this is really all about IPF6.01. I therefore added a reference to this technical report in the paper on Page 2, lines 4-5. Here is the new addition to the "References" section:
Kyrölä, E., Blanot, L., Tamminen, J., Sofieva, V., Bertaux, J. L., Hauchecorne, A., Dalaudier, F., Fussen, D., Vanhellemont, F., Fanton d'Andon, O., and Barrot, G.: GOMOS Algorithm Theoretical Basis Document, Tech. Rep. GOM-FMI-TN-040, 2012.

2.2 Equation (1) – I believe the symbol σref should be βref for consistency.

I agree. This has been changed in the manuscript.

2.3 Sect 2.3 –If the cause of the anomalous retrievals is due to low signal-to-noise for dim and/or cold stars, why do these same events not fail for the IPF retrieval? Nothing in the discussion indicates that the AerGOM method lowers the signal-to-noise compared to the operational retrieval (since presumably the same Level 1 transmission data is used as input to both algorithms this could only result from changes in binning or smoothing). If the lower signal stars cause more problems with the AerGOM algorithm compared to IPF it is presumably less stable in some way that needs to be explained.

One aspect of AerGOM that is interesting is that it retrieves all species simultaneously, but this is also a drawback here. In the GOMOS IPF, the retrieval of NO2 and NO3 is done using a DOAS method (as noted in the text page 3, line 21) improving the retrieval of these species in cases where the SNR is low. Since we do not use such an approach, sometimes the values retrieved for the NO2 and NO3 (and ozone and aerosols as well) can be non-physical and much larger (or lower) than one would expect (as seen in Figure 1B). But we recently found a way to improve the retrievals and get rid of the anomalous profiles by using the climatology as a starting point for the retrieval at all altitudes (and not just the top altitudes), a very simple modification that solves this problem and improves the stability of the AerGOM retrieval. It should be noted however that despite this improvement, these profiles have very large errors and especially at shorter, do not have a large information content at low altitude.

We modified section 2.3 to explain a bit more why there were anomalous profiles for AerGOM and not IPF, and how we corrected the problem:
The reason for the retrieval of such profiles by AerGOM was due to a combination of low signal-to-noise ratio (SNR) of the transmittance at shorter wavelengths for dim and cold stars, and an inadequate a priori of gaseous and aerosol species. The operational retrieval sidestepped this issue by using first a DOAS method to retrieve NO2 and NO3, removing their contribution from the measured signal before carrying out ozone and aerosol retrieval.

This problem has now been fixed by using full climatologies of gas and aerosol species as a priori for the spectral inversion. However, this finding prompted the consideration that some of the retrievals might be affected by occultation parameters such as star properties and solar zenith angle (SZA) that could lead to straylight, and occultation obliquity which is an important factor in the imperfect correction of atmospheric scintillation (Sofieva et al., 2009). Therefore, another aspect of this inter-comparison involves studying the consistency of the agreement of AerGOM aerosol retrievals with those of other instruments under various occultation conditions. Section 6 presents the results of these comparisons.

We also modified section 2.2 to mirror this new retrieval approach:
Climatological profiles of various species are provided as a starting point for the non-linear Levenberg- Marquardt spectral inversion, leading to slant path integrated column densities and aerosol optical thicknesses at each tangent height.

2.4 Figure 2 – In this and subsequent figures the following labels are used – “Sage 2/3” and “POAM 3”. These should really use the Roman numerals (e.g., SAGE II) for consistency with the text.

This has been done. Note however that POAM III comparisons in Section 6 have been omitted, due to the large variability of the relative difference as seen in Figure 3 and also because POAM III data did not add much to the discussion.

2.5 Page 6, Line 20, last sentence beginning “It is also important...”. I am not sure what this comment means or why it’s relevant. Since only spatially coincident measurements are used for the comparisons (as they should be) it is not clear how the high-latitude sampling of SAGE III and POAM III should introduce any spatial bias to the comparisons. The authors should clarify this comment or remove it.

I agree that this sentence is confusing and not necessary. It has been removed.

2.6 Figure 3 – The lighter curves in the right column (Absolute Extinction) corresponding to the other instruments are almost impossible to see. Either get rid of all the grid lines on the plots or change these curves to make them more visible (e.g., same thickness as AerGOM only different line type or symbols)

Agreed. These curves now have a * symbol on top to help with the visibility, so I think that it should be much clearer to the reader. I didn’t think a good idea to use dashed lines because it might have been confusing, as we already used dashed lines in the other panels to show IPF results.

2.7 Page 11, line 20 – The last statement that “...the GOMOS data used is exactly the same.” is not strictly true according to the description in section 2.2, which says that data from spectrometer B1 has been included in AerGOM. Presumably these channels are not used in the IPF algorithm. Have you quantified the effect of adding this data into the aerosol fitting algorithm? These wavelengths (750-776 nm) correspond to the oxygen A-band, which is a strong absorption feature and thus dominates the background aerosol extinction. Have you quantified the effect of adding information from this channel, and can you show that it does not it cause any bias?

That is a good point. I removed that last sentence but added an entire subsection (5.2) dedicated to the discussion of IPF and AerGOM differences, as I think that this is an important point of the paper, which was also noted by the other referee.

Concerning spectrometer B1, it is indeed the case that IPF does not use data from this spectrometer. We actually avoid using pixels within the oxygen A-band to avoid the strong absorption features, so no bias should be introduced. I clarified this point by mentioning in section 2.2 the exact wavelength ranges that we use in spectrometer B1:
*The most important improvements implemented are: 1) the extension of the spectral range used for the retrieval using information from spectrometer B1 (**755-759, 770–775 nm**);*

2.8 Section 6.2 – I find the discussion of SZA dependence confusing or incomplete. All the coincident measurements being compared to AerGOM in this study are made in sunlight. Four of the 5 instruments use solar occultation and thus measure at 90 degrees SZA by definition. The fifth – OSIRIS – uses scattered sunlight on the dayside of the orbit. So the use of GOMOS nightside occultations for the coincidences introduces a time offset, in both absolute UT and local time. This time difference is smallest for the lowest-SZA GOMOS measurements. The authors rightly point out that there is no known diurnal variation in aerosol extinction and attribute any observed dependence on SZA to a scattered light artifact in GOMOS. The GOMOS team should be able to characterize that artifact and know how it affects the aerosol retrieval. Does it make sense that this artifact impacts the wavelength-dependent difference profiles in the way observed?

It is true that the discussion concerning the SZA was rather short. This section has been improved and extended to address the points you raise.

To summarize the additions to the section and answer your questions: it is the case that we assume that the dependence on the SZA is due solely to straylight. Actually, straylight should already be corrected in Level 1 GOMOS files, but there still seem to be some contamination in the data. The presence of straylight would increase the number of photons detected by the spectrometer, hence increasing the value of the transmission and decreasing the extinction. If this decrease in extinction is attributed to the aerosol (which has a slow varying spectral dependence unlike ozone, NO2 and NO3), then the comparison should show a decrease of the positive bias. If we assume the straylight to be more or less constant with altitude, its relative effect should be larger at high altitudes (> 25 km) and for longer wavelengths due to the generally much smaller aerosol extinction values typically found in such cases.

2.9 Section 6.3 – It is not obvious to me that anything useful is added by including this section. There does not seem to be any definitive conclusion as to the effect (if any) of the obliquity of the GOMOS occultations on the data comparisons. This section is really just a long list of observations from the various plots that don't tie together in a coherent way. I would therefore suggest the authors consider removing this section.

I only realized later that the methodology used in this section was flawed. Basically, one does not expect the obliquity to change the results of ensemble means, as was shown in Figure 7. The larger obliquity should introduce errors due to the inability to correct anisotropic scintillations, but these effects are random and observed at the level of one profile, not by averaging several profiles together (which would then just average the variability to nothing). Hence, this section should have shown instead a measure of the variability of the relative difference. But after looking at such results, there was even less to conclude from, and therefore I decided to follow the referee's recommendation and simply skip this section. We added some text in section 6 explaining this:
A study of the effect of the obliquity on the bias was carried out, but the results did not bear any concluding evidence of a repercussion on the AerGOM measurements and were therefore omitted from the discussion.

3. TECHNICAL CORRECTIONS

3.1 Page 10, Line 26 – I think instead of ‘throughs’ you mean ‘troughs’.

Yes, this has been corrected.

3.2 Page 14, line 16 – Correct ‘cases case.’

Done.

3.3 Before resubmission the document should be scrubbed for editorial corrections. There are other grammatical and spelling errors that I did not bother to point out.

We did our best to correct the paper, but without any specific comments, it is impossible to ensure that the paper is now error-free.