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AerGOM, an improved algorithm for stratospheric aerosol retrieval from GOMOS observations. Part 2: Intercomparisons

by Robert et al.

General Comments

This paper describes results of a new algorithm – AerGOM – used to retrieve aerosol extinction profiles from the GOMOS stellar occultation data. AerGOM extinction retrievals are compared to both the operational GOMOS retrievals from the IPF algorithm and to coincident measurements of extinction at multiple wavelengths obtained from five independent satellite sensors. The paper attempts to characterize the dependence of the observed differences on key occultation parameters that define the GOMOS data quality. This study is useful in that it explores potential sources of random and systematic errors in the GOMOS AerGOM retrievals, which will be of interest to scientists interested in using that data product. The paper is suitable for publication in AMT and I recommend it be accepted for publication after the authors address the issues raised below.

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.

Specific Comments

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.

Equation (1) – I believe the symbol σ_{ref} should be β_{ref} for consistency.

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.

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.

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.

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)

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?

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?

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.

Technical corrections:

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

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

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.