Atmos. Meas. Tech. Discuss.,
doi:10.5194/amt-2017-299-RC2, 2017

Atmospheric Measurement Techniques Discussions

Interactive comment on "The Ozone Mapping and Profiler Suite (OMPS) Limb Profiler (LP) Version 1 Aerosol Extinction Retrieval Algorithm: Theoretical Basis" *by* Robert Loughman et al.

Anonymous Referee #1

Received and published: 3 November 2017

This is a useful paper that describes the OMPS LP version 1 aerosol extinction retrieval algorithm in detail. The background and discussion around various issues involved with limb scatter aerosol retrievals is excellent and really provides some focus on systematic issues that are difficult to deal with. In my opinion, there are a few remaining "major" issues that should be considered; however, I think most are easily addressed. Overall the paper is well done and suitable for AMT.

Major comments:

- Abstract: Is there any evidence to suggest that horizontal variation in aerosol extinction is really a primary limitation? The discussion in the error section of the paper is

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good at a qualitative level, but there is no way to conclude this is dominant source of error. Is it greater than the precision? If this statement remains, can it be quantitatively estimated using model data of the expected variability and expected impact on the retrieved profile?

- The motivation for choosing a bi-modal size distribution remains unclear. The discussion about the various choices of size distribution in existing retrieval algorithms is helpful and really points out that this is a problem that needs to be addressed by the larger community (or at least form some consensus). However, one concludes (and the authors point out) from this discussion that we are really information poor on this aspect, so, why proceed to choose an even more complex size distribution requiring more unknown parameters? While it might be appealing to choose a bi-modal size distribution to "introduce the possibility of a 'coarse mode' of larger aerosols", the proposed algorithm does not provide any capability to actually use this in a meaningful way (i.e. it's static). Additionally, the ER-2 observations and the SAGE II Angstrom exponent during the post-Pinatubo time period at high altitude (30 km) are probably not the most representative of the conditions observed globally during the OMPS mission. Even so, the authors claim the SAGE II Angstrom exponent is relatively constant outside volcanically perturbed time period with alpha = 2, but it looks like (from Figure 8) the value is higher than that ($\sim$2.5) and comes down to 2.0 in the years immediately following Pinatubo.

- Is the signal to noise sufficient to use only a single point (h = 40.5 km) for the altitude normalization?

- The third conclusion in Section 3.4 relating to the correlation of normalized radiance and phase function ratio is not clear to me. Plot the correlation perhaps?

- Why not use a "color index" like the OSIRIS and SCIAMACHY retrievals? Also, the choice of 675 nm is motivated by the fact that this is the long wavelength normalization of the Chappuis band ozone retrieval. However, this requires the correction of ozone-

related absorption interference in the aerosol retrieval (which the error analysis shows can have an effect of up to 20%). Why not just move slightly further to the red end of the spectrum and avoid this? An aerosol extinction retrieval at 675 nm still requires some type of extrapolation across the Chappuis band for the ozone retrieval. If stray light is really the limiting factor, then a quantitative statement or plot in this regard would be helpful. Is there a reference for the statement regarding the increased sensitivity of ASD to longer wavelengths? (page 9, line 30-31).

- What is the spectral resolution at 675 nm? How is this handled in the forward model?

- Is three iterations of the retrieval sufficient for convergence, especially at low altitudes? Convergence is a tricky thing to pin down for a non-linear relaxation like this, but a statement about how much the retrieval profile typically changes with more iterations would be insightful.

- What about error due to stray light? Is there any knowledge about how well this is corrected in the Level 1 product?

Minor comments:

- Don't use acronyms/abbreviations in the abstract; prolific use throughout (AE, LP, GSLS, APF, ASD, LN, SO, LS) the manuscript makes it hard to read. For example "AE" is not a widely used acronym and it does make the text clumsy in my opinion

- Page 1, line 14: hydrated sulfuric acid

- Page 1, line 18: the spread of volcanic aerosol is also in the vertical direction, although much more slowly.

- Figure 1: Should not introduce instruments for the first time in a figure caption. Maybe better to first reference this figure in the text after the occultation discussion in the next section.

- Page 2, line 6: Not sure what is meant by "time of day" since the aerosol lifetime is so

long. “Time of year” is more applicable.

- Section 1.2, second paragraph: Mention should be made of the increased complexity required for LS in the forward modelling of the radiative transfer, i.e. multiple scattering, compared to occultation

- Page 2, line 14: don’t use “=” in a sentence, also page 4, line 17

- Figures 4 and 5: could be interesting and helpful to include an additional panel of solar scattering angle and aerosol phase function variation over the course of a year at various latitudes, since this will map to the seasonal biases in the retrievals

- Page 6: Strange to call out Fig 11 before Figs 6-10

- What tangent altitude is used for the calculations in Fig 11?

- Page 9, lines 18-19: it would be better to point out this difference in Figs 13 and 14 after the retrieval algorithm is explained.

- Equation 4 makes an assumption about the averaging kernel.

- Figure 19 is out of place and should be discussed.

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