

Interactive comment to “The Aerosol Limb
Imager: acousto-optic imaging of limb
scattered sunlight for stratospheric aerosol
profiling” by B. J. Elash et al

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We would like to thank the referee for their helpful comments and suggestions. Below are the referee's comments in italics followed by our reply.

Sect. 1, 2nd paragraph:

“the observations are essentially always limited to some degree” - This statement is unclear, but maybe it means that no single measurement technique provides the full range of aerosol properties unambiguously?

Reply: The text has been modified to improve clarity, and the sentence now reads “...although due to the variability of physical composition and particle size, no single measurement technique can fully determine the full range of aerosol properties unambiguously.”.

Sect. 1, 2nd paragraph:

“. . . Charlson et al., 1969); acquire . . .” the semicolon should be removed.

Reply: It has already been removed in the AMTD published paper.

Sect. 1, 3rd paragraph:

“. . . there are challenges associated with comparing the retrieved extinction profiles to other microphysical parameters” Do you mean that it is difficult to derive other microphysical parameters from the retrieved extinction profiles?

Reply: Partially, it is difficult to determine the microphysical parameters as well comparisons can be difficult between instruments. The text has been modified in the following to make this clearer. “have generally compared well with ground based and in-situ measurements, although there are challenges associated with determining microphysical parameters and comparison between instruments can be challenging.”

Sect. 1, 3rd paragraph:

“. . . allowing for straight forward retrieval” should be one word (straightforward).

Reply: Corrected.

Sect. 1, 4th paragraph:

It appears that the SAGE OSIRIS merged aerosol dataset is described as an “essentially continuous long term record” earlier. In that case, it might be useful to quantify how consistent retrievals from the 2 missions are, rather than simply reporting that they agree “relatively well.”

Reply: It has been noted and the relatively well has been quantified to generally within 30% below 30 km within the text.

Sect. 1, 6th paragraph:

“. . . has been studied extensively and somewhat controversially. . .” I agree that controversy has arisen, but don’t like this wording. The conclusions are controversial, but the issue hasn’t really been “studied controversially,” has it?

Reply: The sentence has been reworded for clarity to the following “the transport and origin of which has been studied extensively and the conclusions are somewhat controversial”.

Sect. 1, 7th paragraph:

It might be useful to measure the CATS mission, which recently began operation of a lidar on the International Space Station.

Reply: The CATS mission was unknown to us and and been added at the end of this paragraph has a current instrument that measures aerosol. The following sentence was added “More recently the lidar instrument Cloud Aerosol Transport System (CATS) (Chuang et al., 2013) has been placed on the international space station in 2015 for an expected mission lifetime of three years.”

Sect. 2.1, 2nd paragraph:

All equations are labeled oddly: The equation number appears to be part of the equation text (rather than right-justified). This is difficult to read and should be fixed.

Reply: This has been fixed in the AMTD published paper.

Sect. 2.1, 3rd paragraph:

“. . . acousto-optic diffraction angle is not constant angle with wavelength. . .” Do you mean “. . . angle varies with wavelength. . .”?

Reply: Yes that is the meaning of the sentence and the text has been altered for clarity. It now read “The acousto-optic diffraction angle varies as the filtered wavelength is changed...”.

Sect. 2.2, 1st paragraph:

“We also attempted to pay careful attention to stray light. . .” Unless you have something you’d like to confess in this paragraph, I think you can honestly say that “We paid careful attention to stray light. . .”

Reply: The changed has been made in the text.

Sect. 2.2, 4th paragraph:

“. . . this wavelength dependant change is negligible. . .” should say “dependent”, and could you quantify how small the change is?

Reply: The correction has been made. And this change is typically less than a micron for the entire wavelength range of ALI and the following has been added to the text “this wavelength dependent change is less than a micron for the current ALI design and is considered negligible.”.

Sect. 2.2, 6th paragraph:

“. . . , a not insignificant fraction of light” again, could you quantify this?

Reply: This is quantified in the following sentence. “The diffracted extraordinary signal comprises at most a ~ 10 nm bandpass fraction of one polarization such that the unabsorbed broadband signal from the polarizers can be on the same order of intensity as the diffracted signal.”

Sect. 2.2, 6th paragraph:

“. . . extraordinary signal compresses at most. . .” do you mean “comprises”?

Reply: Corrected.

Sect. 4.2, 1st paragraph:

This paragraph reports that an unexpectedly large amount of stray light was seen at high altitudes in the field measurements, but neither the magnitude of the expected stray light level nor the observed excess stray light level is clearly quantified here. I would like to see both numbers estimated.

Reply: The text has been altered to include this information and has added the following sentence: "For these altitude the ratio of signal to stray light was expected to be between 2-3 however some regions dropped down to an estimation of approximately slightly below one to one."

Sect. 4.4, 2nd paragraph:

"There are also several possible systematic errors not accounted for in the inversion including the choice of retrieval altitude ranges, particle size composition and distributions, stray light, and the high altitude aerosol load."

This sentence is confusing in a few respects:

1. Couldn't you adjust several of these parameters (such as retrieval altitude range, particle size distribution, and particle composition) to use the same assumption in the ALI retrieval as in the OSIRIS retrieval? Was this tried, in an attempt to sort out which differences might be most significant?

Reply: This was not entirely possible and was attempted. The same particle size distribution that is used for OSIRIS retrieval was used in the ALI retrieval. And the retrieved particle size is vary similar to the OSIRIS assumed distribution (OSIRIS assumes a mode radius of $0.08 \mu\text{m}$ whereas ALI found a size of 0.077μ both use a mode width of 1.6). However, the limiting factor leading to difficulty in using this method is that the OSIRIS retrieval uses a higher altitude for normalization which is outside of ALI's usable range. This results in always using a lower normalization point and results in a difference that could not be rectified by changing ALI's altitude range.

2. How is the high altitude aerosol load a source of systematic differences? Again, couldn't the same assumed value be used for both retrievals? (I assume we're talking about the aerosol above the retrieval range.)

Reply: Yes we are referring to the aerosol above the retrieval range. The

same a priori that is used for OSIRIS is used for ALI. However, just setting the aerosol above and below ALI retrieval range to the OSIRIS value causes discontinuities in the aerosol profile that cause non-physical or unconverged profiles for the retrievals. Instead the retrieval scales the aerosol profile above and below the retrieval range by the nearest scaling factor with the valid retrieval range determined by MART. Altering the slope of the a priori profile above the range was attempted to try to determine the effect and at most a 5% change was noted in the retrieved aerosol profile.

3. The phrase “particle size composition and distributions” is confusing. Maybe some commas are missing, but I think you mean particle size distribution and particle composition. It might be useful to state the scattering angle for the solar beam for the ALI observations, and to compare its value to the same value for the OSIRIS observations.

Reply: The phrase “particle size composition and distributions” has been replaced with “particle size distribution and particle composition” to help clarify this statement. The scatter angle for ALI and OSIRIS have been added to the text and well as a short comparison with the addition of the following sentences “The solar scattering angle for a measurement can also have an effect on the retrieved profile due sensitivity to the scatter cross sections from the particle size distributions. For the ALI image the solar scattering angle is 98 degrees and for the five OSIRIS scan they are 77, 89, 90, 91, 92, 93 degrees. With the exception of the forward scatter angles of 77 and 89 degrees from OSIRIS the scatter angles between OSIRIS and ALI are similar and should not cause a large effect on the retrieved profiles.”

Sect. 4.4, 3rd paragraph:

It sounds like the same aerosol size distribution is assumed at all altitudes in the retrieval, so can you explain what it means when an Angstrom coefficient that varies with height is observed in the retrieved extinction profiles? And how does the observed value compare to the value that Mie theory would predict for the assumed size distribution?

Reply: In the retrieval the Angström coefficient for each altitude is calculated independently and allows for each altitude to have a unique value. However when the state of the model is update the median of the altitude

dependent Angström profile is used as the value of the particle size distribution for the next iteration. This results in retrievals that yield altitude dependent coefficient but uses the median in the next iteration of the retrieval. A varying particle size distributions with respect to height in the retrieval algorithm results in instability in the method thus why the median was used. The observed Angström coefficient is approximately 2.7 whereas the assumed distribution is 2.3. The fine mode average of Angström coefficients from work derived from Deshler et al. (2003) are 2.1 to 3.4 and ALI's determined value is within the expected range as stated in the text. The addition of the retrieved median Angström coefficient and the Desher et al. (2003) range has been added to the text.

Sect. 5, 2nd paragraph:

“ . . . should be tackled in future iterations. . . ” should this say “tackled”?

Reply: Correct in the AMTD published version.
