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Atmospheric Measurement Techniques Discussions

Interactive comment on "Stratospheric aerosol characteristics from space-borne observations: extinction coefficient and Ångström exponent" *by* Elizaveta Malinina et al.

Anonymous Referee #2

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General comments on Malinina et al. (2018):

This paper provides a useful framework for discussion of the information content of limb scattering vs. occultation measurements. The abstract states that "limb instruments have better potential for the PSD retrieval." This statement appears to be true, primarily because (as shown in the text) the scattered radiances are more sensitive to smaller particles than the transmitted radiation measured by occultation instruments. However, this conclusion is somewhat weakened by the last sentence of the abstract, which (correctly) reports that the retrieved quantity (Angstrom coefficient for a single pair of wavelengths) could correspond to an infinite number of combinations of the PSD

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parameters. Two basic problems arise that deserve greater consideration:

1. The analysis presented throughout this paper assumes that the PSD has a single-mode log-normal shape. So the analysis shows that a set of median radius + mode width pairs could produce the same Angstrom coefficient, but confining all analysis to the single-mode log-normal possibility understates the ambiguity that actually exists: Many other types of PSD functions (bi-modal, gamma distribution, innumerable other functional shapes) exist that could also produce a given Angstrom coefficient.

2. Some work is cited that uses extinction measurements to infer PSD information, simply by assuming a type of PSD and selecting one of the many sets of PSD parameters that are consistent with the observations + the spectral variation derived from Mie theory (Yue, 1999, for example). In many respects, the same method is used here for limb scattering, and it is not clearly quantified how the approach leads to better results when limb scattering measurements are used (granting that this is likely to be true because, once again, the scattering measurements are more sensitive to smaller particles).

Specific comments:

Abstract:

The term “remarkable events” seems to be used as a synonym for “volcanic eruptions that perturb the stratosphere” (here and in several subsequent places). This should be clarified, perhaps by replacing that phrase with something more specific (such as “volcanic perturbations?”)

Sect. 1, 2nd paragraph:

The fact that the aerosols are assumed to be spherical should be explicitly stated here.

Sect. 1, 3rd paragraph:

The second sentence (“Known existing . . . (Damadeo et al., 2013)” is awkwardly

worded.

Sect. 2.1, 1st paragraph:

The solar/lunar occultation mode of the SCIAMACHY instrument is mentioned here – are those observations usable for this study? If the measurements have sufficient quality, they would certainly add to the limited number of coincident occultation + limb scattering measurements.

Sect. 3.1, 1st paragraph:

As discussed earlier, the failure to consider the consequences of any PSD other than single mode log-normal is a significant limitation of this study (just as it is a significant limitation of most limb scattering aerosol retrieval work to date). Even a single example testing this approach for another type of PSD (realistic, but not single mode log-normal) would add significant value to this study, by providing some indication of how much the results presented depend on that restriction.

Sect. 3.2, 5th paragraph:

It would be helpful to provide some guidance about the sigma value that corresponds to w = 0.01 microns for a few examples, since this is an unconventional way to describe the PSD.

Sect. 3.2, 7th paragraph:

The basis for the perturbation analysis presented is confusing: Apparently the temperature and pressure are treated as variables that can be perturbed independently (without regard for hydrostatic balance, for example)?

Figure 9:

This figure illustrates some limitations on the conclusions that can be drawn from this analysis due to the constraints that have been imposed on the PSD during this analysis: What does it mean to perform a retrieval based on a single PSD, then analyze the

variation of Angstrom coefficient with altitude (which should be zero, if a single PSD truly characterizes the stratospheric aerosol)? Or am I misinterpreting something in the methodology?

References:

Yue, G. K.: A new approach to retrieval of aerosol size distributions and integral properties from SAGE II aerosol extinction spectra, J. Geophys. Res., 104, 27 491–27 506, 1999.

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