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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.

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We thank the reviewers for the time they spent thoroughly reading the manuscript and commenting on the paper. We hope we have answered the reviewers' questions and improved the explanations where needed. To distinguish the referees' comments from the author's responses, the comments are shown in italicized font and the responses are highlighted in blue.

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 instru-

ments 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 parameters.

Based on this and next comments of the reviewer we realized, that there might be a misinterpretation of the general concept of our research. In our method, we used the retrieved PSD parameters: R_{mod} and σ (the retrieval method was presented in Malinina et al. (2018)); and then from the retrieved PSD parameters extinction coefficients and Ångström exponents were recalculated with Mie theory. We understand that this misinterpretation was caused by our formulations, so the text of the revised manuscript has been changed accordingly.

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.

We agree with the reviewer, that there is an ambiguity in the assumption of the PSD shape only by one type, and in the reality the shapes might change from one event to another. However, for our PSD parameters retrieval (see Malinina et al. (2018)), the uni-modal log-normal distribution is assumed. The study presented here is based on

this particular product and, thus, is related to this particular assumption, which for now haven't been proven to be either better, or worse than the other ones, suggested by the reviewer. For that reason, we find it unnecessary to conduct studies with the other assumptions, as completely new retrieval algorithms need to be developed for this purpose, and there is not enough information to retrieve, e.g., bi-modal distribution. We changed the text of the manuscript to highlight, that our conclusions are based on our particular product and particular assumption.

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).

As mentioned in the first reply to the reviewer, there is a misinterpretation of the study concept. We do not use the method, which is usually applied for occultation measurements. In the previous paper (Malinina et al., 2018), which was cited in the manuscript, we state, that SCIAMACHY PSD retrieval does not use the retrieval of the aerosol extinction coefficient as an intermediate state. This particular retrieval algorithm retrieves R_{mod} and σ from the limb radiances directly. For this reason, we consider this reviewer's comment as inapplicable. The misinterpretation was caused by poor wording of the original manuscript. To make the text of the revised manuscript more clear, we added a more detailed description of the algorithm, and highlighted that the study is based on the PSD product.

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?")

By the remarkable events here and further not only the volcanic eruptions were meant, but also biomass burning events (e.g. Black Saturday in 2009) or any maintenance works or degradation of the instruments. We clarified that in the revised manuscript.

Sect. 1, 2nd paragraph:
The fact that the aerosols are assumed to be spherical should be explicitly stated here.

The assumption on the aerosol form has been added to the text.

Sect. 1, 3rd paragraph:
The second sentence ("Known existing ... (Damadeo et al., 2013)" is awkwardly worded.

The text has been revised in accordance with the reviewer's comment.

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.

SCIAMACHY solar and lunar occultation measurements were done around 60° latitude in both hemispheres. Since currently the PSD product is limited only by the tropical region (20° N –20° S), there is no possibility to include those in the study. Additionally, yet there is no PSD product from SCIAMACHY occultation measurements. However, in the future the synergistic use of the both modes will be tested.

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.

We chose to use uni-modal log-normal distribution because that is the one, which is assumed for our PSD retrieval algorithm. Additionally, uni-modal log-normal distribution was assumed for such instruments as SAGE II and OSIRIS. The suggested by the reviewer study is interesting as itself, but is outside of the scope of this paper.

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.

The σ values corresponding to some combinations of R_{mod} and w were provided.

Additionally, in Sect. 3.1 the formula of w calculation is presented (see Eq. (2)).

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)?

Although the reviewer is right, that the temperature and pressure are not independent, for this particular study it does not play a major role. For SCIAMACHY PSD product, ECMWF operational analysis data for the specific date, time and location of each SCIAMACHY limb measurement were used. The same data was used in this study. This product has about 10% uncertainty for each of the variables. The other possible datasets (e.g. MERRA) have a similar uncertainty. Thus, it was decided to change pressure and temperature independently. This explanation was added in the manuscript.

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?

Because of the poor wording in the original manuscript, the reviewer misinterpreted the methodology of the study. The PSD parameters were first retrieved and then Ext and Ångström exponent was recalculated from it. Since the PSD varies, Ångström

exponent varies as well. We added more clear explanations to the text regarding the way Ångstöm exponents were obtained.

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.

References:
Malinina, E., Rozanov, A., Rozanov, V., Liebing, P., Bovensmann, H., and Burrows, J. P.: Aerosol particle size distribution in the stratosphere retrieved from SCIAMACHY limb measurements, Atmos. Meas. Tech., 11, 2085-2100, https://doi.org/10.5194/amt-11-2085-2018, 2018.

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