Atmos. Meas. Tech. Discuss.,
doi:10.5194/amt-2020-277-RC1, 2020

Atmospheric Measurement Techniques Discussions

Interactive comment on "Retrieval of stratospheric aerosol size distribution parameters using SAGE-III/ISS extinction measurements at three wavelengths" *by* Felix Wrana et al.

Anonymous Referee #1

Received and published: 14 October 2020

General Comments

This paper describes an approach to retrieve the median radius and mode width of a particle size distribution supposed to be lognormal. The method is based on the use of a look-up-table of extinction ratios computed from extinction channels chosen adequately. The method is applied on measurements from the SAGE III experiment on the International Space Station (ISS). This study many similarities with the approach used by Echle et al. (J. Geophys. Res., 103, 19193, 1998), and it might be useful to cite this work which addressed aspects and issues relevant in the present context. The paper is written in a clear way and overall, the methodology and data analysis are

Printer-friendly version

Discussion paper

carried out with the necessary care. The topic of this study is very relevant, but some important imprecisions should be first addressed and clarified before publication.

Specific comments

L. 20, p.1-L. 32, p.2: For the completeness, the growing importance of large fires in the feeding of the stratospheric aerosol layer should also be mentioned.

L. 30, p. 2: “while sedimentation . . . aerosol layer”: this sentence should be rephrased: sedimentation limits the averaged size, not the size of individual particles.

L. 30, p. 2: “Evaporation . . . temperatures”: Since the last aforementioned atmospheric layer is the troposphere, the authors should mention again that they are now considering the stratospheric altitudes.

L. 66-68, p.3 : For the sake of completeness, the authors should also cite works by Bauman et al., (e.g. Bauman et al., J. Geophys. Res., 108, D13, 4382, 2003), and Bingen et al. (e.g. Bingen et al., Ann. Geophys., 21, 797-804, 2002).

L. 116, p.5: “As a result”: It is the other way around: Making the assumption that aerosol particles are spherical, the Mie theory can be used.

L. 122-123: Is there any compelling reasons to make this hypothesis (e.g. possible convergence of an iterative process to unrealistic solutions with a mode width out of range), or is it just a matter of defining a realistic range for the LUT ?

Figure 2: It is not clear, from the caption, what is the meaning of the numbers annotated in the figures (“50nm”, “60nm”, etc.). It is also not clear to which point these values refer. The use of arrows could help specifying the link between the values and corresponding dots (if this is the link the authors mean).

L. 54, p. 2 and L. 131, p.6: The acronym “PSD” should be defined in l. 54, p. 2.

L. 133, p. 6: n and k are determined by the assumption made in l. 114-119, p.5. This should be clarified. It might also be useful to mention, here or in the paragraph on l.

154-157, p.7, that these indexes of refractions are wavelength-dependent.

L. 146-153, p. 7: In §2, it is mentioned that the extinction at 1543.92 nm is based on a 30-nm bandwith channel. While the relative uncertainty for this channel is twice the uncertainty at 1021.20 nm (See Table 1), the uncertainty on the wavelength is much higher than in the case of the other channels, including 1021.20 nm for which the spectral resolution is probably about 2 nm (as mentioned in §2). Did the authors perform a sensivity study to evaluate what the impact of these increased uncertainties (on both the extinction and wavelength) is, how it affects the retrieval of the PSD mode parameters, and to which extend the choice of the 1543.92 nm channel is better than the one of 1021.20 nm (if it is).

L. 163-164, p. 7: How does this interpolation occur ? Following the example give in Fig. 3, the solution for each tangent altitude is likely to cover a large range of sigma-values. Did the author perform some regression ?

L. 165-169, p. 7: I don't see why it is necessary to exclude solutions with large values of ïĄş. In all cases, a range of solutions is likely to provide the set of extinction ranges, taking into account the uncertainty of the different extinction channels. Moreover, a large value of the mode width could be useful as an indication that the assumption made on the aerosol composition is not appropriate (e.g. due to the presence of clouds). In the past and in other frameworks, the exclusion of "unrealistic values" led to overlook unexpected but critical physico-chemical processes, as important as the discovery of the ozone hole. This should make the authors cautious while rejecting values.

L. 170, p.7-l. 184, p. 8: This argumentation is not true because it considers the response of a single particle, and not of a population of particles with a possibly large value of mode width. Hence, it does not take into account the fact that the combination of responses from all individual particles with slightly different radii "blurs" the extinction efficiency signal, in particular in the case of thin particles with respect to the wavelength. It this case, the extinction curves may be much less distinct, and the re-

trieval of the mode parameters, much less reliable. It should be noted that restricting the allowed range of mode width values may alleviate artificially the problem, leading again to overlook possible solutions.

L. 193, p. 9: How do the authors choose the extinction channel and why ? This should be specified. Would it be meaningful to consider all of them to reduce the uncertainty?

L. 210-213, p. 10: Also the uncertainty on the wavelength might play a role, see comment on l. 146-153, p.7.

L. 239-243, p. 11: See comment on l. 165-169, p. 7, and l. 170, p.7-l. 184, p. 8.

L. 304, p. 15: Please indicate the geolocation of Calbuco to ease the analysis of the figure.

L. 324-325, p.16: The comparison does not only depend on the accuracy of the mode parameter retrieval, but also upon the extend to which the extinction spectral dependence for the actual aerosol population is well described by the Angström law.

L. 337-338, p. 17: This statement has to be qualified and reformulated: indeed, no assumption is required to retrieve the aerosol extinction, but conversely, the authors did use an assumption on the particle size (i.e. lognormal function) to derive expressions of the different mode parameters.

L. 339, p.17: The authors should specify they consider solar occultation in the present case.

L. 346-348, p.17: I am not sure I understand this statement: if the measurements are not independent, off-diagonal terms of the covariance matrix have to be additionally taken into account, and the risk of systematic error may be higher.

Technical corrections

L. 250, p.11: “were compared”. L. 261, p.12: Did the authors check that the excluded values are not likely to be due to high aerosol load after a volcanic eruption (e.g., from

their

Interactive comment on Atmos. Meas. Tech. Discuss., doi:10.5194/amt-2020-277, 2020.

Printer-friendly version

Discussion paper