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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Optimal estimation retrieval of aerosol microphysical properties from SAGE II satellite observations in the lower stratosphere" *by* D. Wurl et al.

Anonymous Referee #2

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Review of

Optimal Estimation Retrieval of Aerosol Microphysical Properties from SAGE II Satellite Observations in the Lower Stratosphere

by D. Wurl, R. G. Grainger, A. J. McDonald, and T. Deshler

General comments:

This is an interesting and well written manuscript dealing with the retrieval of microphysical properties of stratospheric background aerosols using SAGE II solar occultation observations at different visible and near-IR wavelengths. The novel aspect of this

study is the use of the optimal estimation technique that allows combining measured information with a priori information on the fraction of the aerosol particle size distribution that has essentially no impact on the occultation observations. As the authors put it, using a priori information allows filling the 'blind spot' associated with the occultation measurements. This concept appears plausible and useful at first glance, but it is associated with several problems, that are – in my opinion – not adequately addressed in the paper, as outlined in the following general and specific comments.

I start with two general comments:

a) If the 'blind spot' is filled with a priori information, how useful are the retrievals then? This certainly depends on the fraction of aerosol particles that are below the detection threshold. You write several times, that the occultation observations are insensitive to aerosol particles with radii less than about 100 nm. According to Fig. 2 and Table 3 the mean median radius of your SAGE II retrievals is about 70 nm, i.e. less that the rough sensitivity threshold of 100 nm. Looking at the histogram in Fig. 3b I estimate that less than 2 – 4 % of the retrieved median radii are actually larger than 100 nm, and I guess this implies that almost all of this information comes from the a priori, and not from the observations. This is a major issue and must be addressed. The higher order moments are certainly not as affected.

b) Following the previous comment the choice of the a priori information becomes very critical here. You use in-situ observations at 41N latitude for the period 1991 to 1997. Several important issues arise from these assumptions that are not discussed in the manuscript at all. During this period the Pinatubo eruption occurred, which means, that the ensemble of in-situ measurements does clearly not provide an appropriate a priori for the retrieval under background conditions. Secondly, using a priori information valid for a certain latitude is not appropriate for retrievals at all latitudes. Although the comparisons with in-situ observations in 1999 are done at the right latitude, section 4.2 suggests that all SAGE II observations in December 1999 ('19700 retrieved results') were retrieved with the same a priori.

Issue a) can certainly not be solved with the present approach, but it should be mentioned and discussed in detail in the paper. The paper is still worth to be published, but it should actively address the issues associated with the basic approach.

All in all, the manuscript requires major revisions before becoming acceptable for publication. I also would like to point out, that the paper is very well written and structured, and I believe it will make an interesting and relevant publication once the limitations of the method are discussed in more detail.

Specific comments:

Page 23720, line 26: 'For instance, by scattering a large portion . . .'. I think 'large portion' is an exaggeration. If I'm not mistaken the typical radiative forcing of stratospheric background aerosol is less than 1 W / m2, which is important, but clearly not a large portion of the incoming seasonally and globally averaged solar irradiance (342 W / m2).

Page 23724, line 10: I'm surprised to read that the imaginary part of the refractive index is zero. It's quite small, OK, but I don't think it's zero. In the introduction you also write 'Through scattering and ABSORPTION of . . .'

Page 23726, line 2: 'to particles with radii less than 100 nm'. This threshold appears several times in the paper, and I wonder how robust it is. Considering that almost all of the retrieved sizes are below this threshold (see general comment above) it would also be very useful to provide a more quantitative estimate of the contribution of these smaller particles to the observed extinction signal. This can be easily done using Mie-calculations.

Page 23731, line 13 and Fig. 3: The vast majority of the retrieved median radii is smaller than 100 nm, i.e., below the sensitivity threshold. I suggest estimating what fraction of the derived sizes actually affects the observations.

Page 23732, line 15: 'they are both spherical and homogeneous'. This is a minor point,

but can we really know that they're truly spheres? Such a statement cannot be verified, I believe.

Page 23739, line 32: The SPARC report was compiled and edited by Larry Thomason and Tom Peter. I suggest they should be listed as authors (or at least editors) of the report rather than the SPARC steering group.

Page 23741, caption Table 1: 'these correlation coefficients are all significant'. Significant at what confidence level?

Page 23745, Table 5, (2)/PCA: I don't understand '(15-20)+50', guess there's something wrong here?

Typos etc.:

Page 23721, line 13: I suggest rearranging words from 'retrieval aerosol problem' to 'aerosol retrieval problem'

Page 23739, line 15: 'trueber' -> 'tr\"uber' and 'Metalloesungen' -> 'Metall\"osungen'

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 23719, 2009.