Atmos. Meas. Tech. Discuss.,
doi:10.5194/amt-2018-221-RC2, 2018

Atmospheric Measurement Techniques Discussions

Interactive comment on "Improvement of stratospheric aerosol extinction retrieval from OMPS/LP using a new aerosol model" *by* Zhong Chen et al.

Anonymous Referee #2

Received and published: 9 September 2018

In this paper, the authors use a gamma distribution fit to a sectional aerosol model run (coupled to a GCM) to derive aerosol scattering phase functions. These are used in the radiative transfer forward model for the retrieval of aerosol extinction coefficient profiles from the limb scattering measurements made by the OMPS Limb Profiler. The assumption of the aerosol size distribution in the radiative transfer model for aerosol extinction retrieval from scattered light measurements is a long standing problem, and it boils down a basic lack of information in the remote sensing measurement to make a bias free retrieval. Other groups working on similar limb measurements with SCIAMACHY and OSIRIS have tried other forms of the size distribution with no real agreement or even criteria for what is best.

In general this is an insightful paper and the sample data sets that are presented show improvement over the previous version of the OMPS retrieval that used a bi-modal log-normal distribution. However, there are two related major points of concern. The first is that the main source of proof for improvement presented in the paper is analysis of a one month test data set (plus the six month time period used for SAGE III intercomparison). It does indeed seem that the gamma distribution is an improvement over the previous bi-modal assumption from V1.0; however, as the authors point out, the actual aerosol size distribution is a strong function of time, latitude, volcanic perturbation, etc., and it could very well be the case that the retrieval is worse at other time periods that are not analyzed. The V1.0 bi-modal assumption certainly had difficulties (choice of 5 free parameters, uncertainty in fitting OPC data) and the gamma function is demonstrably better, however, what about the simple unimodal size distribution assumed by the OSIRIS and SCIAMACHY algorithms? These are also simple 2 parameter distributions that roughly match the (measured, not modelled) background aerosol state. The corresponding phase functions for these distributions should be compared to the gamma distribution used here, and a clear case made for the use of gamma distribution. Since the bias is such a strong function of solar scattering angle, which for OMPS is essentially a latitude dependence, it might be the case that a "better" choice for OMPS is not a better choice for an instrument in a different orbit. Overall, users of limb scatter aerosol products would benefit from uniformity in the algorithm choices between the various groups, or at least publications that show how/why the assumptions are different.

The second major point is a more philosophical point about the use of model data in the retrieval. The authors are not yet using space and time dependent model size distribution, but they allude to this work as the first step towards that plan. To do this the authors must make a convincing case that the information folded into the retrieval from the model size distribution makes the result substantially better in a way that is quantifiable. The bias resulting from uncertainty in the aerosol size distribution is a second order effect that can be understood and characterized in a relatively simple way. But now will introducing a complicated spatially/temporally varying model distribution make enough improvement to push this uncertainty to a third order effect, or will it just modify the results so that the second order effect is harder to understand and characterize due to the complex nature of the input assumption? Again, I realize this is not the case for this paper, but anticipation of this as an obvious next step is worrisome. Some of these issues should at least be discussed and approached with caution, especially as with this paper they have chosen to move away from using in-situ measurements to using model output.

Finally as far as I can tell, this paper is essentially a revision of Chen et al., 2018, which was not submitted for final publication. It seems than that this paper should stand independently and not reference the previous discussion paper, although the editor should weigh in on this.

Other minor comments:

Page 2, 3rd paragraph: The SCIAMACHY and OSIRIS work must be better referenced and discussed to put this work in context. These groups have done much more work, especially with regard to the size distribution, since the papers that are briefly mentioned here.

Several places throughout the paper refer to results from internal validation tests. It seems that some of these are shown and some are not. Is this simply referring to testing of the algorithm performance without validation data from other instruments? If not, this language is frustrating and it leaves the reader wondering what is behind the scenes.

Why choose the CARMA simulation for no volcanic eruptions? If the goal is really to use representative model data, why not run as realistic a simulation as possible for the OMPS mission time frame (which is definitely influenced by small volcanic eruptions) and then choose the median or average distribution? Why do the authors then choose to analyze a one month period that is perturbed by the Calbuco eruption?

What does a 10% change in the gamma distribution parameters mean in terms of particle size? Is this realistic for a moderate volcanic eruption?

Is there any potential for stray light or calibration effects in the interpretation of the spectral residuals? Why does the southern hemisphere look extremely good, where most of the differences in the phase functions seem to be for backscatter angles?

The refractive index should be representative of hydrated sulfuric acid and referenced.

The symbol E for aerosol extinction is not standard. Why not k? Then E switches to x in Equation 3. “Extinction ratio” is usually used for the ratio of aerosol to molecular extinction. What is the point of the discussion of the non-linearity of the ratio of the data versions with reflectivity?

Interactive comment on Atmos. Meas. Tech. Discuss., doi:10.5194/amt-2018-221, 2018.