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Atmospheric Measurement Techniques
Discussions

Interactive comment on "Stratospheric aerosol particle size information in Odin-OSIRIS limb scatter spectra" *by* L. A. Rieger et al.

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The paper focuses on a particle size distribution retrieval from measurements of the scattered solar light in limb viewing geometry. As this topic is quite new and not yet well investigated the paper is of a great importance for scientists working on aerosol retrieval. The authors demonstrate the importance of the size distribution knowledge for the aerosol extinction coefficient retrieval and describe a method to obtain this information from OSIRIS measurements. The paper would greatly facilitate development of similar retrieval methods for other satellite sensors if a detailed study was presented. Unfortunately information provided by the paper in its current form is too general and a lot of details is missing. Several important investigations are not done or their results

are not shown. In general, the paper is written quite inaccurate. Furthermore, the presented study seems to be a continuation of the work published before in Bourassa et al. 2008a. This however is not clearly stated and any discussion of commonalities and differences is missing (with exception of a very shot remark at page 5073).

Despite the current deficits the paper is very promising and certainly will be suitable for publication in AMT after my comments have been addressed.

Detailed comments:

- page 5067, lines 7-1: OMPS has to be mentioned and cited.
- page 5067, lines 19-20: "... or retrieval of aerosol parameters in addition to extinction coefficient" - this statement is confusing because if you make an assumption about aerosol parameters (composition and particle size distribution in absolute units) the extinction coefficients are determined and do not need to be assumed additionally. I suggest to delete the end of the statement starting form "in addition" .
- page 5068, line 3: "As Odin is scanned..." - confusing statement. First, the passive form seems to be unsuitable here. Second, I guess this is not Odin (satellite platform) which scans. It should be rather OS instrument. Otherwise IRI would be scanning as well which is not the case.
- Eq. (3): At a quick look it is unclear to me how a normalization to model data can increase sensitivity of a measurement. Please justify the statement.
- Page 5070, lines 3-4: "Finally, the measurement vector is normalized by one or more high altitude measurements" - a couple or lines below you write that you use multiple normalization altitudes. Why do you need to confuse reader by a multiple choice here?

- Page 5070, lines 7-8: "... normalization is chosen over an altitude range such that Eq. (3) is at a minimum within a chosen noise margin" - the statement is totally unclear. What is a noise margin? How it is defined for such a complicated combination of values. Please provide values. What should be at minimum, is it y_i? If you look for a minimum y_i which is still larger than some margin this will give you one boundary of the altitude range, where is the second one? Please rewrite the description making it easier to understand.

- Page 5070, Eq. (4): Provide typical values of the N-th and (m-N)-th altitudes and of N.

- Page 5070, Eq. (4): The definition of matrix W is quite difficult to understand. It would be better if you could write a formula to calculate elements of the matrix.

- Page 5071, discussion of Fig. 1: It should be clearly stated that the discussion is valid under assumption that there is no diurnal variation of aerosols.

- Page 5071, line 28: "This represents the worst case for OSIRIS ..." - what is the worst case? Are you talking about the shift in the local time or about the systematic difference in the extinction ratios?

- Page 5072, line 1: "...lower altitudes and mid to high latitudes do not show this bias to nearly this degree" - please explain why.

- Page 5072, line 15: "this is similar to Eq. (6)" - I guess you mean Eq. (4).

- Page 5073, lines 2-5: please provide the parameters for the used particle size distributions.

- Page 5073,line 11: "... the measurement vectors below approximately 800 nm provide almost no discrimination between particle sizes" - Looking at Fig. 3 it

is impossible to judge if this statement is true. It is obvious that the measurement vector per definition provides no sensitivity around 750 nm. However, below about 650 nm the curves are different. It is just difficult to judge whether the difference is significant or not. A relative difference plot could help.

- Page 5073, Fig. 3: The small plot with size distributions is hardly readable.

- Page 5073, Fig. 3: It would be interesting to see similar results for other scattering geometries.

- Page 5073, lines 25-26: "... there is a solar scattering angle, or viewing geometry, dependence on particle size" - The statement sounds quite messy to me. Please rewrite.

- Pages 5074, Eq. (8): This equation describes only the scattering term and thus seems to be incomplete. I would also expect to see a multiplicative extinction term describing the attenuation of the light coming from the Sun and traveling towards observer after the scattering event. This term should also contain the aerosol extinction coefficient. This makes however measurement kernels, $K_k(r)$ non-linear already for the single scattering (multiplication of two terms containing sums of contributions from different particles). Thus, I doubt, if the concept of measurement kernels is suitable for limb observations. Indirectly you recognize this fact by throwing K away in Sec. 4 and using the Jacobian concept instead. So why do not you skip the "K"-concept from the beginning and redo Sec. 3 based on Jacobians? This will at least eliminate any concerns about the validity of the used concept.

- Pages 5074-5075: It is unclear how the vertical inhomogeneity of the aerosol number density and size distribution is handled within the measurement kernel concept. Bearing in mind the complexity of the limb scattering geometry, it looks suspicious that the information content can be analyzed for each tangent height independently. There should be some assumptions behind the method which are not sufficiently discussed in the paper. If you decide to keep the concept please include more detailed discussion on the validity and possible limitations of the method.

- Page 5076, line 2: How the matrix K is defined? I suppose this is a matrix containing all $K_k(r)$ but this is not explicitely stated in the text. Furthermore, the same notation is used at page 5070 for another matrix. This is confusing and needs to be changed.
- Page 5076, Eq. (10): the much-greater-than sign "$>>$" used in Eq. (10) is not conform with the conclusions made below. This sign assumes usually that the left hand side of the equation is at least an order of magnitude larger than the right hand side. This means, however, that both cases result in one piece of information. Please reconsider the discussion or replace the sign by greater-than sign "$>$".
- Page 5076, after Eq. (10): please provide typical noise values for OSIRIS measurement vector.
- Sec. 3: How do the conclusions of this section change if you (i) add 470 nm wavelength, (ii) change the assumed mode width of the particle size distribution?
- Page 5077, Eq. (12): It looks like you solve this equation for each altitude independently. Is it really the case? If yes it seems to be a major drawback with respect to Version 5 retrieval because the layer interaction is completely ignored. Is the MART technique still applied? Please provide a proper description of the retrieval method with respect to the vertical distribution of the parameters.
- Page 5077, lines 9-10: the method to calculate Jacobians is unclear from this description. Please add more details. What is actually "the forward difference method"?

- I doubt if the norm of Jacobian is a suitable convergence criterion. A small norm tells you that you cannot move far away from your current guess but it does not mean the fit and solution are good. If no additional checks are done it is rather a criterion to throw the obtained result away.
- Page 5078, line 1: "due to the high altitude calibration" - I guess you mean normalization.
- Page 5078, Eq. (18): It is unclear why you are writing this equation because the matrix is not used further in the course of the paper. Furthermore, it is unclear how you translate the measurement error into the error of the retrieved quantities (statement at the end of Sec. 4.1). I cannot accept the reference to Rodgers as an explanation of the method because it is not clear which formula you use and if it is suitable to quantify the errors of your method. So please provide a description how you estimate the errors in the retrieved quantities mentioned at the end of Sec. 4.1.
- Page 5079, Sec. 4.2: I guess the formula to calculate the averaging kernels is given by $A = \frac{\delta x_{ret}}{\delta x_{true}}$. This needs to be clearly stated in the paper. The averaging kernels depend possibly on the assumed "typical" values for the extinction coefficient and the mode radius (and may be also on the distribution width). This must be checked and the results need to be reported (at least in a form of a short discussion).
- Page 5079, Sec. 4.2: In many publications which analyze averaging kernels also such retrieval characteristics as measurement content and vertical resolution are provided. I would suggest to include these characteristics also in your paper.
- Page 5079, Sec. 4.2: Please provide the formula to calculate the smoothing error.
- Page 5079, Sec. 4.2: The investigations (and corresponding plots) need to be

done for more than one observation geometry. The effect of underlying clouds (i.e., elevated altitude of the reflecting boundary) needs to be investigated.

- Page 5080, Sec. 4.3: The errors shown in Fig. 6 disagree with the results presented in Secs. 4.2 and 4.3. For example, the albedo error is much lower and the statement "In the retrieved quantities this translates to an error of 10% in the retrieved quantities near the peak of the aerosol layer." is not confirmed by the right panel of the plot. Please comment on these issues and provide plots for a couple of other observation geometries.

- Page 5080, Fig. 7: Please provide the percentage error.

- Page 5080, Fig. 7: Please provide similar plot for different particle radii.

- Sec. 4: An investigation of possible errors due to the assumed mode width of the particle distribution is clearly missing.

- Page 5081, Sec. 4.5: It would be nice to see a couple of plots showing the initial guess, true, and retrieved extinction profiles for several typical observation geometries. The provided information does not allow to make any conclusion on how different these profiles are.

- Page 5082, Fig. 9: Looking closely to the plot one sees that a significant improvement is achieved until the first half of 2004 while it is much less pronounced thereafter. Moreover the data points in the middle of 2007 are completely missing in Version 6. Please comment on this issue.

- page 5082, Fig. 10: Zero lines are hardly visible.

- page 5082, Fig. 10: Please provide the comparisons resolved with respect to latitude ranges (e.g., tropics, SH, NH).

- page 5082, Fig. 11: Please show cross section plots (i.e. Ångström coefficients vs. altitude for both OSIRIS and SAGE II similar to Fig. 10). Please also show results for other latitude bands.

- page 5082: "... indicating that where larger particles are present the Ångström coefficient derived from SAGE II should be systematically smaller than those from OSIRIS." - this is however not the case between about 20 and 27 km. Here, the Ångström coefficients are smaller, thus the particles should be large and SAGE II is expected to show smaller values. However, the opposite is the case in Fig. 11, i.e., SAGE II values are large than those of OSIRIS. If you want to explain possible differences by different spectral ranges please provide some modeling results to estimate the expected difference.

- page 5082: If the Ångström coefficients are that sensitive to the spectral range why do not you just compare the effective mode radii derived from both satellites? These should be independent of the wavelength range.

Interactive comment on Atmos. Meas. Tech. Discuss., 6, 5065, 2013.