Interactive comment on "Odin-OSIRIS stratospheric aerosol data product and SAGE III intercomparison" by A. E. Bourassa et al.
Anonymous Referee #1
Received and published: 23 September 2011

Authors' response in red: Thank you for the review and the suggested changes. We would be pleased to respond with a revised manuscript.

The authors present a revised version of the aerosol retrieval algorithm, improving the convergence of the iterative retrieval process and increasing the sensitivity to the aerosol signal, as well as a new normalization improving the robustness with respect to the measurement noise. Further, they introduce a simultaneous retrieval of the albedo in order to take into account correlations between this parameter and aerosol extinction. Finally, the retrieved aerosol extinction is validated using the SAGE III data set over the period 2002-2005.

The paper presents a very careful and complete analysis of all the aspects of this complex problem, as some convincing illustrations. I would suggest several minor revisions and I would be happy to have the author's comments about some scientific issues for the following points:

• L. 7 p.25788: What is precisely 1 km at the tangent point?
Good point. It is the vertical size of the field of view of the line of sight. This is clarified in the text and we have also added the horizontal size of the field of view.

• L. 4-5 p.25790: For the sake of clarity, the authors should precise which quantity corresponds to the denomination "measurement vector".
Sorry, we believe this confusion comes the fact that we've used "measurement vector" and "retrieval vector" interchangeably. We have changed the text to use only the term "retrieval vector" consistently.

• L. 10-16 p.25790: Did the author check the sensitivity of the retrieval to the choice of particle size distribution (PSD) ? The choice of PSD is representative for background condition, which is a poor choice in some situations such as periods following a volcanic eruption (as mentioned later on).
Yes, this is referenced in Bourassa et al., 2007 and documented there with a detailed study. We have tried to be clear that the SAGE III comparisons that are possible for this study represent a typical non-volcanically modified "background" state.

• L. 17-18 p.25790: The authors define correctly x as the state parameter, but for the sake of clarity, they should mention which atmospheric parameter it represents.
Yes, this is now clarified.

• L. 24 p.25791: The role of the k index is not defined, and does not appear in the right-hand side of the equation. Do I understand well that there is no more normalization of the radiance in the first term of the right-hand side? Why did the authors remove this normalization?
The k index was extraneous and is now removed. Indeed the normalization is removed; this is because we then introduce the offset method in the following section, which provides a better normalization method.

• L. 9 p.25793: Same problem with index k as in L. 24 p.25791
Done.

• L. 8 p.25794: I suggest that the authors revise their sentence as "… that can be used to determine the offset…" for the sake of clarity (possible confusion with "that can be used for the retrieval itself").
Yes, good point. Done.

• L. 27 p.25794-1 p.25795: Would it be possible that the result of the alternate iterative retrievals of the aerosol extinction profile and of the albedo depends on the initial guess, i.e. that the combination of both problems lead to a probability density function with several local maxima, able to be reached by a suitable choice of the initial guess ?
Indeed, this is always a concern with these multi-variate non-linear problems; however, we have tested this over a range of conditions under simulation. Additionally, the demonstrated agreement with SAGE III indicates the OSIRIS retrieval has converged to the actual solution.

• L. 13-14 p.25795: Do I understand well that the authors mean "increased Rayleigh scattering" in this sentence ?
No, in fact we meant "aerosol scattering". This is clarified.

• L. 10-18 p.25795, also l. 16-18 p.25800: Do I understand well that this problem becomes more acute if the aerosol concentration (and hence the extinction) increases? And what would happen in the case of high volcanic load (let's suppose that the assumption on the particle size distribution is then adapted accordingly to the situation). In such a case, the displacement of the dominating aerosol mode toward coarse particles can induce a roughly constant of even positive spectral dependence of the extinction, instead of the decreasing dependence observed for background aerosols [cf. Brogniez et al. (1996), JGR, 101, 1541]. Would it be problematic for the aspect considered here? Are there conclusions to be drawn about the applicability of the retrieval method in case of high volcanic load?
Yes, that is correct. The problem is essentially related to the total extinction, and not really the particle size or spectral dependence of the aerosol scattering. As the total extinction increases, the lower altitude boundary of the retrieval is increased. In principle one could develop a wavelength-dependent table of altitude limits based on aerosol extinction. This is a good idea for future work.

• L. 17 p.25795: Some explanation should be given on this "positivity constraint", possibly when introducing the retrieval scheme on Eq. (2), or by just citing some reference where it is discussed.
The references are those which are introduced in the same sentence (Bourassa et al, and Rault and Loughman) We hope that's clear.

• L. 3-5 p.25796: Could the authors give some idea about the improvement using the new retrieval vector in terms of number of iteration to get the convergence ?
Yes, the original algorithm required > 20 iterations compared to typically <10 for the new algorithm This is now included in the text.

• Fig 7: This figure is remarkable and shows the added value of a limb scattering instrument in detecting the local variation of the aerosol load in the stratosphere. However, in which extend can one consider the high extinction values as reliable in view of the assumption made on the particle size distribution ? (cf. remark on L. 10-18 p.25795)
We agree that the figure is remarkable, thank you for the comment! The large values of extinction are certainly not to be considered quantitatively reliable as you suggest and for that reason we have included the discussion on both pages 25800 and 25801 warning users of this issue.

• L. 10-11 p.25800: This sentence should be attached to the previous paragraph discussing the same case of 2005. Starting a new paragraph with it before talking about general considerations on validation without link with this sentence is a bit confusing.
Corrected as suggested.

• L. 5-7 p.25802: “The authors should add : “throughout the bulk of the stratospheric layer in the conditions of low volcanic load encountered in 2002-2005, …”.
Corrected as suggested.

• Residual spelling errors of badly constructed sentences are found, see l.25 p.25787 and l.4 p.25793.
Corrected, thank you.