

Interactive comment on “Retrieval of aerosol optical depth and vertical distribution using O₂ A- and B-band SCIAMACHY observations over Kanpur: a case study” by S. Sanghavi et al.

Anonymous Referee #2

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General comments

There are not many papers about aerosol vertical profile retrievals from oxygen absorption bands, so the topic of this paper is welcome and fits in AMT. But the paper has serious shortcomings, which will be described below.

Although the paper has the relevant Section titles, the paper is not written in a structured way. Essential information that belongs to one section appears in other sections.

This holds for example for the algorithm description and its uncertainties. Referencing is too limited. There are more relevant papers about cloud height retrieval from the oxygen A-band of SCIAMACHY. The introduction of the topic of the paper in Section 1 is rather weak. No mention is made of lidars. There is no proper introduction of the SCIAMACHY data; the limited information given about SCIAMACHY is scattered throughout the paper.

On the positive side: the discussion of the behaviour of the Jacobian matrix in Sect. 3 and the sensitivity studies in Sect. 4 are interesting and may be useful for others, when extended with the relevant additions, mentioned below.

At various places in the paper it is said that cloud filtering is not applied. So the retrieved aerosol properties (optical thickness, vertical profile) could very well belong to clouds. This means a drastic change of the scope of the paper; the reader should be informed about this in the title, abstract, introduction, and conclusions.

The main problem of the paper is that the real retrievals using SCIAMACHY data are not convincing at all. The retrieved AOD does not agree well with AERONET data. Furthermore, there is no validation of aerosol height, so it can not be concluded that the retrieved aerosol vertical profile has any merit. What has to added is a good error analysis for the retrievals of real SCIAMACHY data. It is not clear which algorithm assumptions are most contributing to the poor end result. Is it the missing cloud detection, the surface albedo or the aerosol microphysical parameters? Please discuss in Section 6 whether the problem is due to the retrieval method or due to the SCIAMACHY data, or due to another reason.

The paper needs to be rewritten according to the specific comments before it can be accepted.

Specific comments

Abstract:

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- More specific and quantitative information about the results of the paper is needed in the abstract. Now lines 1 – 10 of the abstract are largely an introductory text which belongs to Section 1. - Line 18 mentions “good agreement” but this is not a true summary of the results shown in the paper, especially those of Figure 11. It should be mentioned that the SCIAMACHY retrievals of AOT do not agree well with AERONET data; there is a poor agreement in Figure 11. There is no validation of the aerosol height. The problems in the real retrievals should be mentioned.

Section 1, Introduction:

Section 1 has several shortcomings and should be rewritten: - A discussion of lidars to measure aerosol height is missing here. The last paragraph of Section 6 in fact belongs in the Introduction. - The difficulty of aerosol height retrievals from passive satellite measurements should also be discussed. - This section is a mixture of previous work and own work. Please separate the two, and do not summarize your own results already in the introduction (e.g. on p. 6781, l. 21-22). - This section should introduce the topic by properly referencing to earlier work, and by mentioning the goal of the paper and its unique contribution. - What is new in the current paper as compared to Sanghavi et al., Appl. Opt., 2010 ? Please briefly summarize this paper, since it is an essential part of the algorithm in Sect. 2 and later sections. - Some sections of the paper (4 and 5) are mentioned without introducing the structure of the paper. Please add a last paragraph to the introduction with the structure of the paper. - Last paragraph: Apparently, clouds are not discriminated from aerosols. This is not so much of a problem for cloud retrievals, since clouds have much larger optical thicknesses than aerosols, but it is a severe problem for aerosol retrievals. What is the value of the retrieved AOT? It seems that non-discriminating between clouds and aerosols undermines the scope of the paper, which is aerosols.

p. 6780, l. 22: large eruptions of the Eyja volcano took place in April and May 2010.

p. 6781, l. 7ff: there are more relevant references on the retrieval of cloud height from

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the O2 A-band from SCIAMACHY, like the FRESCO method papers: Koelemeijer et al., JGR, 2011; Wang et al., ACP, 2008.

p. 6781, l. 10ff: please refer to the paper by Solomon et al. (JGR, 2003) on the information content of the different O2 absorption bands.

p. 6781, l. 18-19: which parametrized form?

Section 2:

p. 6782, l. 14: The Aerosols and UV part of the 2002 OMI ATBD, referenced here as Stammes and Noordhoek (2002) is published as a refereed paper, namely Torres, Tanskanen, Veihelmann et al., JGR, 2007. This is the preferred reference (also later in the paper).

p. 6783, l. 21ff: which aerosol model was assumed? What is the Angstrom parameter of this aerosol type? Does 10 % allow sufficient flexibility? Please include these points in the error analysis to be discussed in Sect. 6.

p. 6784, l. 3: what are these measurement data? The SCIAMACHY data (mainly its spectral and spatial properties) are not properly introduced in the paper. This should be done in Sect. 1 or anyway before the retrieval method is discussed.

p. 6784, l. 20-21: I do not see how Eq. (6) is testing that the difference between successive iterations is an order of magnitude less than the estimated error.

p. 6784, Eq. (7) and l. 23: is this state vector length n the same as the N in Section 3, l. 3 on p. 6785? Please use one symbol for one variable.

Section 3:

p. 6785, l. 6: please number this equation for later reference

p. 6785, l. 12: ..., respectively ρ_A , and ρ_B , ...

p. 6785, l. 13-14: continental surface > land surface (this also occurs later on)

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p. 6785, l. 26ff: please explain the 'depth of the radiance' in terms of the quantity $\ln R/dx$ which is shown in Figure 1 and successive figures. Define this R.

p. 6786, l. 6: optical thickness > aerosol optical thickness

p. 6786, l. 6: ... is a direct consequence ...: this should also #depend on the height of the aerosol layer

p. 6786, l. 14: 'Since the bulk of the Rayleigh scatterers resides close to the surface': this is too simplistic, because the Rayleigh scale height is about 8 km; is that close to the surface?

l. 15: net > mean

l. 20: the log normal tails: of the vertical distribution function

l. 22: sensitivity > vertical sensitivity

l. 23: depths > geometric depths

l. 27-28: optical thickness > aerosol optical thickness (also in the remainder of this section)

p. 6787, l. 15: also evident > are also evident

p. 6787, l. 15: What is new in Fig. 4 as compared to the convolved curves in Figs. 1-3? Is there a change of slit function, since the curves in Fig. 4 look less smooth than the convolved curves in Figs. 1-3?

Section 4:

p. 6788, l. 3: 0.5-1.0 > 0.5-1.0 km

p. 6788, l. 3: is the assumed error of 1 % in SCIAMACHY data not much too small, given the fact that SCIAMACHY has quite some radiometric calibration uncertainty? Please give a reference. Is this the error in the radiance or in the reflectance? Is it a random error or a systematic error?

p. 6788, l. 6-7: remove 'between the x- and y-axes'.

l. 7: what are vertical parameters?

l. 9: 'truth lines' > true values

l. 10: negligible > small

l. 14: vertical profile > the vertical profile

l. 17: 'the difference in surface albedos . . . adds to the information content of the latter measurement': can you please explain this? What is the physical reason?

p. 6789, l. 3-4: truth line > true value

p. 6789, l. 5: 'we can assume very robust retrievability. . .': Please remove 'very'.

l. 6: 'moderately good': this is too optimistic, please replace by: 'only limited information on σ_p '.

l. 12-15: An important question rises here: is in this synthetic retrieval the surface albedo assumed to be known perfectly? But in reality this is never the case. How do you deal with the unknown surface albedo in retrieval of real data? Wouldn't it be better to simultaneously retrieve the surface albedo together with the aerosol parameters?

In the sensitivity studies of Section 4 no mention is made of the dependence of the results on the following parameters: 1. assumed error in the satellite data 2. neglect of error in surface albedo (see point above). 3. assumed aerosol microphysical parameters, like phase function and single scattering albedo 4. geometry of satellite and sun. These parameters seem important and belong to the sensitivity studies of the retrieval algorithm.

Section 5:

p. 6790, l. 5: There could be a real seasonal cycle in the relative contrast of the surface, so the value to be chosen may depend on the season.

l. 10-16: This paragraph seems to suddenly change the scope of the paper. If the paper is aiming to determining atmospheric scatterers, whether they are clouds or aerosols, this should be said clearly in the title, abstract, introduction and conclusion of the paper.

In fact, the part of Section 5 from line 10 on p. 6790 to line 19 on p. 6791, discussing fundamental issues of the retrieval method, does not belong here. It belongs to Sect. 4, or even earlier in the paper. After the SCIAMACHY results are presented this earlier fundamental discussion can be included in the discussion of the results.

l. 19: ‘such oscillations’: probably more well-separated wavelengths are needed to prevent such oscillations (cf. the MODIS aerosol algorithm).

p. 6792, l. 3: ‘scene inhomogeneities’: this is not a probable reason of the bad agreement, since aerosol fields are much more homogeneous than clouds. More probable is that the assumed aerosol microphysical properties are not correct in reality.

l. 27: what do you mean with ‘later CALIPSO measurements’ ? Did you make a climatology for later years and compare this to your SCIAMACHY retrievals? Please quantify this statement. Please perform the retro-active comparison to show that there is merit in the vertical profile information from SCIAMACHY.

p. 6793, l. 11: see > show

l. 14ff: This is an honest discussion of the problems of the retrieval algorithm. This should also be reflected in the Abstract and Conclusions.

Section 6, Conclusions:

p. 6794, l. 3: 1 nm? but SCIAMACHY has 0.3 nm spectral resolution.

l. 6-7: ‘this is true for most ...’: but this is not true for ocean, which is the largest terrestrial surface.

l. 14: ‘good agreement’: this is not correct. Please indicate the problems of the retrieval

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algorithm, and give an honest report of the results found in Section 5.

l. 15: no comparison of the vertical distribution was shown with CALIPSO, also not retro-actively. Please remove sentence.

Please indicate what the uniqueness is of this work. Apparently AOT from real SCIA-MACHY data shows large discrepancies. Perhaps the aerosol altitude is a unique contribution?

References: - Martonchik: place of dissertation? - Rodgers: publisher?

Figures and captions:

Figures 1 - 3: Please give the fixed aerosol parameters of each subplot in the legend or caption. Please specify the sun-satellite geometry. Please specify the aerosol type.

Figures 5-7: Please specify the geometry. Is σ also given in km? Please state that the black line is the 1:1 line.

Caption Fig. 9: which measurement data were used?

Caption Fig. 11: what is the value of the correlation coefficient of the bottom panel?

Some textual comments:

Abstract: trace-gases > trace gases

Introduction: Ejya ... > Eyja ...

p. 6780, l. 21: punctuated by? please find another word, like: stressed, emphasized, underlined

p. 6782, l. 1 (and in many other places): scenarios > scenes (scenario is a model situation; a scene is a real situation)

p. 6792: time progression > time

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