Dear Reviewer #3,
The authors are very grateful to the reviewer for their valuable comments and suggestions, which have greatly improved the content and the presentation of our paper.

First of all, the first author apologizes to the reviewer for the difficulty that must have been caused by the lack of structure of the previous manuscript. I have posted an author comment entitled "General modifications" which presents a summary of the main changes, both structural and content-related, made to the new manuscript.

The retrieval of aerosol height information from passive satellite measurements in the oxygen absorption bands is an interesting topic and the authors have chosen a promising approach in exploiting both O2A and O2B in parallel. The goal of this work is at the same time very ambitious, which should be taken into account in the judgement of the results.

In the following we answer all reviewer comments on a point-by-point basis:

The existence of information about height and width of the vertical aerosol distribution in both oxygen absorption bands is demonstrated. However, the paper suffers from several shortcomings, most importantly the lack of a comprehensive discussion of error sources such as e.g. the aerosol model or surface pressure, a missing validation of the retrieved aerosol height parameters and little explanation for the relatively poor quality of the derived aerosol optical depth values.
We admit that systematic error sources were not properly discussed in the paper. Thus, a new section on Information content and error analysis (Section 3.2) has been added to address these concerns.
Specifically:

- surface pressure: not discussed in the paper but can be assumed known with 1-2hPa accuracy from global meterological forecast models (e.g. Salstein et al. 2008)
- aerosol model: now discussed in Section 3.2
- validation of retrieved height: not attempted with real data, this analysis is confined to the synthetic study, showing that it works given a perfect measurement.
- AOD discussion: Many systematic errors due to SCIAMACHY's large footprint, radiometric calibration and/or O2 A-band spectroscopy (e.g. neglect of line-mixing) can all contribute to the error but it remains difficult to unequivocally contribute it to a single source of error.
- Furthermore, we had missed pointing out in our previous manuscript that the correlation coefficient between our non-monsoon monthly mean AOTs with AERONET is as high as 0.92, inspite of the appearance of a poor correlation due to the absence of measurements at the same time.

The title, abstract and conclusions of the paper are therefore somewhat misleading, since the impression of a successful retrieval of vertical aerosol distribution from SCIAMACHY data is given, which is actually not shown.
The title has been change to reflect that we are not interested in aerosols alone: "Retrieval of the optical depth and vertical distribution of particulate scatterers in the atmosphere using O2 A- and B-band SCIAMACHY observations over Kanpur: a case study."
Further, we have justified our vertical distribution retrievals on the basis of our error analyses and sensitivity studies, together with the fact that the residence of most aerosols over Kanpur between 0-5 km as suggested by our results can be confirmed by comparison with simultaneous twice-daily back-trajectory data (Pickering et al. (2001), Schoeberl et al. (1995)) available from the AERONET website. Any further claims have been removed.

The paper should only be published after a thorough revision. Specific comments: Section 2: You have chosen a lognormal profile but you don't provide a justification for this other than stating that you assume it is close to reality on average. Since the paper deals with remote sensing of the aerosol vertical distribution, a more detailed discussion of this question should be part of the manuscript. By looking at LIDAR data, such as provided by CALIPSO, one could easily assess whether other profiles are more suited (e.g. bi- modal distributions). I guess SCIAMACHY does not provide enough information to aim at more complex profiles, but then this should be clearly stated in the text.
Text has been modified to reflect the fact that a retrieval of more complex vertical profiles is certainly beyond the capability of the SCIAMACHY instrument.

p. 6784, l. 22: The test for convergence as written in the text seems reasonable to me. But it is not the same as in formula (6).
The equation has been modified to be compatible with the text, was a typo before.

Section 3:
This section provides quite some insight into the differing sensitivities in O2A and O2B, justifying the use of both bands in parallel. However, these sensitivities to your state vector elements have to be compared to the sensitivities to all possible error sources. What if an uncertainty of e.g. surface pressure causes a similar change of the signal
as one of your state vector elements? What if your aerosol model assumption fails to describe reality? All this needs to be considered (in Section 4 as well, see below).
We have added an in-depth Information content and error analysis (Section 3.2) to address these points. While we have examined the effect of error due to the assumption of the wrong microphysical parameters (Section 3.2.1), the wrong surface albedos (Section 3.2.2) and due to measurement bias (Section 3.2.3), we have not delved in particular into error due to surface pressure, since we include altitude (even though the terrain around Kanpur is fairly flat) and surface pressure information (obtained from ECMWF for the time and location of the SCIAMACHY measurement) in our radiative transfer calculations. As mentioned before, errors in surface pressure are expected to be only on the 1-2hPa range.

Section 4:
You cannot test the robustness of the retrieval concept by just looking at the influence of instrumental noise, this can only be a first check. If your algorithm succeeds to retrieve the input within reasonable errors, you should assess all the other error sources (forward model parameters).
It would thus be important to test the influence of surface pressure, aerosol optical properties (single scattering albedo, Angstrom parameter, scattering phase function), temperature profile, surface reflectance, etc. Do the uncertainties of these parameters erase the sensitivity of the measurements to the aerosol vertical distribution?
As mentioned above, most of these error sources have been discussed in detail in the new Section 3.2.

In figures 5 -7, all three parameters are plotted at once, leaving the y-axes with three different meanings and a different scale for each plot (difficult to compare the deviations from the "truth lines"). Is there any chance to modify these plots?
The authors think it is difficult to fit in more information into one figure, especially because attempting to divide one figure into three different tiles, each dedicated to only one parameter causes even less space to be available to each.

If I understand it correctly, your study shown in figures 5 -7 is based on a single retrieval run for each case. Instead it should be based on a sufficiently large number of cases to illustrate the average error resulting from each error source. The success of a single retrieval run from a noisy measurement is rather random.
If I understand the comment correctly, the reviewer wishes the use of different a priori assumption within the expected range to arrive at a statistical measure of the retrieved parameter. While this approach may be valid, it is very time-consuming. Since the suggested results are more easily available (without any loss of rigor) using the statistical tools, e.g. the a posteriori covariance matrix, described in Section 3.2, we have used these tools for an error analysis that is complementary to the sensitivity study.

Section 5: I won't go into detail since I would recommend to revise the whole section. The retrieved aerosol height parameters are not validated and the retrieved aerosol optical depth values are not very reliable. The influence of clouds on the shown retrievals remains somewhat unclear to me. If the aim of the work is to derive aerosol information, it is crucial to filter out clouds, isn't it?
As the reviewer rightly points out, large modifications were required in the general structure of the paper and this has been done to the best of our ability. The height retrievals have indeed not been validated directly for lack of simultaneous measurements, but we have reinforced our concept by means of our sensitivity studies and our error analysis.
As for cloud-screening, our choice of Kanpur for our case-study partly alleviates this problem, mainly during non-monsoon months due to the generally low likelihood of cloudy skies in the region. Application of our algorithm to other parts of the globe may require very reliable cloud-screening if only aerosols are targeted, but by changing our title to focus on atmospheric particulate matter and not only aerosol, we have shifted the scope of the paper.

Section 6: p.6794, l. 2: 1nm?
The instrumental FWHM is 0.43nm for the B-band and 0.44nm for the A-band. We have summarized this as a spectral resolution of approx. 0.4 nm. The 1nm must have been a typo, our apologies for the confusion.

l. 11: "good retrievability": This is not shown in the sensitivity studies, only the influence of noise is shown.
This statement in the Conclusions has been replaced by: “an ability to retrieve a lognormal approximation of the vertical profile of the aerosol column for hazy scenes.”
It is not fair to state you found a "good agreement ... with CALIPSO measurements...". The fact that aerosols were found between 0 and 5 km by CALIPSO in later years, is not at all a validation / verification of your retrieval.
All references to CALIPSO for validation have been removed since we have not performed an in-depth validation analysis.