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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Utilization of O_4 slant column density to derive aerosol layer height from a spaceborne UV-visible hyperspectral sensor: sensitivity and case study" *by* S. S. Park et al.

Anonymous Referee #1

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The manuscript describes an ambitious attempt to determine an aerosol effective height from a combination of OMI spectra and MODIS aerosol retrieval. The method, if it were to be improved to operational maturity, is of high interest to remote sensing and modeling communities in search of observational data on aerosol profiles.

However, I see important obstacles on the road to practical application of this method, several of which are not or only barely addressed in the manuscript. In particular, these regard the choice of aerosol parameters (size, shape), possible mismatches between OMI and MODIS data, and cloud contamination of OMI data.

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In addition, the method is currently not described in sufficient detail; e.g., it remains unclear why MODIS AOD and type are used instead of OMI data, or why the DOAS fit of O4 is explicitly included in the AEH retrieval algorithm (when a look-up-table of air mass factors would appear to be sufficient: O4 has a broad absorption spectrum and fitting the SCD is relatively straightforward). As I noted in my review of the initial draft, there are too little references and comparisons to previous work (similar sensitivity studies have been performed by Veihelmann et al., 2007 and Wagner et al., 2010). The literature is cited in the introduction, but a summary of the previous findings and the relation to the current findings is missing from the manuscript.

Lastly, and as mentioned in my review of the initial version, the presented case study does not provide convincing evidence that the algorithm works. First of all, only a single case is presented; second, CALIOP backscatter profiles are shown of which only a small part is detected by OMI (at 35-40 N, 122.5-123 E) — and these values do not agree very well (CALIOP doesn't exceed 1.7 km, whereas the retrieved AEH appears to vary from 1-5 km in this region). The comparison would have been more meaningful if AOD and aerosol type from CALIOP had been included, and a longer orbital segment had been selected. Third, as mentioned in the previous review, the comparison with ground-based lidar is not at all appropriate for reasons of collocation mismatch (the station is over land; the OMI measurement >100 km away and over ocean).

In summary, I recommend that this paper be thoroughly revised before being re-submitted. The most important revisions (addressed above) include:

- More references and comparisons to literature

- Detailed, step-by-step description of the AEH algorithm in a separate section

- Assessment of additional error sources (wrong aerosol model assumptions; cloud contamination)

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- Addition of more, and more appropriate case studies

Some suggestions for improvement of the paper are given below, but because in my opinion the manuscript requires extensive re-writing, more suggestions would follow in the next round of review.

Other comments

P.7934, ll. 11-14: "Overall, the error (...) vertical distribution type." Mention that the cited error values apply to the base case (SZA=30, VZA=30; I was unable to find the reference AOD and AEH). More importantly, the overall error here does not include the uncertainty due to vertical distribution. Although this is mentioned in the quoted sentence, it does not appear to be fair to leave out this major error contribution — particularly because its magnitude was explicitly determined.

P. 7935, ll.15ff: "The information on the aerosol height is important (...)" Also for the improvement of trace gas retrievals (better air mass factor calculation) the aerosol profile is of importance.

P. 7936, l.8: "(Wagner et al., 2010)" This reference is not appropriate, better would be, e.g: Wagner, et al., 2008, doi: 10.1088/1464-4258/10/10/1040192008), but there are many others, too.

P. 7936, ll.8-28: "Recently, several studies (...) aerosol transport cases." The results from the cited studies need to be summarized and discussed in more detail, probably in a separate section. The findings from those previous studies should be used as starting points for your own studies, and you should explain what your own studies add to the existing body of knowledge.

P.7937, l.10: The term SCD is not explained. I think some DOAS theory, or at least a discussion of radiative transport, is needed in this section. I strongly encourage the use of AMFs instead of SCDs, because the numbers are more intuitive. Apart from that, since the O4 VCD is well known, it might as well be removed (i.e., divided out) for

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simplicity.

P. 7940, l.19: ”the noise level” Where does the (relatively large) noise in the simulations come from?

P. 7940, l.22- P.7941, l.19: ”Figure 2 shows the comparison (...) to retrieve aerosol height.” This section raises some issues, e.g.: how do the data look for AOD=0? An AOD of 0.15 appears rather high, although this might account for occasional cloud contamination of OMI data. The correlation is good, but not perfect, and it would be interesting to know if there are systematic deviations (e.g., for certain solar/viewing geometries). I would expect some deviations, particularly at larger viewing angles, simply due to the coarse resolution of the LUT (at the swath edges SCD probably depends strongly on viewing angle). The fact that the O4 cross section needs scaling for a better agreement of results is attributable to the difference in cross-sections used by the authors on the one hand, and the OMICLDO2 retrieval team on the other hand.

P. 7942, l.21: I would rename this section to, e.g., ”Sensitivity of O4 SCDs at various wavelengths to AEH”, and then add another section, e.g. ”Sensitivity of 477nm O4 SCDs to various aerosol parameters” at page 7944, line 4 to improve readability. This is the section where a comparison with previous sensitivity studies should be presented.

P. 7942, ll.4-6: ”However, the absorbing aerosols in low AEH cases (...) and 380 nm.” What do you mean by ”fluctuated” ? And what is the cause of the large fitting error?

P. 7942, ll.6-8: ”For this reason (...) in the AEH range of 2.0 to 4.0 km.” This is a quite clear definition of -dO4/dZ, but in the next lines, you often use a different definition, e.g. in lines 9-10 on the same page. This appears to be the maximum -dO4/dZ for a certain altitude, which is not in agreement with the definition cited above and confuses the reader.

P. 7942-7943: The results in this section should be presented in a more clear and concise way. In fact, they can be summarized (somewhat crudely) by simply saying

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that O4 absorption features at wavelengths other than 477 are not suitable for AEH retrieval because the sensitivity of the O4 SCD to AEH is smaller than or comparable to the fitting error.

P. 7944, ll.13-14: "Torres et al., (...) due to the cloud contamination." This is not very relevant to the current study, as no OMI aerosol data are used.

P. 7944, ll.14-15: "SSA varies widely as the categorizing aerosol types." Do you mean: SSA varies widely for different aerosol types?

P. 7945, Sect. 3.2: Discuss uncertainties arising from errors in assumed particle size and shape (phase functions). Also missing is the uncertainty due to mis-classification of aerosols (e.g., COPO as WASO). Cases with more than one layer of aerosols also deserve attention here.

P. 7947, ll.6ff: Large parts of this section, particularly the description of the OMI instrument and the description of the AEH derivation algorithm, should be put into a separate Methods section. The section should also contain an explanation of how MODIS data are selected and integrated into the AEH algorithm.

P. 7948, ll.12-13: "From CALIOP observation, ... for most observed regions." What about the small region that is collocated with the OMI/MODIS measurement?

P. 7948, ll.25-26: "the investigated algorithm quantitatively estimates the AEH over East Asia." This statement is rather too bold (as mentioned previously). You have not proven this with the one case study presented in the manuscript.

P. 7949, Sect. 5: Add the error from profile shape assumptions to the total error; this would appear to be more fair.

PP. 7963-7964: Why not merge Tables 6 and 7?

Fig. 3 : the lower panel is wrong; it shows results for 360 nm instead of 340 nm

Figs. 3-6: Add the Rayleigh AMF (more informative than the geometrical AMF); it is

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given in Fig. 7 for 477 nm (at AOD=0).

Fig. 9a: What is the cause of the red color?

Fig. 9e: Add the CALIOP ground track.

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