Interactive comment on "Utilisation of O4 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.

General comments

The authors did substantial modifications on their new version of the manuscript. They took account most of the comments highlighted during the review process. I appreciate to see more details explicitly described in the different sections, like the methodology of the error analysis or a higher number of study cases in the last chapter. Moreover, the section about the sensitivity of the AEH retrieval to the different O4 spectral bands is more concise and goes straightforward to the key message: the importance of the 477 nm band, compared to the other ones. The error analysis performed here look now more realistic. In particular, as expected, the analysis surface albedo and AODs (among other parameters) show to have large impact on the AEH retrieval. The results presented at the end on real OMI observations, focused over ocean, show very large differences with the CALIPSO dataset. However, the authors make links with the error analysis and insist on the different parameters to improve (in particular the surface albedo, and the aerosol properties) prior to the retrievals. In spite of these differences, this work will be useful for the community who works on this topic.

I still have a few remarks, mostly minor. I would appreciate the authors to address them, before the final publication, as I believe they will help the readers.

Thank you for your encouraging and helpful comments for our manuscript. We revised the manuscript by considering reviewer's comments. Please see below about our answers for your detailed comments.

Specific comments

1) Explanations about the LUT in Section 2.2

The authors compare their own retrieved O4 SCDs to the OMI NASA products. For that, it is mentioned that the WinDOAS software is used, and then a Look-Up Table as described in Table 2. However, the link between these 2 elements is not completely clear.

I believe that a few lines should be added before the beginning of the 3rd paragraph. I guess that a number of simulations were performed with VLIDORT for different SZA, VZA values. And then a DOAS fit was performed to deduce O4 SCD with WinDOAS. And finally, a LUT could be generated. Am I correct? Please explain it.

Ans) To explain the link the O_4 SCDs between a Look-Up Table and OMI NASA product,

we added the sentence on lines 252-253 in the revised manuscript as:
"Furthermore, the DOAS analysis for LUT calculation can be used to compare the O_4 SCD from OMCLDO2."
To supplement the explanation of the LUT development, we also added the sentence on lines 256-257 in the revised manuscript as:
"The LUT of O_4 SCD is estimated by the DOAS analysis using simulated radiance from VLIDORT with various geometries as shown in Table 2."

2) Surface albedo value of 0.1 in Section 4
I am not sure, but this value may be a little high for OMI observations over ocean. In my view, ocean surface is in general darker than land surface (although it can be highly variable depending on ocean conditions, as mentioned by the authors). Depending on seasons, OMI surface albedo values over land are usually (but not always) below than 0.1 in the visible. Could the authors explain or add 1 or 2 references justifying this value?
Ans) On lines 281-283 in the revised manuscript, we mentioned why this study assumed high surface albedo. In addition, we added the sentence to supplement the reason and reference of surface albedo value of 0.1 on lines 281-283 in the revised manuscript as:
"Although assumed surface albedo is higher than minimum LER from Kleipool et al. (2008), the surface albedo of 0.10 is realistic value for ocean surface albedo at mid-latitude (e.g., Payne, 1972)."

Reference :
Payne, R. E., 1972: Albedo of the Sea Surface, J. Atmos. Sci., 29, 959-970.

Furthermore, we also revised on lines 286-288 in the revised manuscript as:
"Although the comparison result is not perfect, the calculation by the VLIDORT simulates the satellite observation and can be used for sensitivity tests and case studies to retrieve aerosol height."

In Section 4 (lines 562-563), we also added a sentence, "If the surface albedo is changed but known, the qualitative conclusion here is not affected."

3) Sensitivity of AEH accuracy to AOD (Section 3.2.1)
This section is very interesting, and confirms that the feasibility to retrieve the aerosols altitude, from passive sensors, depends on the aerosol amounts. Later on, in the

manuscript, it is also mentioned that angles (SZA, VZA) have a significant role. I believe that overall the continuum reflectance (which directly depends on these 3 parameters) drives the AEH accuracy (as the O4 shielding effect, through the SCD, is constrained by the continuum reflectance magnitude). In case of low continuum reflectance, aerosols (and even clouds) have limited effects on the slant column It is somewhat mentioned in [Acarreta et al., 2004]. You could refer to it. Moreover, such a sensitivity was found in [Sanders et al., 2015] although a different spectral band was used. This section and your plots look consistent with the findings of [Chimot et al., 2015], where different approaches were employed for analysing the interplay between the OMI cloud retrievals, through the O4 spectral band, and the presence of aerosols. Could you please verify and confirm this for consistency?

Ans) To verify and confirm the geometry dependence of accuracy for AEH and cloud information, we added the reference and sentences on lines 376-381 in the revised manuscript as:

"From previous studies, the error for cloud height information depends on the observation geometries due to changing average optical path length (Accarreta et al., 2004; Chimot et al., 2015). Moreover, the retrieval error sensitivity for observation geometries is also found in aerosol height estimation by O_2-A band (Sanders et al., 2015). Similar to these previous studies, the AEH error becomes larger for short light paths and smaller for long paths."

4) Review some typos / sentences

I would recommend, to perform a final check of the manuscript and to reformulate some sentences, where necessary. There are sometimes some incomplete sentences, or some formulations are not clear.

Below are given some examples, but this list is, I think, not exhaustive:

Page 6, Line 125: "This new algorithm is applied to the radiance data": I think here the author should say it was applied to the O2-O2 SCD, available in the OMI NASA product.

Ans) We added "O_4 SCD" on line 125 in the revised manuscript.

Page 6, Line 135: "atmospheric molecules and aerosolS" (final s to be added to "aerosol")

Ans) We revised to your suggestion on line 134 in the revised manuscript.

Page 14, Line 307: "On the other hand, the sensitivity of the O4I at 477 nm has significance to estimate AEH" => "is a significant variable to estimate AEH"

Ans) We revised to your suggestion on lines 311-312 in the revised manuscript.

Page 14, Line 328: "materials" => properties
Ans) We revised to your suggestion on line 332 in the revised manuscript.

Page 15, lines 345-346: I do not understand the statement "from OMI … respectively". Please reformulate.
Ans) We revised on lines 349-353 in the revised manuscript as:
"From OMI standard products, the expected error of the AOD over ocean is the larger of 0.1 or 30% for absorbing aerosol, and the larger of 0.1 or 20% for non-absorbing aerosol (Torres et al., 1998, 2002). For this reason, the uncertainty of AOD is assumed to be 0.1 in this study, although uncertainty of AOD would be larger than the assumed value for large AOD."

Page 15m Line 348: please justify the AOD uncertainty of 0.1 (reference?).
Ans) We added the references on lines 349-351 in the revised manuscript.
""From OMI standard products, the expected error of the AOD over ocean is the larger of 0.1 or 30% for absorbing aerosol, and the larger of 0.1 or 20% for non-absorbing aerosol (Torres et al., 1998, 2002)."

Page 16, Line 354: "for small AOD at low AEH, which has small shielding effect" => since here aerosols are at very low altitude (close to the surface) and O4 SCD increases, I think the term enhancement would be more appropriate than shielding. Indeed, in that case, aerosols have a similar effect than albedo, as they increase the sensitivity of most of the O4 column. They do not shield here the O4 column.
Ans) We revised on lines 358-359 in the revised manuscript as:
"which has small shielding effect with large enhancement effect"
In addition, we revised that all of "albedo effect" is changed to "enhancement effect" in the revised manuscript to avoid confusion for "surface albedo".

Page 16, Line 360: "decreases as the altitude" => do you mean aerosol altitude?
Ans) This phrase does not mean "aerosol altitude" but mean "atmospheric altitude". We revised "atmospheric altitude" on line 366 in the revised manuscript.

Page 17, Line 377: "scattering aerosolS" (s added)
Ans) We revised to your suggestion on line 386 in the revised manuscript.

Page 17, Line 378-379: “It effectively brings albedo effect dominant for aerosol layer”. Please reformulate this, and use enhancement effect, as opposite to shielding effect.

Ans) We revised on lines 387-388 in the revised manuscript as:

“aerosol layer effectively brings enhancement effect.”

Page 17, line 382: “which is corresponds to” => which corresponds to?

Ans) We revised to your suggestion on line 391 in the revised manuscript.

Page 18, Line 411: “noticeable effects ON THE phase function” (on the added)

Ans) We revised to your suggestion on line 421 in the revised manuscript.

Page 18, Line 418: “which setS”

Ans) We revised “which corresponds” on lines 428-429 in the revised manuscript.

Page 19, Line 434: “aerosol layer shut off” => aerosol layer attenuates

Ans) We revised to your suggestion on line 444 in the revised manuscript.

Page 19, Line 438: ”by the albedo effect to the O4I is larger for the absorbing aerosol” => I do not understand the last part of this sentence. Please reformulate.

Ans) We revised on lines 446-448 in the revised manuscript as:

“Furthermore, it is found that the difference of O4I due to surface albedo change is larger for the non-absorbing aerosol than the absorbing aerosol, because absorbing aerosol attenuates the reflected radiance more than the non-absorbing aerosol.”

Page 19, Line 446: “”AEH increaseS and AOD increaseS”

Ans) We revised to ”AEH increases and AOD decreases” on line 456 in the revised manuscript.

Page 20, Lines 450-452: “However…the reference case”: Please check English and formulation

Ans) We revised on lines 460-461 in the revised manuscript as:

“However, the AEH error sharply decreases as AOD increases and AEH decreases, when aerosol signal becomes dominant.”

Page 20, Line 457: “less” => lower or smaller?

Ans) We revised “lower” on line 466 in the revised manuscript.

Page 23, Line 521: “a half and a quarter” => 50% and 25%

Ans) We revised on lines 530-531 in the revised manuscript as:

"Note that the result of error analysis explains about 50% for SSA and 25% for size parameter in calculating the total error budget."

Page 24, Line 556: What is FMF? I did not find this term earlier in the manuscript. Further, please define the CALIOP AEH. How did you compute it? Or was it available in the CALIOP product?

Ans) We defined FMF in line 50. After defining "FMF", we used the "FMF" in the revised manuscript.

In addition, we estimated from extinction coefficient at 532 nm for CALIOP AEH. Definition of AEH for CALIOP is same to the sensitivity test. To explain the CALIOP AEH, we revised on lines 593-594 in the revised manuscript as:

"The AEH from CALIOP is estimated by the data from vertical profile of aerosol extinction coefficient at 532 nm."

Page 25, Line 579: "on the dateS"

Ans) We revised to your suggestion on line 591 in the revised manuscript.

Sorry for the mistakes that should have been corrected carefully. Thank you very much for your suggestions and help.