

## ***Interactive comment on “The Role of Cloud Contamination, Aerosol Layer Height and Aerosol Model in the Assessment of the OMI near-UV Retrievals over the Ocean” by Santiago Gassó and Omar Torres***

**Anonymous Referee #1**

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The manuscript provides an assessment of the OMI OMAERUV algorithm over the ocean. This algorithm retrieves the AOD and SSA for cases with significant AAI, using assumptions on the aerosol layer height, the aerosol model (refractive index, size distribution and shape) and the surface albedo. By comparing the retrievals with AERONET, Modis and Calipso data, the impact of remaining clouds, of aerosol layer height and aerosol model are quantified using case studies. The main outcomes of the study a recommendation to the user to filter the data based on the number of successful neighboring retrievals, and a recommendation to implement a non-spherical dust model in the next version of the OMAERUV algorithm. Overall I find this a useful contribution to

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the literature. I like the work because it goes beyond the often presented comparisons between data product and tries to explain the causes for differences. My main criticism would be that the role of assumptions on the BRDF of the oceans are not discussed. I would suggest at least explaining what is assumed in the algorithm, and how this can effect the results. Furthermore, more information should be added in different places in the manuscript, see specific comments below.

Line 21: “within the expected levels of uncertainty for the OMI AOD retrievals”: this shall be quantified.

Line 46 “singled” I think the authors mean “signaled” ?

Line 84-85 “OMI retrievals of SSA are the only global and daily operational retrievals of among all Earth viewing platforms.” I find this a very strange sentence, please revise. Note that the algorithm only provides a retrieval when the AAI is over a threshold, therefore it is clearly not a global product.

Line 93: “chiefly” should be replaced by “mainly”

Acronyms should be written out the first time they are used. For example AERONET (line99), MODIS (line 126), CALIOP (line 126), TOMS (line 145), GOME (line 145), SCIAMACHY (line 146).

Line 105: I think the authors mean MODIS instead of AERONET?

Line 107-108: “it is assumed that a realistic and accurate AOD retrieval must have an associated realistic SSA as long as a realistic assumption on aerosol layer height has been made.” I strongly doubt this assumption. From principal component analyses it is found that the dominant information in the wavelength pair is related to the AOD. In addition, the spectral slope of the radiance depends on the SSA, the aerosol vertical distribution and the aerosol microphysics (size distribution and refractive index). Given the fact that the system is very underdetermined, there is no guarantee that an accurate AOD will result in an accurate SSA. The opposite is true: an accurate AOD

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is a prerequisite for an accurate SSA, given the dominance of the AOD in the information content. Given the above, the authors should revise the statement, or delete it altogether.

Line 160: “steady” should be replaced by “stable”

Section 2.1.1: Given that the OMI pixel increases strongly towards the edge of the swath, are all the across track pixels used in this study?

Section 2.1.2: I looked into the Torres [2013] paper to find a definition of the aerosol types (size distributions and index of refraction). However, Torres [2013] refers to two other papers. Because I think it is important for this manuscript, I recommend including a table with the specification of the aerosol types.

Section 2.1.2: Add a description of the treatment of the ocean BRDF.

Line 202: add a reference for the CALIOP-based climatology.

Section 2.3. In this section it should be discussed that for the OMI-MODIS hybrid method to work, the MODIS and OMI aerosol types as used in the retrievals should be consistent. This is mentioned later on in the discussion of the results, but should be added here.

Section 2.3. Because the MODIS retrievals are very insensitive for aerosol absorption, it would make more sense to use the scattering AOD instead of the total AOD in the hybrid method. The absorbing part of the AOD in the MODIS product is fully based on a prior information. The scattering AOD is defined by  $SSA \cdot AOD$ . Using the scattering AOD would result in a better separation of scattering and absorption effects, because these parameters are more orthogonal than the SSA and AOD.

Section 2.4. It is unclear why CALIPSO and AERONET are described in one section.

Line 296. The impact of distance between two satellites for cloud clearing has been quantified in Genkova, et al AMT, 5, 595-602, doi:10.5194/amt-5-595-2012, 2012. This

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reference can be used to quantify the impacts of the time difference between OMI and MODIS described here.

Line 314: I think it is meant “closest MODIS AOT retrieval to the OMI pixel center ..”

Line 322-323 “detector anomaly” should be replaced by “row anomaly”

Section 3. Please include if the AERONET observation closest in time is used, or if the AERONET observations are averaged over the time window.

Section 3. It should be discussed that by comparing with AERONET an additional implicit cloud clearing is performed. Can you provide statistics on the percentage of cases for which an OMI retrieval was present, but no AERONET observation was available due to the cloud flags used for AERONET?

Line 350: The criterion of 8 surrounding pixels selects spatial homogeneous cases, not necessarily “fairly clear sky conditions”.

Section 3. In addition to the scatter plot shown in Figure 2, I strongly suggest to also include histograms of absolute and relative difference between AERONET and OMI. The shape of the histograms (e.g. modal, long tails) can provide additional information on the behavior of the algorithm, that cannot be extracted from the presented scatter plots.

Line 414 “stretching” this is not clear, I think the authors mean the effect of the increasing size of OMI pixels towards the end of the swath.

Lines 432-442. It is unclear to me why the results of section 3 are not used in section 4. How does the comparison with MODIS change when the 8-neighbour criterion is applied to OMI. This should be investigated in this section.

Line 440. A closing parenthesis “)” is missing.

Line 468 “the instrumental inability to resolve the subpixel contamination due to the coarse spatial resolution.” This is a strange sentence because no instrument will re-

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solve subpixel clouds. However, the OMAERUV algorithm doesn't seem to use the higher spatial sampling available in the OMI data through the so-called small pixel data. These data could be used to construct a better cloud mask. Given the importance of the cloud clearing, this should be a recommendation for a future version of the algorithm and mentioned in the conclusions section.

Line 523-525. I miss the point of this sentence. Please clarify what you mean.

Line 580-583. The effect of the choice of the aerosol models shall be discussed when describing the hybrid method. See my earlier comments.

Line 606-607. I don't understand this sentence, please revise.

Line 610 Apart from aerosol shape, also the assumed aerosol size distribution is important.

Line 645 add a comma: "... samples, but ..."

Line 699. Although in this case the results of the SSA do not depend on the aerosol model, it does have a very strong effect on the Absorption AOD, which is a measure for the radiative effect on the atmosphere. This should be mentioned.

Line 707-711 "This ... aerosols." This section can be removed as it adds nothing.

Line 724 "This" should be replaced by "The"

Section Acknowledgements: there is missing information in this section.

Figures 8, 9, 10 and 11: please use consistent terminology. In these AAI is referred to as UVAI and UV Aerosol Index. This is confusing. By the way, I have a preference for UV-AI instead of AAI, but that is up to the authors.

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