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Comment on amt-2021-8

Anonymous Referee #2

Referee comment on "Retrieval of UV–visible aerosol absorption using AERONET and OMI–MODIS synergy: spatial and temporal variability across major aerosol environments" by Vinay Kayetha et al., Atmos. Meas. Tech. Discuss., https://doi.org/10.5194/amt-2021-8-RC2, 2021

Review of Retrieval of UV-Visible aerosol absorption using AERONET and OMI-MODIS synergy: Spatial and temporal variability across major aerosol environments by Kayetha et al.

This manuscript presents a method for deriving aerosol absorption from a combination of satellite and ground-based remote sensing measurements. The single scattering albedo is derived in five wavelengths in the UV-Visible range. The method can be applied over locations with co-located satellite and ground-based measurements. The method adds UV information on top of the existing data from AERONET. The method is applied to a data record over more than 100 sites globally for the period 2005 – 2016. Results are discussed per region.

Overall, I think this is interesting work which has a lot of potential. However, the way that it is presented can be much improved. I recommend splitting this manuscript in two parts, where part I describes the method, sensitivity study, case studies and validation, and the part II describes global and regional results. Part I would fit for AMT, whereas part II would better fit in ACP or a similar journal. Part I should answer questions like, what is the added value of this method with respect to the standard AERONET retrievals of SSA? How can the retrieved spectral behavior of the SSA be explained by the expected refractive index? That is why I encourage the authors to withdraw the current manuscript and resubmit it in two improved parts. Because of this recommendation I will not provide detailed textual comments on the current manuscript, but rather indicate where the work needs further improvements.

Section 3.

Before section 3.1 a text needs to be added that introduces the methods physical

background. E.g. what determines the radiation measured at ground-based and satellite level. A diagram would be useful for this.

Section 3.1.

The selected wavelengths in the visible are affected by NO2 and ozone absorption. However, these absorptions are apparently not part of the LUT design. These should be included, or the authors should justify why these absorptions can be neglected.

In the end, the method uses reflectances from the satellite instruments and derives the SSA using the LUT. Therefore, the SSA is the depended parameter for the method. For this reason, the axes of Figure 3 should be switched (x-axis reflectance, y-axis SSA). This will immediately visualize the problem for low AOD, where the method will be very noisy.

Section 3.3.

It is unclear where the surface pressure information is coming from.

Section 4.

The sensitivity analysis is incomplete. The approach to only perform a sensitivity analysis for parameters which are controlled in the retrieval is clearly not acceptable and also not true, because the real part of the refractive index and other aerosol model parameters are also selected as part of the algorithm. So, all identified parameters should be included in the sensitivity analysis, along with the surface pressure, signal to noise of the instruments, the (tropospheric) ozone column and the NO2 column. This should be presented in graphical way to convince the reader that the method is sound.

While the analysis of the GSFC method is good, I am left with a number of questions. First of all, the SSA should significantly lower values at 646 nm compared to the other wavelengths. Is this realistic. Is this in line with our knowledge about the refractive index in the visible? Is it possible that this is related to ozone absorption in the visible?

I propose that on top of the GSFC analysis, the authors present 3-6 cases studies of single retrievals over different sites, where for a given day for which both the AERONET SSA and the combined SSA retrievals are available. These cases should cover both good and bad comparisons and discuss the reasons for these results. This gives an opportunity to demonstrate the added value of the satellite method.

Section 7

Logically, the next section would be the validation (currently section 7).

The authors have chosen to compare only the 466 and 646 nm SSA to AERONET. Also comparison between 388 nm retrievals and 440 nm AERONET shall be included. Although the spectral distance is larger than for 466 nm (MODIS), it is the best way of also including the OMI retrievals in the validation.

The validation data should also be split by AOD bin. In this way I hope that some correlation can be demonstrated for the medium and high AOD values. Alternatively, times series of data sets over sites with large variability in SSA could be presented to convince the reader that the retrievals add value wrt to the results from AERONET. The current Figures 14 and 15 (top plots) are not very convincing, however the representation of Figure 15 (bottom) is much better.

Section 5

In section 5 regional results are presented.

There are different ways of computing the average SSA. If I understand it correctly, the unweighted average SSA is presented. Alternatively, given the dependence of the accuracy on the AOD, the AOD could be used as weights. This would be equivalent to computing the mean SSA as (1- mean(AAOD)/mean(AOD). Also, mean and standard deviations can be significantly affected by outliers, whereas the median and percentiles are more robust statistics. How would the presented results be affected if other methods of computing statistics are used?

The results are presented per region. However, given the poor spatial sampling the statistics will not be representative for the whole region. It is therefore questionable if this analysis is useful at all. For example, there is a huge difference between aerosols on the Californian coast and those of continental Canada, however they are in the region. The same is true Mediterranean sites and sites in Northern Europe as well as other regions. The authors should rethink how these data can be best presented, beyond the current split in regions. I suggest starting with some global maps where the data is plotted per season and aerosol type. Maybe as a circle of which a quarter is used per season or something similar.

Section 5 is very hard to read, as it mixes observations with speculations. Also results from large regions are discussed in terms of very local phenomena. Here a clear choice should be made by authors to either discuss the global distribution of the SSA, or to dive into the details of one or more regions. Now the scope is somewhere in between and that doesn't work for me. Furthermore, it should be clearly identified when the other claim that the data prove something, or when they speculate about possible explanations.

In many cases in Figure 6/7/8/9 a significantly lower SSA at 648 nm is reported as compared to the other wavelengths (e.g. 6a/b, 8 a/b/c, 9 b/c). Do the authors have an explanation of this, in terms of the spectral behavior of the refractive index? Is this reported in other studies? I am not convinced that this is not caused by measurement errors.

Section 6

I am not convinced by the analysis based on the mean AAE. Overall, satellite retrievals of the AE are difficult and the AAE is much more difficult. Also note that AAE is a combination of the spectral behavior of the AOD and that of (1-SSA). Before concluding jumping to conclusions on the AAE, the authors should first provide that there is any value in these mean AAE results.

Comments on Tables and Figures:

Tables 3. I propose remove this table to the supplemental material and make it available as complete data set (e.g. HDF5 file(s) or excel files(s)) for all the sites, containing both individual retrievals and statistics.

Figure 1. Move this to supplemental material.

Figure 10-12, replace the bars by violin plots or box-whisker plots.

Figure 13, replace the plots by box and whisker plots using the spread of the points instead of the assumed uncertainty.