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Atmospheric Measurement Techniques Discussions

Interactive comment on "The role of aerosol layer height in quantifying aerosol absorption from ultraviolet satellite observations" *by* Jiyunting Sun et al.

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OVERALL

The manuscript deals with a hot topic, i.e. the constraining the aerosol Single Scattering Albedo (SSA) using satellite observations. This quantity is difficult to capture by observations and at the same time very important regarding the radiative forcing of aerosol. The proposed scheme to infer SSA information is new and of interest to the AMT community. The manuscript is suitable for publication in AMT after the issues below are addressed.

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Discussion paper

GENERAL COMMENTS

The manuscript does not read smoothly. Formulations need to be improved in many instances. The specific issues listed below cover a number of these instances but the list is not exhaustive.

The study is dealing with a single case (a plume of one specific emission event). It needs to be discussed how robust are findings.

The study approach needs to be explained upfront more clearly. The choices regarding the source of data (UVAI, ALH, AOD, SSA) used for training the SVM-based scheme, for evaluating the SVM-based algorithms, and for evaluating the RTM-based algorithms, needs to be clarified (at a high level upfront, in detail in the specific sections).

SPECIFIC COMMENTS

Line 12: It is not clear how SSA is retrieved, which algorithm is employed. Reference to "conventional radiative transfer simulations" is not sufficient.

Line 13: The approach to constraining the SSA retrieval is not clear. Is the ALH fixed in the forward model used in the SSA retrieval?

Line 17: The sentence "In the second part of this paper, we propose. . ." is not clear. Clarify that the method relies on an empirical relation that has been established based on long-term datasets of UVAI, ALH and AOD based on the SVR concept. The term "data-driven" is misleading.

Line 20 (also caption Figure 8): AERONET does not "measure" SSA directly but retrieves it. Reformulate.

Eq. 1 is unclear and the variables are not introduced. Without more information the reader cannot guess how to interpret the superscribed labels "obs" and "Ray". It is recommended to explain the UVAI concept and highlight that the obtained index is

sensitive to elevated absorbing aerosol.

Line 41: What is meant by “with various spectral choices”?

Line 68: “quantitatively determine” –> quantify.

Line 72: data-driven –> empirical. Proposed to reformulate “We propose an empirical [. . .]. ML algorithms learn [. . .]”.

Line 77: one piece of information is missing: does the training of an SVM requires less training data than ANN?

Line 77: ML and SVM seem to be used interchangeably, which not entirely correct: also ANN can be seen as ML tools.

Line 79, 81: inconsistent use of singular and plural

Line 84: The term kernel functions is used as if it had been introduced already. Are these related to the support vectors?

Line 107: reformulate “TROPOMI ALH retrieval is based on the pattern . . .”

Line 102 “For the forward radiative transfer calculations, the input aerosol profile is parameterized as . . .” is this choice consistent with the assumptions made in the ALH algorithm?

Line 129: The relevance of the reference to Herman & Celarier is not clear. Does the statement “A spectrally flat As is assumed . . .” apply to the OMI LER product? Or do you need to make this assumption?

Section 3.1 falls short on an explicit and upfront specification of the source of the input data (such as AOD, ALH, UVAI) used for the RTM-based method. A discussion of temporal mis-registration between MODIS and TROPOMI data acquisitions is missing.

Line 156/157: it is not clear what are the implication of using surface reflectance data from the OMI LER for reproducing the UVAI using the RTM method. Please discuss.

It is assumed that the surface reflectance generated within a UVAI product can be reproduced in a straight forward fashion if needed.

Line 161: The justification of the reporting wavelength of the retrieved SSA is not understood; in the end it is determined by the OMAERUV reporting wavelength?

Line 173: Aerosol models cannot be a combination of a project and an algorithm. Rephrase.

Line 175: the phrase “The particle size distribution . . .” needs grammatical/syntactic fixing

Line 179: subtype “BIO-1” is referred to without explanation/reference

Line 177: real PART OF THE refractive index

Line 178: imaginary PART OF THE refractive index

Line 181: Sentence incomplete

Line 182 (also caption Figure 8): The specification of ∆ðİlJĚ in % in not clear. Clarify.

Table 1: The specification of the imaginary part of the refractive indices ðİlJĚ is unclear. For which reference wavelength are the numbers in the rightmost column valid? In the column “Refractive index imaginary part at 354 nm (k354) one expects an explicit list of values rather than a formula. Clarify.

Line 186: Absorbing Ångström Exponent –> Ångström exponent

Line 207: “13-year measurement OMAERUV and AERONET measurements” rectify formulation.

Line 210: an OMI pixel is collocated –> OMI observations are considered as collocated

Section 3.2.2 (Preparing training and testing data sets) is quite confusing. Please rewrite. Some terminology is used inconsistently (e.g. the terms “extra SVR”, “adjusted ALH”, “predicted ALH”, or “ALH from OMAERUV” and “ALH from OMI”). Maybe

introduce Table 2 and Flow charts (Figure 5) already at the beginning of the section.

Line 240, 259, 326: It is referred to “ALH from the OMAERUV” suggesting that ALH is generated by the OMAERUV algorithm. It is stated in the manuscript that these ALH values are actually taken from CALIOP. Please refer to “ALH from CALIOP” for clarity.

Line 240: It is stated that the ALH from the OMAERUV product (actually from CALIOP) may not have sufficient quality. Clarify what is the concern. Co-location?

Line 242: What is meant with the OMI ALH? Is it the same as the ALH from the OMAERUV (CALIOP)?

Line 245: Is the TROPOMI ALH the one retrieved from the O2-A band?

Line 249: It is stated that the “extra” SVR is trained on the Thomas fire case. A training sets should cover more than one case. Please discuss the validity of the approach.

Line 253: it is noted that this “extra” SVR is a temporary intermediate step to obtain a better ALH”. Please explain upfront the approach.

Line 255: It is stated that “there is no necessity to do this anymore once a reliable ALH product is accessible to build up training data sets, e.g. the TROPOMI ALH product that will be released in the near future”. Clarify in which sense the training set using TROPOMI ALH is expected to outperform the training set using CALIOP ALH.

Line 259: What is meant with “The rule of thumb ratio is 70% versus 30%”?

Eq. 3: Introduce the variable n.

Eq. 3: What is the dimensionality of omega? What is meant with ||omega||? Some kind of norm? What is the dimensionality of omega?

Eq. 4: Introduce the variable x.

Eq. 4: Why introduce the kernel function K? What is done with it?

Section 3.2.4 Data for case application: Please report the number of validation samples

Figure 5: The figure shows at the same time SVR based ALH prediction and SVR based AAOD prediction, this is confusing. It would help to depict the two schemes for AAOD prediction in one flow chart, and the ALH prediction in a separate one.

Figure 6 (also Line 206): Why is the sign of the correlation coefficient not reported? Report the sign or justify and clarify in caption that |rho| is reported.

Figure 6: For which ALH parameter are the correlations reported? For the predicted one or for the one from CALIOP?

Figure 7: The 3D plot is hard to interpret. Recommended to replace it with 2D scatterplots (AOD versus ALH) where the UVAI is only color-coded.

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