

Closing the Gap: An Algorithmic Approach to Reconciling In-Situ and Remotely Sensed Aerosol Particle Properties

The authors reported the ISARA retrieval framework in detail and attempted to validate it with ACTIVATE mission data. I appreciate the plethora of information re: the approach and the comparison datasets. However, I found the logic to be confusing at times despite the high level of details (see my specific comments and questions below). In addition, it would be helpful if the authors validated the retrieved aerosol properties (κ) with any available aerosol chemical composition from the same mission. Perhaps it is because the manuscript is rather long, there are quite a few typos and odd sentence structures. This manuscript would benefit from a major revision that I hope will address my questions and comments below.

General comment:

For particle diameters $> 1 \mu\text{m}$, the authors should consider reporting them in μm instead of nm (e.g., 3488 nm on L281).

The authors should consider updating the symbol of surface area concentration from $dA/d\log D$ to the conventionally used name of $dS/d\log D$.

In a few places (e.g., L427), the authors used the term “aerosol particles” but all aerosols are particles.

There are a lot of repeated definitions of acronyms (e.g., HSRL-2 on L233, Table 2 title and content) that should be cleaned up. There are also instances of using acronyms first then defining them later (e.g., CRI appeared on L156 then defined on L177).

Was there relevant aerosol chemical composition available from the ACTIVATE mission (e.g., AMS) that the authors could have used to compare with derived hygroscopicity parameter κ and real refractive index?

Specific comments/questions:

L120-122: the authors already defined the diameter range for fine mode and coarse mode particles on L95-97, so remove them here.

L166: define what phase function represents and why it is introduced

L205-211: is it necessary to list out the dates of the ACTIVATE deployments instead of referencing the campaign overview by Sorooshian et al. (2023)? This information does not seem to be used later.

L239-247: a lot of repeated information on HSRL-2 products. Please rewrite this part of the paragraph to be more concise.

L 274: extra words “the 30”

L275: what is “ram heating” in the context of SMPS data?

Table 1: in HSRL-2 section, missing a comma between “532” and “1064 nm”. Also, repetitive definitions of HSRL-2 and RSP in both the table title and the table content. Consider adding acronym of extinction coefficient, backscatter coefficient, etc. in parameter description for clarity.

L291: “success rate” of...what exactly? Please be specific. Is it in retrieving coarse-mode particles?

L319: extra word “are”

L339: water vapor “density” or mixing ratio?

L346: travel “up to” 8 km?

L369-370: repeated definitions of n^0 , a^0 , v^0 (previously defined on L190-197). This repeat of definitions also happened to other acronym and symbols (e.g., Table 2 title). Please re-check the other sections of the manuscript as well.

L383: why did the authors restrict calculated absorption coefficients to within 1 Mm⁻¹ of the measured values? Restricting the calculated values within a constant 1 Mm⁻¹ may bias the filtering process to higher absorption values, where 1 Mm⁻¹ difference is a smaller fraction of the total value (e.g., 10% of 10 Mm⁻¹) vs. smaller absorption values (e.g., 50% of 2 Mm⁻¹). Should this restriction be a percentage difference like with scattering?

L427: the authors stated that the derived hygroscopicity for aerosol in the ACTIVATE region is low ($\kappa \leq 0.1$) but also concluded that the aerosol population is sulfate-dominant. This value of κ seems really low. Organic is often assumed to have kappa value of 0.1. Sulfate (inorganic) are usually with kappa > 0.4. An aerosol population dominated by sulfate and organic as stated by the authors should have kappa >> 0.1.

L432: misspelling with extra “i” in scattering

Figure 3: this does not seem like a correct representation of the calculation process. On L415-420, you mentioned that $C_{RH,meas}$ was actually calculated from $C_{dry,meas}$ and estimated gamma from Eq. 7. Really, the authors were using a single measured $C_{RH,meas}(85\%)$ and $C_{dry,meas}$ to estimate gamma. Then, they used gamma to derive $C_{RH,meas}$ at 80% - which now is an estimated value and not a measurement. Why didn't the authors use $C_{RH,meas}(85\%)$ from the nephelometer instead?

L452: extra word “aerosol” or “particle”

L461-462: typo with “,” between number and μm

L458-464: the use of CAS size distribution to derive coarse-mode particle properties contradict with L320-325 in section 2.3.2, where the authors said to use CDP mainly and CAS only if the other 2 datasets (CDP and FCDP) are not available. Please review and correct the appropriate section. Also, if the author chose CDP over CAS, what is the reasoning when CAS provides a wider range of available aerosol diameters?

L506: the reported % does not need to be accurate to 2 decimal points (e.g., 26.49%), especially when the authors reported the biases as full % point (L587).

L515-517: why did the authors remove HSRL-2 AOD < 0.08 where HSRL-2 AOD uncertainty is 0.02 (stated in Table 1)?

L521-524: I did not follow the logic. What is this 50% AOD difference criteria [between HSRL-2 and RSP derived AODs] based on? Similarly, how did the authors arrive at the filtering criteria of coarse-mode AOD being < 0.1 ?

L555: extra “respectively”

Figure 5: the authors could add lines that represent the range of measured data from Figure 1 on Figure 4 to show the range in synthetic data is smaller than the actual measurement (L578-579). Also, there are currently 2 panels (b) in this figure.

L587: did the authors mean “respectively” instead of “respectfully”?

Table 3: update the table title since it also includes information for Table 4. Also, the authors repeated the same information in the first paragraph immediately following Table 3.

L699: remote sensors or remote sensing retrievals?

Figure 11: why don't we have LR > 2 km for HSRL-2 data? Both C_ext and C_bsc are available at these altitudes for HSRL-2 (panel b, e). Also, panel (i) seems to be missing from Figure 11.

L745: compared to case 7, case 9 with high smoke counts and high HSRL AOD would be interesting to investigate further compared to case 7.

Figure 12: the authors should add uncertainty to the calculated kappa, RRI, IRI that were derived with ISARA.

Conclusion: I am not sure why the authors defined ISARA, CRI, etc. again when they had been mentioned all throughout the manuscript. Please remove this redundant information.

L792: Similar to the work...