Atmospheric Measurement Techniques Discussions

Atmos. Meas. Tech. Discuss., referee comment RC1
https://doi.org/10.5194/amt-2022-205-RC1, 2022

Comment on amt-2022-205

Anonymous Referee #1

Referee comment on "First assessment of Aeolus Standard Correct Algorithm particle backscatter coefficient retrievals in the eastern Mediterranean" by Antonis Gkikas et al., Atmos. Meas. Tech. Discuss., https://doi.org/10.5194/amt-2022-205-RC1, 2022

The authors assess the particle backscatter coefficient profiles from Aeolus/ ALADIN using co-located ground-based lidars at three locations in Greece, together with auxiliary model and satellite datasets. They attempt to attribute discrepancies between space borne and ground based lidars to (i) the natural variability of aerosols, (ii) instrument or retrieval limitations and/or (iii) spatial temporal co-location issues. This paper needs substantial improvements before it is worthy of publication in AMT.

Major comments

In a very general sense, numerous sentences throughout the paper are too wordy and their structure just too complicated. For example, “are utilized towards an optimum characterization of the probed atmospheric conditions under the absence of a classification scheme in Aeolus profiles” could be replaced by something along the lines of “are used to characterize the atmosphere as Aeolus/ALADIN does not provide an atmospheric classification product”. This makes for a lengthy paper. Other examples are “obtained results” instead of simply writing “results” or “probed Atmosphere” instead of “atmosphere” throughout the paper.

We recommend that the authors:

. Use ALADIN in all caps or Aeolus/ALADIN consistently (instead of small caps and/ or interchangeably using both names). Also, they should spell it out once when first introduced (i.e., “Atmospheric LAser Doppler Instrument”).

. Add a table describing the lidar(s) at each of the three locations. This table could

contain, for example, the lidar's name, a small description, its limitations and uncertainties, its products. It could also contain information on how cloud screening is performed and possibly a dominant type of aerosols at each location (together with references related to these stations).

. Add a brief description of the two models used in this study and, especially, their limitations. The way the analysis is written might sometimes give the impression that model results are considered as accurate as observations.

. Add more text comparing their results to those in studies such as Baars et al. (2021) and Abril-Gago et al. (2022). In a more general sense, it would be helpful to open the paper by describing in more detail what their study brings to the table/ how it complements other studies.

. Specify the way they average the MODIS AOD and add more analysis to their spatial characterization of aerosols at the three different stations. We believe that there is more to the characterization of aerosol spatial variation than simply averaging the AOD in different boxes. The authors should refer to previous studies such as Anderson et al., (2003), Sayer and Knobelpiesse (2019) or Shinozuka and Redemann (2011)

. Discuss the limitations of their cloud screening of the Aeolus/ALADIN aerosol backscatter profiles using SEVIRI.

. Discuss whether the Aeolus/ALADIN, with its 87 km horizontal resolution, is in fact able to characterize the aerosol natural variability at the three locations.

. Avoid strong statements such as "very good performance" when it pertains to a specific altitude, one case study and no quantification of the differences between space borne and ground based lidar in that case.

. Use additional satellite derived aerosol information (e.g., CALIOP, TropOMI) to further characterize the aerosol during their case studies.

. Specify why they use AERONET level 1.5 instead of more accurate level 2 data (quality assured). Also, they should show AERONET-derived FMF and SSA at different wavelengths throughout the day (in addition to what they already do -- spectral AOD and angstrom exponent). This would be like Figure 8 in Abril-Gago et al. (2022). Let us remind the authors that, in addition to a size difference, a difference in SSA at two wavelengths from AERONET could point to the presence of dust versus smoke (e.g., Russel et al., 2014; Kacenelenbogen et al., 2022).

. Specify why they show Aeolus/ALADIN profiles that are cloud contaminated (or "unfiltered") in their analysis. It is not obvious why there would not be more disagreement between cloud-unfiltered space-borne and cloud-filtered ground-based profiles compared to cloud-filtered space and ground profiles. Or is this a way to test their SEVIRI-based cloud filtering method?

. Figures should be called in the order they appear.

. Shorten the conclusion

Detailed comments:

Title: The authors might want to add "ALADIN" and "aerosol" in the title for increased searchability

Line 29: Why not give examples after "a variety of aerosol species."

Line 32: Why is PANACEA spelled out but not AERONET, CAMS, MERRA-2 etc…? We recommend either spelling none or all of them.

Line 33: we recommend writing "sunphotometry observations"… "model reanalysis" …" modeled air mass back trajectories"

Line 36: Again, multiple sentences throughout the paper are too wordy. For example, "are utilized towards an optimum characterization of the probed atmospheric conditions under the absence of a classification scheme in Aeolus profiles" could be replaced by something along the lines of "are used to characterize the atmosphere as Aeolus/ALADIN does not provide an atmospheric classification product"

Line 40: "very good" is too strong a statement here.

Line 44: We recommend writing “46 identified cases when using [this time frame] at all three stations…”

Line 47: “positive tendency” could be replaced by “improvement” and “both Aeolus vertical scales” by “multiple Aeolus vertical scales”

Line 48: we recommend to replace “justified” by “explained” + “in the vertical the Aeolus performance”

Line 49: “performance decreases” followed by the explanation for that decrease is not clear

Line 83: We recommend “Such observations are provided by networks… or by dedicated experimental airborne (Ansmann et al., 2011; Weinzierl et al., 2016) or shipborne campaigns (Bohlmann et al., 2018)”

Line 87: We recommend “characterization of aerosol vertical structure at global (e.g., Liu et al., 2008) … was performed using CALIOP … and CATS… respectively on the CALIPSO and the ISS…”. CATS could use other references such as Lee et al., (2019)

Line 108: “good quality” needs a reference

Line 116: ALADIN needs to be in all caps throughout

Line 117: We find the description in Flament et al., (2021), a little clearer (e.g., “The UV laser beam is linearly polarized at the laser output. It goes through a quarter-wave plate before being routed towards the telescope and is thus transmitted towards the atmosphere with a circular polarization…”)

Line 126-128: This is important and should be explained in more detail. This paper is about validating Aeolus/ ALADIN. The limitations of the lidar should be clearly explained and other papers should be referenced.

Line 130: We recommend “ALADIN” and why are “continuous” calibration and validation needed? Please explain.

Line 136: We recommend “L2A aerosol optical properties”

Line 138: Regarding the “excellent agreement” here, we recommend adding some nuance. These results for a case study with a strong non-depolarizing aerosol, were ~satisfying only between ~4 and 8km.

Line 142 – 156: If the type of aerosols over the three regions is discussed here, then you might consider not repeating it elsewhere (e.g., section 4). In general, we recommend adding a table describing the lidar(s) at each of the three locations. This table could contain, for example, the lidar’s name, a small description, its limitations and uncertainties, its products. It could also contain information on how cloud screening is performed from these ground-based lidars and possibly the dominant type of aerosols at each location (together with references related to these stations).

Line 184: HSRL was already introduced on line 116.

Line 187: We recommend “are backscattered”

Line 206-215: What is the purpose of describing algorithms that are not used in the study (e.g., ICA and MCA)?

Line 216: We recommend “the primary and most reliable”

Line 217: We recommend “measured signals in the Mie channel”

Line 224: We recommend “signals in each channel”; also, the sentence is not clear

Line 260 and section 4: Again, the three stations, type of lidar(s), products, uncertainties, limitations (e.g., overlap), etc. could really use a table. That table could also show a predominant aerosol type over the region and a median and standard deviation AOD from satellite(s).

Line 263: We recommend using “different” instead of “adverse”

Figure 1-i: It would be helpful to write “all three stations are within Xkm of each other”.

Line 272 to 275: The authors must mean “to ensure the consistency of all lidar-derived observations”?

Line 279: We recommend deleting “measurements” here.

Line 280-281: Why is this assumption plausible? Does it remain to be tested?

Line 286-288: Doesn’t this apply to all three stations? Also why not add biomass burning aerosols here?

Line 338: We recommend “Aerosol spatial variability in the vicinity of the PANACEA sites”. A description of the dominant aerosol type at each station would fit well here but then the authors would have to delete it from the introduction to avoid repetition.

Section 4.4: The purpose behind studying the spatial variability could be explained more clearly. Our understanding is that the authors are attempting to characterize spatial variability to explain a potential disagreement between Aeolus/ ALADIN and ground-based lidars. A disagreement could be due to imperfect spatial co-location and/or simply ALADIN’s 87 km horizontal resolution. The authors are studying horizontal variability by using total column integrated AOD and that should be mentioned as well. There could be minimal horizontal variability but a strong vertical variability. It is also not clear how the authors have computed the mean AOD from MODIS. Is it a arithmetic or geometric mean? It does make a difference -- see e.g., Sayer and Knobelspiesse, (2019). Spatial characterization analysis usually uses mean and standard deviations within each satellite grid cell and/or the variation between consecutive satellite pixels or airborne measurements within a region (e.g., Anderson et al., 2003 or Shinozuka and Redemann et al, 2011).

Fig. 2: The orange arrow is hard to see; It is also not clear if the analysis involving the 46 cases considers the closeness of the actual track to the station (e.g., better spatial colocation on July 1st)

Line 381: The authors should discuss this “temporal window extension” in more detail and attempt to explain its consequences.

Line 385-397: We recommend “derived from radiances measured by SEVIRI”, “indication

of cloud presence"; the limitations of using this SEVIRI cloud mask should be discussed. For example, could SEVIRI be missing small broken water clouds? What about cirrus clouds? and what would be the consequences on the Aeolus/ ALADIN aerosol profiles?

Line 403: Regarding (ii), how will the authors differentiate the effects of natural variability, the imperfect co-location and the errors in the Aeolus/ ALADIN instrument? See e.g., section "nature versus noise" in Anderson et al., 2003. Regarding (iii), this was already demonstrated in numerous studies.

Line 406: We recommend "(…) Cal/Val study to facilitate the interpretation of our findings and to identify possible upgrades in the Aeolus/ALADIN retrievals."

Line 409: We recommend "the results are depicted in Figure 3"

Line 411: We recommend "… Aeolus retrievals are provided at a coarse horizontal and vertical resolution …"

Line 420: We recommend "To depict the spatial patterns (…)"

Line 423: The fact that MERRA-2 and CAMS provide "aerosol products of high quality" is a strong statement and should be explained. The explanation should include model evaluation results from previous studies. Model aerosol optical properties and model aerosol speciation have serious limitations, which should absolutely be mentioned in the text.

Line 425: Why not use AERONET Level 2 (quality assured) instead of Level 1.5?

Line 427: We recommend "characterization of the aerosol load and size over the station"

Line 432: Figures should be called in the order they appear. Figure S4 is introduced before S1, S2 or S3.

Line 435: We recommend adding "at 550 nm"

Line 437: The truth should be in the sunphotometer direct measurements. This sentence, as written, could be interpreted as things being the over way around.

Line 438: The Angstrom exponent should be briefly explained here (i.e., difference of AOD at two (or more?) wavelengths that informs on the particle size) and references for typical dust angstrom exponent should be added to the text (e.g., Dubovik et al., 2002)

Figure S2: This10 min-worth of high VLDR content looks suspiciously high compared to the consecutive profiles in the curtain plot. How do the authors explain that dust was present for only 10min and then suddenly disappears?

Line 442: Why would this case be ideal for evaluating Aeolus/ ALADIN as we know Aeolus cannot measure non-spherical dust properly?

Line 449: What is meant by "statistical uncertainty margin"?

Line 473: The authors mention "fine particles" but an explanation is missing here; we recommend "until their arrival over…"

Line 480: We recommend "AOS are mainly attributed …"; the models seem to be taken, once again at face value here (i.e., they seem to be treated the same as observations, but model species (and their spatial variation) are sometimes not reliable)

Line 484: We recommend "ALADIN reproduces the layer's structure well"

Line 493-496: This sentence is not clear. The fact that MERRA-2 and CAMS aerosol optical properties and speciation disagree cannot be directly connected to a good or bad performance between models and AERONET. One would need to directly compare aerosol optical properties from MERRA-2 or CAMS to aerosol optical properties from AERONET. Also, the way this is written could make it sound like AERONET provides aerosol species, which it does not. Instead, it measures aerosol optical properties, which can be used to define aerosol types and that can be indirectly translated into aerosol chemical species in certain cases (Kacenelenbogen et al. 2022).

Line 502: We recommend "Aeolus performance depends on altitude according to Polly…".

Line 554: The reader needs to be reminded which case studies are included here -- are those the 46 case studies of line 383?

Line 563: The statement referring to “the contribution of depolarizing particles is quite low based on the ancillary dataset” needs more explanation and needs to be supported by some results.

Figure 4: The metrics need to be described, like in Abril-Gago et al. (2022). Authors should specify why they show Aeolus/ALADIN profiles that are cloud contaminated (or “unfiltered”) in their analysis. Is is not obvious why there would not be more disagreement between cloud-unfiltered space-borne and cloud-filtered ground-based profiles compared to cloud-filtered space and ground profiles. Or is this a way to test their SEVIRI-based cloud filtering method?

Line 586: We recommend “Fig. 4” instead of “Fig. 5”.

Line 591-592: The authors should mention that “SCA mid-bin” is expected to perform better than SCA

Line 597-599: This is a repeat from line 137

Line 624: Do the authors mean low SNR instead of high SNR?

Figure 6-7: Again, why show cloud contaminated Aeolus/ALADIN profiles? Also, why show SCA instead of SCA_bin as the latter is expected to lead to better results (already shown in Fig. 5)?

Line 641: Again, can Aeolus/ALADIN profiles still be cloud contaminated after applying the SEVIRI cloud mask?

Line 641-643: This statement about not using QA flags appears too late in the text. It should be in the method or the Aeolus/ALADIN section.

Line 654: “many similarities” needs to be described in more detail.

Line 682-688: The authors should explain why they expect a difference in performance between the ascending and descending orbital data. Grouping the data per orbit direction seems inconclusive and we question the usefulness of mentioning the results.

Line 701-705: This appears too late in the text. It should be part of the comparison method between Aeolus/ALADIN and ground-based lidars.

Line 702: It is not clear what the authors mean by “the theoretical assumptions”.

Line 719-722: Again, the authors should add some nuance to the discussion on model performance.

References:

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