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Comment on amt-2021-181

Ad Stoffelen (Editor)

Editor comment on "Aeolus L2A Aerosol Optical Properties Product: Standard Correct Algorithm and Mie Correct Algorithm" by Thomas Flament et al., Atmos. Meas. Tech. Discuss., https://doi.org/10.5194/amt-2021-181-EC1, 2021

Review of "Aeolus L2A Aerosol Optical Properties Product: Standard Correct Algorithm and Mie Correct Algorithm" by T. Flament et al.

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General Comments

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This paper gives the impression that the evaluation/validation (and maybe development) of the aerosol/cloud products from Aeolus is still in the early stages, in marked contrast with the aeolus wind retrievals1. The paper presents only one example using real data where the lidar ratio results may be plausible. Almost 3 years after launch, I would have hoped for a more advanced state with respect to the aerosol/cloud product evaluation/validation.

It could well be argued that this paper is premature, however, having said that there are also reasons why this paper is potentially publishable at this time. This paper could serve as a point of reference for the lidar community and to serve as an accessible introduction to the instrument and the existing L2 aerosol/cloud retrieval algorithms. With regards to the later point, to be useful, the presentation of the paper must be improved. I found several areas to be more confusing than illuminating and, at times, the presentation seemed geared more towards "Aeolus insiders" rather than the wider lidar aerosol-cloud community.

The paper was also "thin" on examples using real observations imparting on the reader of the paper no real feel at all for the quality of the data. To this end, the authors should include additional examples, for example, showing:

-profiles and 2-D plots of the Aeolus Attenuated backscatter (both before and after cross-talk correction).

-profiles and 2-D plots of the retrieved extinction and backscatters.

-comparisons of the extinction and backscatter retrieval results for the MCA and SCA algorithm.

The above examples should, ideally, span an appropriate number of representative cases (e.g. cirrus clouds, light and heavy aerosol loadings etc..)

My specific comments follow.

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Specific Comments
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First off, I am puzzled by the use of "correct" in the name of the algorithms refered to by this paper. There are other approaches to inverting HSRL signals to derive extinction and lidar ratio that are mathematically valid. What is special about these algorithms that make them "correct" ? It would be useful to the reader if this point was somehow addressed in this paper.

We added the following discussion in section “high noise and extinction retrieval”:

“ The SCA is very similar to the classical log-derivative algorithms but the thickness of ALADIN range bins (up to 2 km) mean that the particulate optical thickness ($\alpha_p * \Delta R$) can be large and the approximation used for the molecular extinction (Eq. 6.34 in \ cite{flamant_aeolus_2021}) cannot be used for α_p. This is why we later need to inverse function H rather than simply derive the logarithm of the attenuation of the Rayleigh signal. As a side note, this refinement is also the reason why the adjective "correct" was added to the name of the algorithm.”

Abstract Line 1:

"Although ALADIN is optimized ....."

Corrected

Abstract: Line 11:

The last line is badly worded. I suggest "This is illustrated using Saharan dust aerosol observed in June 2020".

We changed the sentence to the reviewers suggestion.

Page 1: Line 19:

I find this short description awkward and not accurate enough. I suggest something like: "Two separate main optical detection channels are implemented on-board ALADIN. They are referred to as the Mie and Rayleigh channels. Both channels detect a mixture of particulate and molecular

scattering. However, the primary task of the so-called Mie channel is to detect the spectrally narrow (FWHM on the order of 10s of MHz) return from atmospheric hydrometers. The Rayleigh channel primarily detects the spectrally broader (FWHM of several GHz) backscatter from atmospheric molecules."

We adopted the reviewer's suggestion.

Page 2: Line 32

Delete the "In" directly after the reference to Winker et al.

Done

Page 2: Line 33

2006 was 15 years ago. I think you can delete the "already"...perhaps "previously" was meant.

We removed "already".

Page 2: Line 34

Please be specific. What is "all the available information" ?

We changed to: "… is estimated using information from several wavelengths and a depolarization channel"

Page 2: Line 46.

This is a very interesting point. Please provide a reference (even if it is only a tech note or report).

We added a reference to an EGU presentation:

Letertre-Danczak, J., Benedetti, A., Vasiljevic, D., Dabas, A., Flament, T., Trapon, D., and Mona, L.: Aerosol Assimilation of lidar data from500Satellite (AEOLUS) and Ground-based (EARLINET) instruments in COMPO-IFS., other, pico, https://doi.org/10.5194/egusphere-egu21-4799, https://meetingorganizer.copernicus.org/EGU21/EGU21-4799.html, 2021

Page 2: line 53

Please mention how can the general community get access to the updated L2A ATBD.

This is indicated in the reference. We changed the sentence from "can be found on-line" to "can be found on the ESA reference page :https://earth.esa.int/eogateway/catalog/aeolus-l2a-aerosol-cloud-optical-product". We also requested ESA to investigate the possibility to obtain a DOI, which is not ready at the time of answering this review.

Page 2: Last line

"...followed by a conclusion" ==> "...followed by a conclusion section".

Corrected

Page 3: Line 68

I am confused by the reference to the "..previous 24 sec cycle of the burst-mode operation of the laser". Previous to what ? Was this burst mode used early on in the mission ? If so, why was it no longer used ? Or, was it something previously planned but not implemented ?

We clarified this paragraph by saying that some of the parameters foreseen at the time of writing the science report were changed before launch. Among others, the discontinuous burst mode has been replaced by a continuous mode of observation.

Page 3: Line 75

"Fine bins.." ==> "Finer resolution bins…"

We changed “fine” for “thin bins”

Page 3: Lines 83-94 and Fig. 3

I found that the discussion of the spectrometers to be very confusing ! Only after reading through the L1 and L2 ATBDs, it became clear that the Rayleigh A and B signals are the result of integrating the images projected on CCD detectors. So for the Rayleigh channels, for each time-height bin two spectrally integrated measurements are available. This should be explained here.

For the Mie channel I found the presentation here to be misleading. The text and Fig. 3 first had lead me to believe that in the case of the Mie channel, that the data yieled by the device was a spectrum such as that illustrated in Fig.3. It took some time and iterating between the two documents, to realize that that for the Mie channel, that the curve shown in Fig. 3 corresponds only to the central position of the Fizeau wedge !

Only after reading through the L1 and L2 ATBDs I understood that there are 16 different spectral channels available. Further, the response of each channel is the result of integrating the spectrometer output image along the different columns (corresponding to wavelength shift). Since the central wavelength varies as a function of Fizeau wedge position, the measurement will consist of the INTEGRATED filter spectral response (e.g. as shown in the bottom right-panel of Fig. 3) with the center frequency shifted according to its position along the CCD rows.

I understand that the author's would likely desire to keep the explanation concise, however, the presentation here really needs to be more detailed and accurate ! It did cost me some time to understand what was being shown here and how the instrument really functioned and I am sure this would also hold true to many other readers in the general community.

We understand that this description is not enough. We now aim to better introduce the interferometer design and the L1B product that is used as input to the L2A. We also added a better

description of the calibration coefficients and how they model the transmission of each type of spectrum, Rayleigh or Mie, through each channel. Channels which are also (somewhat confusingly) named Rayleigh or Mie.

Page 6: Lines 104-109

The description of the "MCA" is likely incomprehensible to anyone not intimately involved with the data processing itself ! What does "some sort of cross-talk correction" mean ? What is the L1B-derived scattering ratio ?

This should be easier to understand with a clearer L1B introduction now. We removed “some sort of”

Either provide more details about the MCA (even references to the appropriate sections of the publicly available ATBDs would help) or, if it is deemed not essential, to the paper just skip it.

The same general comments apply to the description of the ICA.

We added that we mention the ICA and group product for completeness and do not intend to describe it more in detail. The longer paragraph about them was removed.

We specifically added that we do not recommend to use them. As we describe the data released with v3.12, we think this might be a useful indication to the users.

Page 6: Lines 110

"At last.." ==> "Lastly,.."

Corrected

Section 2.2.1

See my later comment (Page 13: Line 252)

It would be useful for the general reader if it were to be explained what advantages (or disadvantages) the SCA method have compared to the usual method of determining extinction by calculating the log-derivative of the Rayleigh ATB profile ? Off hand. I can think that the low vertical resolution bins dealt with here may be a factor. Is this correct ?

Please see the response to the other associated comment.

Are any multiple-scattering considerations taken into account in the retrieval. It looks like they are not. Do you expect this to have any impacts on e.g. cirrus cloud retrievals ?

Multiple scattering is not taken into account in our algorithms. It is usually expected that the extremely narrow field of view of ALADIN is enough to avoid strong multiple scattering influence on the observations.

Page 6: Line 118-119

"concision" is rarely used in modern English. I suggest "brevity" or "conciseness".

Sorry, that was a Gallicism!

The sentence is awkward: I suggest something like:

For the sake of brevity, only an outline of the SCA algorithm is presented here. Only the main features of the algorithm, necessary to understand the subsequent sections, are covered."

We adopted the reviewer's suggestion.

Page 7: Line 129

Delete the "(dR(z)=R'(z)dz)" It is trivial and does not add anything to the presentation.

It has been deleted.

Page 7: Line 144

"..equation (2)" ==> "..equations (1) and (2)."

Changed according to suggestion

Page 8: Line 180

"..thermal constraint on the primary mirror..." does not make any sense here. Do the authors mean "thermally induced distortion" or "thermal strain" ?

Yes, we changed the phrasing to "thermally induced distortion".

What is meant by "orbit phase" ? Do the authors mean the "orbit position" ? Does the distortion vary predictably along the orbit or is function of the e.g. solar background ?

We changed "orbit phase" to "position along the orbit".

The distortion seems to vary with the up-going thermal infrared radiation from the Earth.

Line 183: "...called the Instrument....(IRC) mode,…"

We added "mode"

Line 184" "...target with negligible Doppler shift due to the nadir pointing."

We changed the sentence according to the reviewer's suggestion

Page 9: Line 190

(Also relevant to Eqns. 5 and 6) What is the maximum height given by the AUX_MET product. Is there any account given to the Rayleigh transmission between the top to the AUX_MET product and the top-of-atmosphere ?

The AUX_MET reaches ~80 km high, we use it to simulate optical properties of the molecular atmosphere up to this altitude, but do not account for anything higher than that.

Page 9: Line 198

"Constraints" ...see my comment (Page 8: Line 180)

changed to distortions

Page 9: Line 210:

It would be useful if the authors could elaborate on this point a bit. For example, what order of magnitude error do they believe background aerosol levels may have on the accuracy of the calibration?

This is a difficult question. The L1B scattering ratio is used qualitatively, and its precision would not be sufficient to try to correct for the impact of particles on the calibration. If we trust the SR values, the absolute maximum increase in signal would be 16 %. In reality, this is probably only a few percent.

We now mention that "… might overestimate the radiometric coefficients by a few percent. Future work will investigate this potential source of bias."

Page 10: Line 215

See my comment above (Page 6: Lines 104-109). To the general reader the "L1B scattering ratio" is a meaningless term unless you explain it !

We added a section about the input data of the algorithms as section 2.2 of the revised paper.

Page 11: Lines 223-229

How is the lidar ratio chosen? Is it fixed or does it vary with altitude, latitude etc..

The MCA was designed to use a climatology of aerosols lidar ratio. However, the MCA doesn't have a capability to distinguish between clouds and aerosols. in practice, such a climatology isn't used and a fixed lidar ratio is used. It is currently of 1/0,07, i.e. around 14.

Section 3.1

It would be useful if the magnitude of the results of only measuring the co-polar return was discussed !

We added a discussion about the measured lidar ratios.

Page 12: Line 245:

"Designed as a wind lidar, ALADIN does not have the ability to measure depolarization". This sentence(along with the text that follows it) implies that this wind lidar do not (can not?) measure depolarization. Is this true in general or only for the specific design of ALADIN ? What design constraint has lead to ALADIN not detecting the co-polar return.

We mention that ALADIN is a wind lidar as a way to underline that aerosol studies where not in the initial scope of the mission. The main technical constraint is then that studying depolarisation was not part of the objectives of the mission.

This is the new sentence:

“Designed as a wind lidar, ALADIN was not initially aimed at observing aerosol optical properties in detail. Under these requirements, it was not fitted with the ability to measure depolarization.”

Also, ALADIN transmits and recieves circularly polarized radiation NOT linearly polarized !

The polarization of the beam at various stages inside the instrument has been better described. We hope that the text is now clearer and that a curious reader can find enough information with the schematic of the instrument in the Science Report. Below is the modified paragraph:

“The UV laser beam is linearly polarized at the laser output. It goes through a quarter-wave plate (Fig. 4.13 in ESA 2008) before being routed towards the telescope and is thus transmitted towards the atmosphere with a circular polarization. On the way back, backscattered light goes again through the quarter-wave plate. The circularly polarized light that was transmitted might come back elliptically polarized in the case it was backscattered by depolarizing targets. After going through the quarter-wave plate is a mix of linearly polarized light, along the same direction as the transmitted light (co-polar) or along the perpendicular direction (cross-polar). The beam then reaches a polarized beam splitter. The co-polar light is routed towards the interferometers, while the cross-polar light is routed back towards the laser and is lost for the analysis. This means that, in order to compare Aeolus observations of backscatter coefficient and lidar ratio to other instruments, only the co-polar component must be considered.”

Page 13, Section 3.2

The concept of the relationship between the extinction profile and the log-derivative of the Rayleigh attenuated backscatter profile is used throughout this section. From a mathematical view-point, it is certainly true that any approach to retrieving the extinction solely using the molecular backscatter profile (either explicitly or implicitly) involves computing the log derivative of the attenuated backscatter profile. This must be true also of the SCA approach briefly described in Section 2.2.1. It would be useful to guide the reader with regards to this point. For example, outlining how the SCA approach is related to the standard log-derivative method for retrieving extinction would be useful !

In the L2A ATBD, the approximation applied to molecular extinction In Eq. 6.34 is not applied to to particle extinction in Eq. 6.35. This is because ALADIN range bins are thicker than for most lidar systems and the total extinction from particle within a given range bin becomes large. The use of function H(x) rather than simply e^{-x} is more “correct”. We wrote the following paragraph:

“Extinction can be calculated in a simple way from the molecular backscatter, or more precisely, from its derivative. The SCA is very similar to the classical log-derivative algorithms but the thickness of ALADIN range bins (up to 2 km) mean that the particulate optical thickness ($\alpha_p * \Delta R$) can be large and the approximation used for the molecular extinction (Eq. 6.34 in \cite{flamant_aeolus_2021}) cannot be used for α_p. This is why we later need to inverse function H rather than simply derive the logarithm of the attenuation of the Rayleigh signal. As a side note, this refinement is also the reason why the adjective "correct" was added to the name of the algorithm.”

Page 16: Line 308

"..are out of the graphics.." ==> "..are off scale in Figure 9…"

This was corrected according to suggestion.

Page 16: Lines 229-330:

The naming of the instruments and the platforms they are on are all conflated here ! I suggest, for example, CALIOP on board the NASA/CNES CALIPSO platform.

We changed the naming to the following format: “INSTRUMENT aboard Funding institution PLATFORM”.;

Page 16: Lines 333:

"...quality with.." ==> "..quality using.."

This was corrected according to suggestion.

Page 18: Fig. 11 Caption.

"..from 384 and 354nm spectral bands" ==> "derived using the 384 and 354nm spectral bands."

This was corrected according to suggestion.

Page 18: Line 345

"This allows for the rejection of the low.…"

Changed to “This allows for the rejection of the bins with low signal.”

Page 19. Fig 1 and Section 4.2 in general.

The figure is fine. However, it would be useful to also present the retrieved extinction as well as the Aeolus observed attenuated Mie and Rayleigh backscatter images. The absence of such images is conspicuous.

We added figure with the L1B useful signals and another figure with the attenuated backscatters.

Page 19, Lines 350-360.

There is a well-established relationship between the linear depolarization ratio and circular depolarization ratio that should hold for must circumstances. Given this it would be useful for the authors to give a quantitative number for the expected impact of the depolarization on the Aeolus measured lidar-ratio.

We improved the description of results, discussed the values and cited Wandinger et al. 2015.

Section 5:

Join the first two paragraphs.

Done

Can you please provide more detail connected with the points being made here ? For example:

-What type of new algorithms are being developed ?

We added a reference to:

Ehlers, F., Flament, T., Dabas, A., Trapon, D., Lacour, A., Baars, H., and Straume-Lindner, A. G.: Optimization of Aeolus Optical Prop-480erties Products by Maximum-Likelihood Estimation, preprint, Aerosols/Remote Sensing/Data Processing and Information Retrieval,https://doi.org/10.5194/amt-2021-212, https://amt.copernicus.org/preprints/amt-2021-212/, 2021b

and mentioned the work of the EarthCARE team to adapt their algorithms to Aeolus data.

-Can you at least give a reference to the assimilation work ?

We added one

Letertre-Danczak, J., Benedetti, A., Vasiljevic, D., Dabas, A., Flament, T., Trapon, D., and Mona, L.: Aerosol Assimilation of lidar data from505Satellite (AEOLUS) and Ground-based (EARLINET) instruments in COMPO-IFS., other, pico, https://doi.org/10.5194/egusphere-egu21-4799, https://meetingorganizer.copernicus.org/EGU21/EGU21-4799.html, 2021