Atmospheric Measurement Techniques Discussions

Atmos. Meas. Tech. Discuss., referee comment RC2
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Comment on amt-2021-181

Anonymous Referee #2

Referee 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-RC2, 2021

The paper describes in a short and concise way the algorithms for the retrievals of the aerosol and cloud product from Aeolus. It is thus very informative and valuable for the scientific community and therefore, in principle, well suited for publication in AMT.

However, I have some major concerns which need to be addressed before the paper can be published. The authors have done great work in developing the algorithms for Aeolus and updating calibration schemes, but the current presentation style of the paper needs to be clearly improved otherwise it is not understandable and thus publishable. Therefore, most of my comments are with respect to that topic.

General remarks:

The paper tries to make a compromise between extended algorithm description and concise information. This, however, was not successful all time. Especially, a lot of “Aeolus internal“ language is used, which is not understandable for an external readers. Some examples are given below, but please check that everything is explained and clearly references so that a person with no access to the internal ESA pages can understand everything.

Furthermore, the main ESA documents which the authors reference on (e.g. the ATBD) should be made available in a sustainable way. Currently they are published on an ESA webpage, but who knows if this is still the case in 1 or 2 years. Thus, please either put this important information on a repository where you can obtain a DOI (e.g. zendo) OR submit it as supplementary material.

Furthermore, while progressing with the reading the paper, the language style gets more and more sloppy and clearly needs to be improved (to be honest, one has the feeling it was submitted before it was really finalized, i.e. some sections are still in a draft-stage). E.g., the current conclusion is not sufficient and not appropriate for a journal like AMT. Also, the language itself is partly not

scientific and still a lot of typos exist. Thus, this should be improved during the revision or language editing should be made by Copernicus.

In general section 4.1. and 4.2 has to be overworked. The explanations are partly insufficient and one has to guess many times what is meant…..

References: The references given in the introduction and the paper are not up to date (one has the feeling the list is 2-3 years old). Meanwhile, some papers have been published, also dedicated to aerosol and cloud products, and should be mentioned. Some examples are given in the specific comments.

Figures: Please explain each Figure you use and what can be seen in this Figure. Currently, very often you draw conclusion from a Figure, but for an external reader it is not comprehensible/understandable because it is not sufficiently explained what is shown in the Figures. In principle, you need to explain each Figure in the text, and additional as a self-standing description in the caption. So that one could understand the Figure from reading the caption only, but also from reading the text only. Furthermore, please check which Figures you really need to make you message: “Illustrations should only be shown if they are necessary for the understanding of the paper, not because they have been created. “

Specific comments:

Line 26: “molecular photons“ do not exist, I guess you mean photons backscatter by molecules.

Corrected

Line 32: delete IN (after Calipso)

Done

Introduction in general. Please review the current status of Aeolus (space lidar) related literature and add the important most recent references.

Lind 35: Wrong naming in reference, it is OMAR et al and not Ali et al.

Corrected

line 41: add “and“ before nature

Done

Line 52: Sustainable source for Flamant 2021 needed.
The document was added to a specific documentation page for the L2A on the ESA Aeolus page. A request for a DOI was addressed to ESA.

Line 66: Add “Fig.“ before 1 and 2. (do you need these figures?) added “Fig.”. We thought these figures would be useful to underline Aeolus particular slant looking geometry and the range bins.

Line 79: “the top-most Rayleigh bin that must be above the top-most Mie bins“ Please explain why and/or give reference.

This is a built-in technical constraint of the hardware, which is now explained in the text. References are only ESA internal documents.

84: “The shape of the optical filters is drawn in Fig. 3.”. Language! What is shown in Figure 3 are the transmission curves for the different channels/filters.

Corrected

86: “ The figure shows that the Mie peak in the spectrum is significantly filtered out by the dual Fabry-Perot as it stands half-way between the peak transmissions of Fabry-Perot A and B, where the sum of the two transmissions reaches a local minimum.” I assume that you reference now to the right panel of Figure 3. I took me quite some while to understand what you have written. You need to rewrite this paragraph adding more explanation. Please guide the reader to what is the “Mie peak”. Explain all abbreviations in the Figures (e.g. what is TA and TB?) and use the line colour and style when referring to a specific curve to help the reader understanding.

The explanation on this figure has been expanded and the figure legends changed.

90: “Overall, the efficiency of the Rayleigh detection chain for the photons backscattered by particles is about 50% of what it is for molecular photons, while it is 130% through the Mie detection chain.” The current phrasing is very, very hard to understand. Please try to rephrase to make it clearer for the reader.

We tried to rephrase these paragraphs to make them easily understandable. We also give a figure with the actual C coefficients to illustrate this paragraph.

Figure 3: y-Axes caption on the right column missing. X-axis caption: What is 0? This is never explained. I now it is the difference to the emission frequency but you need to state this. Thus, it is not the frequency but the frequency difference/shift.

The frequency offset is now explained in the text is corrected on the figure. The y-axes are spectral power distribution, in arbitrary units.

Line 99: L1B never explained. What is this, any reference? I think all, the L1B and L2A ATBD and product description documents need to be published with a DOI from the current versions.

We added a short explanation of what the L1B input data that we use. Having a DOI would be good indeed. We asked ESA to consider this in the future. The L2A documents were also provided on a specific page, other than the initially referenced “announcement of opportunity” page.

106: L1B derived scattering ratio is defined by the physical quantities. But how can this be done at L1B level?

See response to the above comment.

107: Which pre-defined lidar ratio is used? - please state this here.

The backscatter-to-extinction ratio used in the L2A was added.

112: Please explain SNR and also please explain what “high” means.

We added an explanation:

“...with large enough Signal to Noise Ratio (SNR) within one BRC before applying the SCA. The threshold is set at a SNR of 3.5 in Baseline 12.”

112: Is the group product really limited to one BRC? I thought features are grouped on the basis if “measurements” independent of the BRC.

Groups are processed in each BRC separately. It comes with the limitation that no group can be larger than one BRC and no group can spread across the border between two BRCs.

122: S_rie--> S_Mie

Corrected

130: add “and” after the formula

Done

133ff: As C1 to C4 are fundamental coefficients, the short explanation is not sufficient to me (as the reference given is a zip file only and no peer-reviewed document). Especially the adhoc calibration procedure (line 137) needs a short and concise explanation here. Also the uncertainties related to that should be briefly discussed: E.g. how good are your C1 to C4 determined and what happens if the calibration fails.

A short description of how the coefficients are derived is now provided.

Errors on C and K coefficients are not accounted for in our error estimation. This is a flaw, as our models are not as precise as it was hoped before launch. We added the following discussion on errors:

“Currently, the error on the calibration coefficients is not accounted for in the error estimates that are derived below. The models used in the calibration are not able to describe imperfections of the instrument that were discovered in flight. In particular, the C_3 coefficient is derived from simulated transmission curves, and comparison of transmissions and simulations on the internal path show discrepancies of up to 20 %. The actual transmission of the Fizeau interferometer for a light beam backscattered from the atmospheric (the "atmospheric path") is difficult to calibrate. This improved calibration and the inclusion of calibration errors into the error estimates could be addressed in future versions.”

144: I guess you invert not only Eq. 2. but also Eq. 1? At least id did not understand how to achieve 7 and 8 by using only Eq. 2. Please also write inverted “to”…..

We changed the sentence to:

“In the first operation of the SCA, for each bin i, equations (1) and (2) are inverted to:”

Eq. 7 and 8: C_3 subscript for “3” missing

corrected

155: maybe rephrase to “the assumption is made that within the first bin no particles exist”.

We changed the sentence to:

“the assumption is made that there are no particles within the first bin and hence ...”

159: Eq 12: ß_m needs subscript sim as well?

Yes, we added the subscript

Line 167: What is x – it's not explained. In general formula 14 is hard to understand. Do the brackets behind H^-1 correspond to x? That is unclear for me. Probably it would be better to explain this formula in two steps or you simply do not use "H".

We need to use H^{-1} because there is no formula to describe this function. In the software, H is being inverted numerically. "x" is just a variable to explain what function H is (In France it is rather usual to call "x" any scalar variable, that we can use to describe a function, sorry if that was not clear). We rewrote this part as follows.

"Where H is the function: $x \rightarrow H(x) = \frac{1-e^{-x}}{x}$ "

Figure 4 is never referenced. I think this should be done as it is very interesting.

Figure 4 is now referenced in the text.

169: For my own interest: Did you ever estimate to what extent possible extinction above the first bin could influence the SCA? Is the normalization procedure probably more prone to produce high errors than a possible extinction above the first bin?

The normalization is here to get rid of this effect. As you noticed, however, the normalization is very prone to errors because of the low level of signal in the uppermost bin. We did not consider to change this part of the algorithm.

The future Maximum Likelihood Estimation algorithm will allow to determine the optical depth between the satellite and the first bin. This is something that we will be able to study more in depth in the future.

180: Please explain "M1". E.g.: "...the primary mirror (called M1)". But probably this is not needed. Do you have by the way any reference for that statement? E.g. during last ILRC, a lot of Aeolus presentations were made:

https://www.epj-conferences.org/articles/epjconf/abs/2020/13/contents/contents.html or even that manuscript: https://amt.copernicus.org/preprints/amt-2021-171/ ?

We added a reference to Weiler et al. 2021.

188: Does it impact the retrieval or the calibration?

We rephrased to:

"This change in the instrument characteristics would require a constant recalibration. In the absence of such a correction, both the winds (Weiler et al., 2021) and the aerosol optical properties retrieval would be impacted."

Line 190: Do you have any reference how "clear sky" is defined? I.e. which measures do you apply?

We added a description of the "clear sky" selection method that we apply.

Line 196/197: What is meant with step 2 in Figure 5? Is is not clear for me. More explanation is needed here.

We replaced "step 2" by "the second row".

Fig. 5: I think Fig 5, left is not useful to understand the paper. If you want to keep it, it must be enlarged, colour scale must be changed and much more explained. But from my point of view, Fig. 4 and Fig 5. (right) are sufficient. Nevertheless, it is surprising to see a big difference in Fig 5. left for the orbit averaged k and the M1 fitted K, while in the distributions on the right side it is obviously not the case. Can you explain? Virtually you have much more values around or above 4 for the orbit averaged K but this is not seen in the distribution (right panels).

We replotted figure 5 with a broader range of values but there is no visible difference between the 2nd and 3rd distributions. We are still wondering why the "recalibration" option produce such broad distribution of errors.

We would like to keep the left panels as they give a different on the correction. We described them better.

206: please explain what "distribution" you mean. I guess you refer to Fig. 5 right, but you need to explain what is shown there.

We added: "on the right panel".

211: "The fit being made…..": which fit? It is unclear for externals what is meant with all this. Please explain more solid in scientific language.

We rephrased:

"The fit of the signal to the M1 temperatures is made over a full "orbit file", which is at least half an orbit long and most of the time longer, this is enough to guarantee that a high reaching particulate feature in a given region would not bias the fit too much."

216: "L1B derived scattering ratio": Never explained. What is this?

Eq. 16: What is roh_L1B,I ? Is this the scattering ratio. What is the difference between a real scattering ratio and the L1B derived one. Need to be explained.

We introduced the L1B product in a dedicated section. This was indeed missing.

224: Tm,sat,i-1 is not in Eq. 17. Please correct.

"sat" subscript was added for both Tm and Tp, as it should have been.

228: Which lidar ratio did you use and why? This is an essential information.

We added:

"… under-estimated due to the fixed lidar ratio of ~14 (1/0.07) used by the MCA (see Fig ..."

228: How one can see a dust plume? Please indicate in Fig. 6 and maybe also put geophysical coordinates to Fig. 6 (as for the Calipso image or Fig.12).

We added geographical coordinates to figure 6.

Fig. 6: The size of this Figure is good as well as the color scale. Some more explanation is needed in the text: what are the white areas, why do the top of the profiles changes. Where has this curtain been made, etc……

On top of adding the coordinates along the curtain, we added the following paragraph:

As explained in \ref{aladin_and_aerosol}, the Range Bin Setting (RBS) is changing along the orbit in order to find a compromise between the highest sampled altitude and the resolution. This is visible in the big steps in the maximum altitude of the profile. Smaller steps are due to the terrain following capability(e.g. towards the end of the the orbit), that shifts the RBS to reduce the amount of data acquired below the ground and extend the profile higher up. Some parts of the profiles, especially in the lower atmosphere are not processed (white pixels). This happens when the measured Mie signal becomes negative, often below thick clouds.

231-239: I am puzzled how I should deal with this information. So the ICA is kept in the data for historical reasons. But no development have been made. What does it mean? Shall I neglect the ICA? Same for the group product. A clear statement would be desirable. Or do not describe the ICA and group at all (maybe only in the introduction) as it is not used in your analysis. At least, in the current form it might be more confusing for the reader than providing valuable information.

This paragraph was removed and only mention these parts of the product in the introduction.

245: As far as I know, ALADIN is not linearly but circular polarized….thus there is also no parallel direction….

The explanation on the polarisation has been rewritten to give more details. This is not well known by the community and was pointed by the three reviewers, so we tried to make things clear.

245-247: Please put reference here. E.g. Ansmann et al., 2007; Flamant et al., 2008, or Baars et al. (2021).

This paragraph has been further developed and we added a reference to the ESA Aeolus “Science Report”:

“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. The UV laser beam is linearly polarized at the laser output. It goes through a quarter-wave plate (see Fig. 4.13 in (Science Report)) 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.”

255: Any reference for that statement that signals are weaker than expected as before launch? e.g.: Reitebuch,2020, ILRC or even directly in this special issue?

We added a reference to Reitebuch 2020.

268: “See Fig. 7.”: More explanation for Figure 7 needed. E.g. which plot in Fig. 7 is meant, what is shown there, etc. Just to refer to a Figure without any explanation what is shown there is not sufficient. Furthermore, many things shown in Fig. 7 are never discussed, e.g. backscatter *30sr….

We changed see Fig. 7 into:

“ This is illustrated in Fig.7, where a large extinction is found in the second bin. This produces a large attenuation on the expected molecular signal (red dashed line) which never becomes larger than observed molecular signal (yellow line).”

We also explained the “backscatter*30 sr” line:

“As an indication of the presence of particles, we also show the SCA backscatter scaled by an arbitrary lidar ratio (middle panel, red line). It shows that the extinction of the SCA (blue line) is detected one bin below the actual particle feature. The MCA extinction is quantitatively wrong because of the fixed lidar ratio, but is detected in the correct bin.”

274: Same as above but for Figure 8. E.g., Fig. 8, left is never referenced. And it is never explained what is seen there in general. Moreover, the panels should be enlarged to page width and been put over each other.

Fig. 8 was redrawn. We put panel on top of each other and added the mid-bin extinction for comparison.

Eq. 18: most of the quantities shown in this equation are not explained, thus one cannot follow the argumentation and understand the formula.

This is better explained now:

“where $\sigma_{L_{p,i}}$ is the standard deviation on particles optical depth in bin i, and $e_{X_{i}}$ is the error added by the observation $ X_i$ on top of the actual value $\overline X_i$, modeled as $X_i = \overline X_i * (1 + e_{X_i})$.”

Fig. 19: Same comment as for Eq. 18. Please discuss the equations or review if you really need to show them. I could not follow any of the argumentation from 277ff.

We added:

“This estimated standard deviation $\sigma_{L_{p,i}+L_{p,i+1}}$ is no longer linked to e_{X_1}, the error on X_1, but only to the error in the two bins that are combined to obtain the "mid-bin" value.”

285: If you “lose” vertical resolution but also gain errors, why to use this method? I guess you mean lose resolution and decrease errors?

We replaced “gain error” by “improvement in error”

296: two times “presented”, delete one of it.

Done

299: E2S never explained, please do so when introducing the end-to-end simulation. Furthermore, what is the difference of the 20 simulation? It is not written here. If they are produced from the same input scene they should deliver the same results unless you alter some parameter. Which ones? what was simulated?

We added “(E2S)” when we introduced the End-to-End Simulator, earlier in the section.

To explain our approach, we added: “ The noise generated in each simulation is different and this allows us to estimate the impact of noise separately from the potential biases of the algorithms”

301: “Most of the time, the backscatter and extinction coefficients are correctly derived” how is this seen? What are you looking at?

See comment about line 316

Figure 10 and discussion: The current Figure is hard to read. It is 16 panels with 6 curves each. Do you really need all panels to make your statement? Maybe show only the most important. Please also explain all abbreviations and formula symbols. Why do you use log-scale for the backscatter and extinction values?

See comment about line 316

Maybe you could start introducing the reader to these kind of Figures by grabbing one BRC and first explain in detail what is shown. Afterwards show the other BRC’s and do your interpretations. But currently you ask too much from the reader to understand what you see in these plots.

See comment about line 316

302: “i.e. errors lie within the range of atmospheric heterogeneity” – how is this heterogeneity determined. It is currently a statement without proof.

See comment about line 316

303: “backscatter coefficients are also mostly correct” what does this mean, where it can be seen?

See comment about line 316

304: “The average of the 20 simulation overlap the expected values with a low dispersion meaning that one realization should be enough to characterize the atmospheric optical properties.” I do not understand this sentence as I do not know to what you are referring to.

See comment about line 316

307: “In practice the vertical resolution of the bins is seldom below 500 m” has it ever be explained that the range-bin setting can be changed and is changed along one orbit? This is an important information….

We completed the corresponding paragraph in section 2.1. It now reads:

“Most of the time, the backscatter and extinction coefficients are correctly derived, which can be seen where the red line (20-run average retrieval for a given BRC) is close to the black line (E2S input averaged over the BRC). Error estimates for the extinction (brown range around the red line) lie within the range of atmospheric heterogeneity (thin black line, derived as the standard deviation of the E2S input over a given BRC). Backscatter coefficients are also mostly correct, although with a slight low bias, e.g. in BRC 5 between 14 and 10 km altitude (compare the red line to the input, black line). The average of the 20 simulation overlap the expected values with a low dispersion (brown-shaded area) meaning that one realization should be enough to characterize the atmospheric optical properties. ”

299-309: The paragraph should be overworked in general. For me it was hard to understand to what the authors refer to when making a statement.

We tried to better point at what to look at in the figure. The paragraph now reads:

“In order to study the sensitivity of the L2A product to noise, 20 independent E2S simulations are run from the same input scene. The noise generated in each simulation is different and, looking at the average retrieval and the standard deviation around it, we can estimate the impact of noise separately from the potential biases of the algorithms. Figure \ref{SCA_vs_E2S} presents how the backscatter and extinction coefficients derived from the SCA mid-bin algorithm compare with the E2S inputs.

Most of the time, the backscatter and extinction coefficients are correctly derived, which can be seen where the red line (20-run average retrieval for a given BRC) is close to the black line (E2S input averaged over the BRC). Error estimates for the extinction (brown range around the red line) lie within the range of atmospheric heterogeneity (thin black line, derived as the standard deviation of the E2S input over a given BRC). Backscatter coefficients are also mostly correct, although with a slight low bias, e.g. in BRC 5 between 14 and 10 km altitude (compare the red line to the input, black line). The average of the 20 simulation overlap the expected values with a low dispersion (brown-shaded area) meaning that one realization should be enough to characterize the atmospheric optical properties. “

316: “The estimated errors are also too low and do not cover the expected values”: How can I see that in the plot? Unclear for me.

The whole discussion on this figure has been reworked, with explanations on the figure itself and what to look at in the figure. We kept the figure as it was and we hope that the text is now enough to guide the reader through it.

319: “In this example,” which one?

We changed to “In Fig. 10”

320: It has been never explained what a “useful” signal is…..

The input data from the L1B is now introduced in a dedicated section.

322: “bias is then propagated up to the calculation of the backscatter” what does it mean: propagated up to? Please check if you can find a proper peer-reviewed reference for CALIOP, e.g. Winker 2009

The sentence was corrected: “The underestimation of the Mie SNR is then propagated through the derivation to the estimated variances of the backscatter and extinction.”

We added Winker, 2009

343: “determined by a threshold on the Mie SNR…” what is the threshold?

We rephrased the paragraph:

“The SCA lidar ratio has been processed from the mid-bin product and SNR based quality check (QC) flags have been applied. The backscatter coefficient retrieval is considered as valid in a specific bin if the Mie SNR is larger than 40 and the extinction coefficient is valid if the Rayleigh SNR in this bin is larger than 90. This allows for the rejection of the bins with low signal, for which background noise is large.”

345: “reject low signal bins” please improve phrasing, what are low signal bins, bins with low signal?

We changed the sentence to “This allows to reject bins with low signal, for which background noise is large.”

346: “L2A valid lidar ratio” what does valid mean? I guess you mean that you applied the validity flags?

We added the sentence:

“The lidar ratio depends on both extinction and backscatter values and the lidar ratio is valid where both extinction and backscatter are valid. ”

Figure 12: What you show is the co-polar lidar ratio, please indicate this in the Figure to avoid confusion. The blue and green frame is hardly seen. Can you use a different color?

We changed the color of the boxes.

347: “high lidar ratio values”…please indicate numbers – it is hard to see from the colors, e.g. 120 - 140 sr. In my opinion you always should state that “only” the co-polar lidar ratio is measured, otherwise readers only looking at the plots may be really confused why the lidar ratio in dust is 2-3 times higher than normal. And you also should state what (co-polar) lidar ratio one would expect in mineral dust. Otherwise the reader is left alone in interpreting if Aeolus L2A data is useful….

We wrote a discussion about the co-polar lidar ratio values and how they compare to the values in the cited literature.

350: You compare apples to peaches: Please state what lidar ratios (numbers) they have been measured (Mona and Nisanzi) and what you would expect for Aeolus taking into account the polar component. There was also a presentation by Wandinger showing that.

We now give values and compares them to the cited articles, using the formulas for circular depolarization ratio from Wandinger, 2015 (Aeolus Workshop):

“Following Wandinger et al. (2015), the co-polar lidar ratio in circular polarization could then be scaled with a factor $S_{co}=S*(1+2\delta_{lin}/(1-\delta_{lin}))$ and compared to other lidar measurements. Mona et al. (2012) report lidar ratios of 40 to 80 sr for mineral dust, with a most likely value around 55 sr at 355 nm, while Nisantzi et al. (2015) report a mean value of 53±3sr at 532 nm. We obtain values between 80 and 120 sr; using a rough estimation of the depolarization ratio at 0.3 from CALIPSO, the scaling factor would be 1.85. which would scale back our measurements to somewhere between 43 and 65.”

353: “A number of studies (Ansmann et al., 2003) have shown that light depolarization ratio of dust and marine particles mixture is significant.” I do not understand this sentence.

This sentence was removed.

358: In my opinion, you devalue Aeolus with no need. The lidar ratio is not overestimated taking into account the Aeolus capabilities. Even more, it is absolutely correct when considering the expectations, e.g., made in Wandinger et al. Thus, you might reconsider your statements here.

We rephrased this sentence.

362ff and Figure 14: Is in my opinion not needed. First, it is “only” model data and therefore only an indicator, second it does not provide any additional valuable information. Thus, consider to omit this. If you consider it as very important, than much more explanation is needed.

We removed this figure and the corresponding paragraph.

As written before, the conclusion seem to be unfinished (i.e. still in draft stage) and are not sufficient in the current form. Please revise.

We expanded the conclusion and provided references to others works related to the L2A (new algorithms, assimilation, validation studies)

References:

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