

Atmos. Meas. Tech. Discuss., author comment AC3
<https://doi.org/10.5194/amt-2021-96-AC3>, 2021
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Reply on RC3

Artem G. Feofilov et al.

Author comment on "Comparison of scattering ratio profiles retrieved from ALADIN/Aeolus and CALIOP/CALIPSO observations and preliminary estimates of cloud fraction profiles" by Artem G. Feofilov et al., Atmos. Meas. Tech. Discuss.,
<https://doi.org/10.5194/amt-2021-96-AC3>, 2021

We thank Reviewer #3 for his/her analysis and comments on the paper. The responses to major and minor comments are given below. We marked the reviewer's and the author's comments by "RC:" and "AC:", respectively.

Major comments

RC: The authors should state clearly in the title that this study is dedicated to cloud products only.

AC: The present version of the article puts more stress on the absolute values of scattering ratios themselves. In addition, we updated the title to "Comparison of scattering ratio profiles retrieved from ALADIN/Aeolus and CALIOP/CALIPSO observations and preliminary estimates of cloud fraction profiles"

RC: The study should include a quantification to some extent, and discussion, on the percentage of the clouds not detected from the 2 lidars with the methodology used. Additionally, a discussion is needed on the effect of these cloud-miss-detections on the results of the intercomparison per altitude (low, mid, high-level clouds).

AC: If we understand this question correctly, it is related to the evaluation of clouds in the GCMs, and this question has been already addressed in (Chepfer et al., 2008). For the current work, we are looking for similarities/differences in scattering ratio and cloud fraction profiles between the two lidar missions, if some clouds are filtered out in our approach, they are filtered out in the same way for both lidars.

RC: Although the title clearly states that this is a comparison of the scattering ratio products retrieved from the 2 systems, in the discussion throughout the paper the authors comments are attributed to the 2 systems only. It should be more clear that different approaches for cloud detection products from the 2 missions could lead to different results. See also specific comment below.

AC: We agree with the statement that different approaches for cloud detection products from the 2 missions could lead to different results. But, the idea of the paper was not to reconcile cloud product by "tweaking" the cloud detection algorithm, but rather to compare the fundamental differences. Therefore, here we used the same cloud detection

for the two system. We agree that after having fully understood and quantify the differences due to the 2 systems (like we try to do here), the future work will include the algorithm adaptation to retrieve the same clouds and to build a long-term cloud record. We added the corresponding text in the conclusion as an interesting and exciting outlook.

Specific comments

RC: Page 1, line 22: "the ALADIN product demonstrates lower sensitivity because of lower backscatter at 355 nm": This statement is not clear. The backscatter at 355 nm is not expected to be lower than at 532nm. Please explain and revise accordingly.

AC: This is an important comment made by all three reviewers. Indeed, there was a confusing explanation regarding the particulate backscatter and we apologize for this. As we wrote in response to the Reviewer #1's question, we meant the contribution of the particles to the total (particulate + molecular) signal. Even though the total backscatter is larger at 355nm, the particulate part can be buried in molecular return because the molecular backscatter is larger at 355nm while the backscatter from cloud particles is about the same. If the signal-to-noise ratio is small, then the cross-talk correction will be noisy and the particulate signal will be retrieved with large uncertainty.

RC: Page 2, line 43: "Despite an excellent daily coverage and daytime/nighttime observation capability (Menzel et al., 2016; Stubenrauch et al., 2017), the height uncertainty of the cloud products retrieved from the observations performed by these spaceborne instruments is limited by the width of their channels' contribution functions, which is on the order of hundreds of meters, and the vertical profile of the cloud cannot be retrieved with accuracy needed for climate feedback analysis." The sentence is confusing. Consider revising to make it easier to follow. Possible suggestion: "...is limited by the width of their channels' contribution functions (which is on the order of hundreds of meters), and their incapability to retrieve the vertical profile of the cloud with accuracy needed for climate feedback analysis.

AC: Thank you for this suggestion, we have simplified the text of this paragraph.

RC: Page 2, line 47: "This drawback is eliminated by active sounders, the very nature of which is based on altitude-resolved detection of backscattered radiation, and the vertical profiles of the cloud parameters are available from the CALIOP (Cloud-Aerosol Lidar with Orthogonal Polarization) lidar (Winker et al., 2003) and CloudSat radar (Stephens et al., 2002) since 2006, CATS (Cloud-Aerosol Transport System) lidar on-board ISS provided measurements for over 33 months starting from the beginning of 2015(McGill et al., 2015).": Too big sentence, difficult to read. Consider revising.

AC: We have simplified it, thanks.

RC: Page 4, line 106: "In Fig.1(a-c), we show the observation geometry and sampling of ALADIN's L2A product as well as three variables retrieved from its observations..": consider revising as: "...as three simulated variables that can be retrieved from its observations..".

AC: Since other Reviewers found this plot difficult to understand, we have replaced it with a 3D view of the orbits and observation geometries. Correspondingly, the description of Fig. 1 has changed.

RC: Page 4, line 106: "In Fig.1(a-c), we show the observation geometry and sampling of ALADIN's L2A product as well as three variables retrieved from its observations..":

consider revising as: "...as three simulated variables that can be retrieved from its observations..".

AC: Thank you for the suggestion, but in the new version of the manuscript we have a different Fig. 1 with a somewhat different discussion.

RC: Page 4, line 120: "The cloud variability along the satellite's track has been estimated from the gridded EAMv1 data using the parameterization of (Boutle et al., 2014). Figure1 also serves as an illustration to theoretically achievable cloud detection agreement discussed below.": Although the cloud variability is estimated, in the plot the scene is cloud free. As the paper mainly investigates clouds, it would be interesting to have a cloudy demonstration also in addition to Figure 1.

AC: Fig. 1 does not exist in its previous form anymore, but in any case, the scene was not cloud free. The horizontal structures with large ATB values corresponded to the clouds.

RC: Page 4, line 123: "...scattering ratio (SR)..": Please write how the scattering ratio is calculated.

AC: This is a good point. In the new version of the manuscript, we have a whole new section (Sect. 3) dedicated to the definitions and formalism.

RC: Page 4, line 124: "An important companion of such a column is a corresponding quality flag column,..... which can be then compared with that of CALIOP.": The description is vague, please write more clearly what filtering you used in the data.

AC: We have updated the text to "The important companions of these profiles are quality flag columns. For our analysis, we kept only the layers, which are marked either by a high Mie SNR flag or by high Rayleigh SNR flag, and by a flag indicating an absence of signal attenuation."

RC: Page 5, line 141: "Since the CALIOP is not a HSRL, the detailed information on AMB and APB is not available, and one has to compare the SR products.": One could also use the temperature and pressure profiles from NWP (provided with Aeolus & CALIPSO) to produce the particulate backscatter coefficient, and convert/compare these parameters. So this part should be revised to highlight the choice of this study and not state it as the only option.

AC: Thank you for this suggestion, that's exactly how it's done in the new version of the manuscript. There's a small correction, though – the molecular backscatter coefficient is recalculated using P/T profiles, and not the particulate one.

RC: Page 5, line 145-150: "The choice of the fitting parameter is not crucial for the purposes of the present work ... collocated data.": I strongly advise the authors to follow the comment of the first reviewer regarding the wavelength conversions. Alternatively, if they decide to keep the analysis as is, then please provide a detailed discussion on the uncertainties induced from this simplified conversion.

AC: For the new version we have updated the wavelength conversions and we discuss the uncertainties associated with it.

RC: Page 6, line 167: "To avoid the risks associated with the solar contamination, we picked up only the night-time cases": As Aeolus is in dusk-dawn, still variability is expected in the PBL with the CALIPSO nighttime observations above land. Can you comment on that in the manuscript?

AC: This is a valid point and, indeed, the diurnal cycle can spoil the comparison. Our answer is in our Fig. 3 (now Fig. 4), which estimates the diurnal effects along with the geometric and sampling differences. In addition, we rebuilt our new Fig. 5 (SR-height histograms) and Fig. 7 (cloud fraction profile per latitude) for the daily data without temporal difference filtering (these versions are not shown in the manuscript). In this approach, the diurnal effects are compensated because both local times are used for both instruments. Still, the SR-height histograms (Fig. 5) and cloud fraction profiles (Fig. 7) plots look about the same for this enhanced dataset as they do for a subset used in the manuscript, so one can conclude that the diurnal effects cannot explain the observed behavior.

RC: Page 6, line 172: "...we have performed a numerical experiment using the same calculated data as we used in Fig.1": Shouldn't they be stated as "simulations"?

AC: This is correct, but now we have a different Fig. 1 and a new section dedicated to the simulations, so this phrase does not exist anymore.

RC: Page 6, line 173 – 180: "This time... the passive observations": It is very hard to follow the approach. A scheme/flowchart would be useful

AC: We added a flowchart and we simplified the text, thanks for the suggestion.

RC: Page 6, line 182: "Overall, we considered about 1E5 pairs of pseudo-collocated data and we present the results of cloud detection in Fig.3": Please include also the region and season(s) used to produce these pseudo-collocated data, which represent the outputs of Figure 3.

AC: We have updated the text of the paragraph and added a flowchart (Fig. 3). Briefly, we used 15 simulated orbits of one day in autumn equinox that cover both hemispheres and give, therefore, a representative snapshot of various atmospheric scenarios.

RC: Page 6, line 184: "or each altitude bin, the cloud detection agreement is a ratio of a number of cases when both instruments have detected a cloud (SR>5)": Please elaborate this choice of cloud cut off (e.g. literature) and comment on the uncertainties on the cloud detection induced from this choice for different altitudes. Could you include in results (Figure 3) and discuss, the percentage of the clouds missed to be detected, from the 2 sensors in your simulation, with the presented methodology?

AC: As for the choice of cutoff, we'd like first to refer to our answers to Reviewer #1's questions and to the two definitions of SR existing in the community. Indeed, a threshold applied to the SR defined as in Eq. 2 of present version of the manuscript should be altitude-dependent. But, as it is shown in (Chepfer et al., 2008, 2013) a fixed threshold can be applied to a SR defined as in Eq. 3 of the manuscript to estimate the difference between the two lidars. Future work will include a more advanced cloud detection algorithm to build a long-term cloud record. But this will be a whole new study.

RC: Page 7, section 3.1. It should be stated clearly in the section that the discussion refers to the SR retrieved products used in this study from the 2 sensors. As for example, a study with the cloud statistics from the Atlid L2A and CALIPSO L2 backscatter coefficient product products may provide different results.

AC: This is true, we hope that the new title clarifies that point.

RC: Page 8, line 224: "In Appendix A, we demonstrate the correlation between individual pairs of CALIOP and ALADIN SR profiles; the conclusion of this exercise is that it justifies using Eq.1, but the uncertainties of the analysis do not allow to refine the conversion

coefficients". This statement is very strong. One could refine the conversion coefficients, independently of the uncertainties of the analysis. I support that the authors should formulate this statement to correctly reflect the choices and limitations.

AC: In the new version of the manuscript, we do not use Eq. 1 and we do not want to retrieve or validate its parameters anymore, so we do not seek to rebuild this plot.

RC: Page 8, line 229: "This observation gives a hint that the instrumental part provides the backscatter information sufficient for some cloud detection up to 20km, but the detection algorithm suppresses noisy solutions." This sentence is not clear. Please improve the phrasing.

AC: We added some explanations after this sentence.

RC: Page 8, line 246: "Below, we will also discuss the YES_YES statistics normalized to cloud amount, but at this point we also want to study the other cases, which cannot be normalized this way" Consider to improve the phrasing.

AC: We have rewritten this section.

RC: Page 9, line 283: "This exercise is not aimed at revealing any altitude offset in backscatter signal registration, because this part of experimental setup is robust in both instruments". Consider improving the phrasing.

AC: We have changed it to "We note that we are not looking for an altitude offset here. The altitude detection of both instruments is beyond question. Instead, we would like to check ..."

RC: Page 9, line 10: "For each local peak found, we have searched for a peak or for a maximal value of CALIOP's SR profile in the vicinity of ± 3 km from the peak height determined from ALADIN". Consider including the information that only the 82% of the clouds are used for this comparison (according to the statistics presented in line 296-297).

AC: We added the proposed information in the following form: "By imposing the ± 3 km search criteria, we filter out about 12% of the cases linked to natural variability, but at the same time we lower the rate of picking up the peak from a different cloud layer."

RC: Page 9, line 304: "As for the clouds between ~ 3 km and ~ 10 km height, the height sensitivity effects skew the effective cloud height detected by ALADIN downwards by 0.5–1.0km", It is not clear which are the high sensitivity effects between 3 to 10 km. Maybe the authors could summarize them in a sentence again here. Also, please comment to what extent could the actual 100-km-cloud-variability at these altitudes be responsible for the deviation in the altitudes seen by Aladin and Caliop in these altitudes. It is not clear if the authors point out on the Aeolus capability to detect the top of the cloud, on the SR methodology capability for the same, or on the effect of the natural variability between the 2 instruments on their products.

AC: We have updated the figure due to an improved recalculation of SR. The text has been updated, correspondingly. As for the possibility of 100km variability to be responsible for the observed shift, it is unlikely. The very nature of this variability is random and we do not expect it to have a bias. Moreover, the figure does not change that much if we loosen the collocation criteria thus adding even more random variability.

RC: Figure 1: "...ALADIN's observation paths for centers of averaged profiles ...": How they are averaged? In Aladin L2A resolution?

AC: We have a new version of Fig. 1 and the caption is now different, too.

RC: Figure 1: " This inclination is schematically shown as an inclined line lying in lidar curtain plane whereas the real projection to the same plane should be a vertical line": This part is hard to understand. Same comment for the part inside the manuscript.

AC: This figure has been replaced with a 3D view and the text has been modified correspondingly.

RC: Figure 2: Can the authors comment on the absence of collocated points between 0-60° lon at $\Delta\text{time} < 6\text{hrs}$?

AC: This is a good point. The problem is purely technical: in this part, the data at 6 h difference come from another day and our collocation used the same day files. The collocation procedure is already heavy enough on resources, so we opted out of reading the other day's files. Technically, this is possible, but practically we would get only ~10% more of the collocated cases in the geographic area, which is not crucial for the comparison.

RC: Figure 7: No data is difficult to be distinguished from the -2km color, both have dark purple. Consider changing the no data color.

AC: We have changed the no data color.

RC: Figure 9: Consider adding the colorbar here also in the upper panel. Additionally, consider stating what the error bars account for.

AC: We have merged old Fig. 8 and Fig. 9 to a new Fig. 10. Correspondingly, all color panels share now the same color bar. As for the error bars, they correspond to r.m.s. of 1-week chunks of analyzed altitude subsets.

RC: Figure A1: The red points are not scaled in the same frequency ranges as the occurrence

frequencies. Wouldn't that be better?

AC: This figure was removed from the new version of the manuscript.

Technical corrections

RC: Page 4, line 101: "According to Flamant et al. (2017)."

AC: Fixed.

RC: Page 6, line 182: "Ansmann et al. (2007)"

AC: We do not quote this work in this context anymore. Please, see the next-to-last answer to the Reviewer #1 comments.

RC: Page 7, line 195: "...between the two products.."

AC: This sentence has been rewritten

RC: Page 7, line 200: "..for the thw instruments"

AC: Fixed

RC: Page 7, line 203: "Analyzing the Fig. 4"

AC: Fixed

RC: Page 8, line 242: consider rephrasing to "from the sensitivity study.."

AC: This part has been rewritten

RC: Page 8, line 237: consider rephrasing to "..behavior of the SR cloud detection product agreement"

AC: We have updated the phrasing here.

RC: Figure 3: "...to the total number of simulations .."

AC: The whole caption of Fig. 3 (now Fig. 4) is different in the new version

RC: Figure 7: "...+-3km vertical vicinity..."

AC: Fixed, thanks.