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Reply on RC1

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.,
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We thank Reviewer #1 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.

General comments

First of all, we want to admit that a simplistic conversion of scattering ratios provided in the first version of the manuscript appeared to be a source of confusion for the reviewers and we apologize for this. Moreover, the reviews helped us to recall that there are two definitions of scattering ratio itself and even though they both are aimed at estimating the contributions of particulate and molecular components to the backscattered radiation, they are not the same. In the present version, we added a section with all necessary definitions and conversion formulae. This section also appears to be helpful in the discussion of the potential discrepancy sources. The collocated dataset has been reprocessed and the new scattering ratios at 532nm have been calculated and analyzed. Despite changes in wavelength conversion methodology, the results and conclusions did not change much. But, we noted a certain improvement of the overall agreement between the ALADIN and CALIPSO datasets (e.g. see the numbers representing the normalized cloud detection agreement at different heights).

Major comments

RC: The title does not reflect the content of the paper. In fact, the authors focus only on the cloud detection capability based on scattering ratios.

AC: The present version of the article puts more stress on the scattering ratios profiles. 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: Furthermore, the whole instruction deals only with clouds and not a single word about scattering ratios is written

AC: We now have a whole new section dedicated to definitions, including those of scattering ratios

RC: The scattering ratio which is the essential part of this manuscript has never been properly defined. According to the reference which is given, I assume that, "the ratio between the total backscatter by particles and molecules and the molecular backscatter" (according to Flamant, 2008) is meant, i.e. the ratio between the total backscatter (represented by particles and molecules) to the molecular backscatter.

AC: We agree that the scattering ratio was not properly defined in the previous version. Please, see the general comments above. Indeed, the quoted definition is what is used in ALADIN product, but a different definition is used in the literature for CALIPSO scattering ratio (as CALIPSO is not a HSRL lidar contrarily to ALADIN). A more sophisticated processing is needed than what was provided in the initial version of the manuscript, to convert the scattering ratio from ALADIN to a scattering ratio similar to CALIOP. We believe that this time both the definitions and the conversion are OK.

RC: The conversion the authors use to account for the different wavelengths of CALIOP and AEOLUS is poor. For example, I have made a sketch using an arbitrary atmospheric molecular backscatter coefficient profile and a height-constant particle backscatter coefficient (equal at both wavelengths) of $7e-6 m^{-1} sr^{-1}$ in order to obtain a scattering ratio at 532 nm shortly above 5 as given by the authors as detection threshold for clouds

AC: First of all, we'd like to thank the Reviewer #1 for his/her efforts to estimate the SRs and the applicability of thresholds. Second, we were not using the same definition of SR as the reviewer in the previous version of the manuscript. Please, read the Section 3 of the present version of the manuscript, which should clarify SR definition, the wavelength conversion and the cloud detection threshold.

RC: Despite all my own doubts concerning this conversion, the authors themselves state: "We would like to stress here that no linear scaling applied uniformly to SRs at all heights could change the ratio of high cloud detection frequency to low cloud detection frequency of ALADIN." Therefore, I wonder: Why they are doing so?

AC: In the present version of the manuscript, we apply a proper conversion to SR'_{532} and we discuss the potential sources of bias associated with the parameters of this conversion. We show that by adjusting the parameters of the conversion one can change the ratio between high- and low-level clouds, but there are physically defined limits for this "tweaking".

RC: The choice of this threshold $SR > 5$ is not clear to me and seems very arbitrary and without justification.

AC: First of all, we draw the Reviewers' attention to the fact that the threshold is applied to "CALIOP-like" SR and not to "ALADIN-like" one (please, see Section 3 for the definitions). Second, the threshold $SR > 5$ is used in CALIPSO-GOCCP product (Chepfer et al., 2008, 2013). It is derived from in depth analyses of the CALIPSO SNR in day time at vertical resolution 480m and horizontal resolution 330m, that has been defined within CFMIP for numerous scientific reasons. $SR > 5$ is the threshold value that avoids false cloud detection in day-time due to low SNR induced by solar photons. Even though we used the nighttime cases for CALIOP, ALADIN's observations are in the twilight zone, so we decided to keep this threshold and to apply it uniformly to both instruments at all latitudes and heights.

RC: What happens if this threshold changes?

AC: The impact of this threshold change is discussed in (Chepfer et al. 2013) for CALIPSO. As for the present manuscript, we discussed the redistribution of the YES_YES, YES_NO and NO_YES cases with respect to threshold value in lines 269-274 of the previous version

and we updated this discussion in Section 5.3 of the present version. Briefly, a uniform increase or decrease of the threshold for both SR products will not change the ratio between the ALADIN and CALIOP clouds because both will decrease or increase simultaneously. At the same time, a technical adjustment of the threshold for ALADIN's SR_532 could improve the agreement between the datasets, but there's a tradeoff between the YES_YES and NO_YES cases: by increasing the threshold we reduce the number of unexplained (see the text) NO_YES cases, but we reduce the number of good YES_YES cases. By lowering the threshold, we reduce the number of YES_NO cases, but we increase the number of NO_YES cases, a part of which is already difficult to explain. Nevertheless, the new plot with zonal cloud fractions (Fig. 7) looks promising.

RC: The different vertical resolution for Aeolus and Calipso is not sufficiently discussed

AC: In Section 3.1 and 3.2 of the present version that correspond to Sections 2.1 and 2.2 of the original one, we provide the information about the sampling of the instruments and about the resolution of the products used in collocation. Moreover, we apply the same cloud detection thresholds, on both SR(z)_CALIOP and SR(z)_ALADIN at the same vertical and horizontal resolutions.

RC: Language and phrasing need to be improved. It is hardly understandable and not well explained. Please use simple sentences.

AC: The text has been simplified and proof-read by a professional. We hope that this has improved the readability of the article.

RC: Furthermore, "insider information of Aeolus" need to be explained otherwise it is not understandable for non-Aeolus experts.

AC: We have removed internal variable names from the text and rewritten some explanations related to Aeolus in Section 4.5.

Specific comments in addition to pdf

RC: Some statements are either simply wrong or wrongly phrased, e.g.: "...is characterized by lower sensitivity to high clouds above ~7 km than CALIOP, that we explain by lower SNR for ALADIN at these heights that is due both to physical reasons (smaller backscatter at 355 nm)". Why should there be a smaller backscatter at 355 nm? This is in absolute contradiction to all my knowledge! The particle backscatter coefficient could be equal in clouds (Angström of 0), but the molecular backscatter coefficient is for sure higher (see plots) and thus the total backscatter is for sure also higher! Could you please comment?

AC: This statement is true and, indeed, the phrasing was misleading, we apologize for that. 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. To avoid the confusion, in the present version of the manuscript we refer to the formalism defined in the second section and explain what we mean.

RC: Abstract: Just one of many examples: "(b) the cloud detection agreement is better for the lower layers. Above ~7 km, the ALADIN product demonstrates lower sensitivity because of lower backscatter at 355 nm" I do not understand this statement. First of all: What do you mean? The volume backscatter coefficient, the particle backscatter coefficient, the molecular backscatter coefficient? It is not clear! And I also do not know

why any of these should be lower at 355 nm compared to 532 nm (and 1064 nm)

AC: We have rewritten the abstract for clarification.

RC: Abstract last sentence: Is not understandable. What values are this? What is a cloud detection agreement value? Abstracts should be self-explaining and understandable.

AC: Thank you for pointing this out. We have added the definition to the abstract. Please, see new Section 3.5 for the details.

RC: Not all references are in alphabetical order

AC: Fixed, thanks.

RC: Some mistakes in the names of the references, please check

AC: Fixed, thanks.

For the rest of the reviewer's comments in PDF, please, see the attached file.

Please also note the supplement to this comment:

<https://amt.copernicus.org/preprints/amt-2021-96/amt-2021-96-AC1-supplement.pdf>