Response to reviewer and editor

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MS type: Research article
Title: Aerosol Optical Properties Measurement using the Orbiting High Spectral Resolution Lidar onboard DQ-1 Satellite: Retrieval and Validation
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Dear editor and David Winker:
We deeply appreciate your valuable time in reviewing our manuscript again and providing us with constructive feedback and suggestions. We carefully considered each comment and made revisions to our manuscript. We sincerely hope that the revised manuscript can be published in Atmospheric Measurement Technology.

General comments

I would like to thank the authors for making significant improvements to the first version of the manuscript. Several corrections and additional details are needed before publication though. Several of my comments below are related to the comparisons between DQ-1 and CALIPSO and the authors' speculation on reasons for differences between the retrievals.

Responses to general comments: We are very thankful to you for your kind words and positive feedback about our manuscript. Based on your comments, we have made revisions to the manuscript, especially regarding the comparison between CALIPSO and DQ-1.

Specific comments

Point 1: For aerosols, Equation 2.8 is a poor approximation of the correct relationship, which has a dependence on scattering ratio. The correct expression can be found in Tesche et al. JGR 2009 (Equation 11). Use of Eqn 2.8 will cause particulate depolarization to be underestimated for aerosols, because the scattering ratios are usually quite low. Cirrus have much higher scattering ratios and Eqn 2.8 can be a useful approximation.

The particulate depolarization of the dust layer in Fig 5e looks too small. Tesche et al. (2009) found a campaign-average particulate depolarization ratio of 30% for desert dust during the SAMUM campaign in the western Sahara desert. As mentioned above, use of Eq 2.8 to derive particulate depolarization from the measured volume depolarization underestimates the particulate depolarization.

Response 1: Thank you for raising this question. Regarding the depolarization ratio, we have updated our retrieval method according to the paper you provided. The particulate depolarization ratio in the manuscript have been modified as:

Line 175:

The particulate depolarization ratio is expressed as:

$$\delta_p(r) = \frac{\beta_m(r)[\delta(r) - \delta_m(r)] + \beta_a(r)\delta(r)[1 + \delta_m(r)]}{\beta_m(r)[\delta_m(r) - \delta(r)] + \beta_a(r)[1 + \delta_m(r)]}, (Tesche\ et\ al., 2009) \qquad (2.8)$$

Line 258: The retrieval results of the particulate depolarization ratio from CALIPSO exhibit a mean value of 0.3, which is consistent with DQ-1.

Figure 5e:

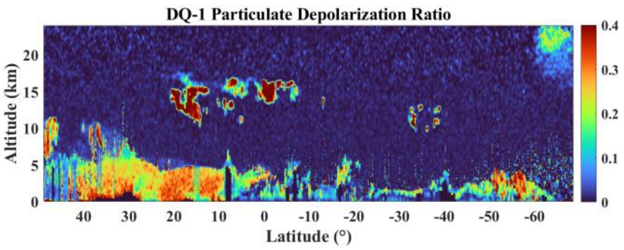
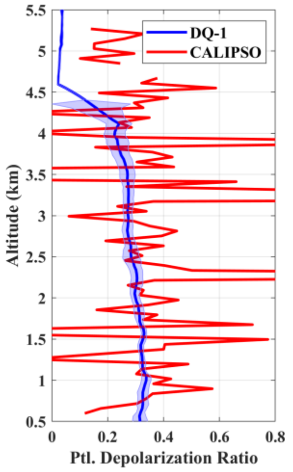


Figure 5i:



Point 2: Line 248 – there is a 400 km offset between the two instruments. The lack of cirrus at 15 km in Fig 5d is likely because there was no cirrus in the curtain observed by CALIOP. CALIOP can detect cirrus with extinction even less than 0.01 /km. I am confident the cirrus observed by DQ- 1 at 15 km in Fig 5c would have been detected if CALIPSO had been flying in the same orbit as DQ-1.

Response 2: Thank you for pointing out our negligence. The difference in the results of high-level cloud observations between the two systems is due to spatial differences. The corresponding modifications were made to the manuscript at:

Line 248: Due to spatial differences, the results of high-level clouds obtained by the two systems are different.

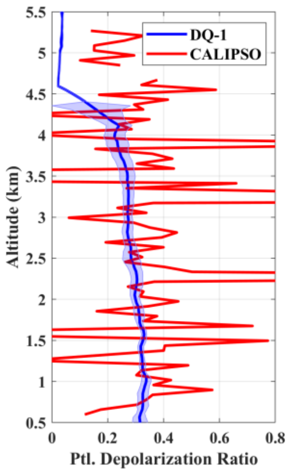
Point 3: How was the CALIPSO depolarization profile in Figure 5i derived? The data in Fig 5f looks too noisy to produce the smooth profile in Fig 5i. My analysis of the CALIPSO particulate depolarization profile within the dust layer between 24N and 30N showed the profile of depolarization is quite noisy. I found the mean particulate depolarization was 25%.

Line 258 – when I look at this scene, I find some noise spikes giving particulate depolarization as high as 0.5 but mean values are less than 0.3.

Response 3: Thank you for raising this issue. The inconsistent results in Figure 5i were due to excessive use of moving averages, and we no longer perform moving average on the CALIPSO profile in Figure 5i. The average value of the modified CALIPSO depolarization ratio is 0.3, which is consistent with the results of DQ-1. The corresponding modifications were made to the manuscript at:

Line 258: The retrieval results of the particulate depolarization ratio from CALIPSO exhibit a mean value of 0.3, which is consistent with DQ-1.

Fig 5i:



Point 4: Lines 262-264 seem to attribute the difference in particulate depolarization seen in Fig 5e, 5f, and 5i to a difference in SNR between the two instruments. CALIOP and DQ-1 initially had similar 532 nm pulse energy (125 mJ). The pulse energy of the CALIOP laser decreased by 10% to 20% over the life of the mission, which decreased the SNR by 5-10%. This change in SNR would have only a small effect on retrievals and does not explain the differences seen here between DQ-1 and CALIOP data. In the standard CALIPSO data processing of the dust layer in this scene, CALIOP backscatter profile data is only averaged 1 km horizontally – much less than for DQ-1. This is likely the major reason that CALIOP data appears noisier. But the difference between the DQ-1 and CALIOP mean depolarization is probably due to the use of Eqn. 2.8 for DQ-1.

Response 4: Thank you for raising this question. As mentioned in the Point 1, the depolarization ratio of the particles we previously retrieved was biased towards smaller values. After using a new retrieval method to calculate, the depolarization ratios of the two systems showed better consistency, and the signal quality of CALIPSO is sufficient for accurate retrieval. The corresponding modifications are the same as the Response 1.

Point 5: Table 1. It is not clear what 'Retrieval result error' refers to. Retrieval of which product? Under what conditions? Retrieval error usually depends on SNR so varies with altitude and is different for day and night. The text should describe how this error was determined - from validation inter-comparisons?

Response 5: Thank you for raising this important issue and we are sorry for our negligence here. The inversion result error refers to a relative error of less than 15% when comparing the depolarization ratio and backscatter coefficient obtained from retrieval with other authoritative data products in low altitude below 6 km under nighttime conditions. The corresponding modifications were made to the manuscript at:

Table 1:

Parameter	Value
Laser Wavelength	532.245 nm
Laser Energy	≥120 mj for pulses A and B
Laser Frequency Stability	1MHz@10000s
Laser repetition frequency	40 Hz
Telescope aperture	1000 mm
Field of view	0.2 mrad
Broadband bandpass filter	0.45 nm
Narrowband FP filter	30 pm
HSRL filters	iodine vapor filter, 1110 line aerosol signal suppression ratio ≥ 25 dB
Overall optical efficiency (excluding iodine filter)	0.16 at parallel polarized channel 0.561 at perpendicular polarized channel 0.375 at high spectral resolution channel
Quantum efficiency of the detector	40%
Retrieval result error	15%*

*** The relative error between the DQ-1 retrieval results (backscatter coefficient and depolarization ratio) and other authoritative data products, at low altitudes below 6 km under nighttime conditions.**

Point 6: It is mentioned in the author's Response to Reviewers that the HSRL retrieves a molecular depolarization of 1% at high altitudes, but this not mentioned in the manuscript. A broadband measurement of molecular depolarization should give a value somewhat larger than 1%. The DQ-1 HSRL uses a narrowband FP filter with a bandwidth of 30 pm. In this case the molecular depolarization should be about 0.3% because the filter rejects the Raman scattering lines and only passes the central Cabannes line, which is less depolarized. Do the authors know the polarization purity of the transmitted beam?

Response 6: Thank you for raising this important question. In the Response letter (*https://doi.org/10.5194/amt-2023-219-AC3*), we did not consider the polarization purity of the transmitted beam when retrieving the depolarization ratio of high-altitude molecules. In fact, according to the research on DQ-1 ACDL laser conducted by the Shanghai Institute of Optics and Fine Mechanics (SIOM), the polarization of the transmitted beam is 0.5% (Chen et al., 2023). After considering the polarization purity of the transmitted beam, the mean depolarization ratio of high-altitude molecules obtained by our retrieval is 0.5%, which is close to the theoretical value. The remaining bias is caused by the influence of the ACDL optical system. Furthermore, the analysis of the depolarization ratio of high-altitude molecules has been added to the manuscript at:

Line 125: To verify the accuracy of the parallel and perpendicular channels, we retrieved the depolarization ratio of atmospheric molecules at high altitude. The results, shown in Figure 1, indicate that the depolarization ratio is less than 0.5%, which confirms the accuracy of the two optical channels.

Figure 1:

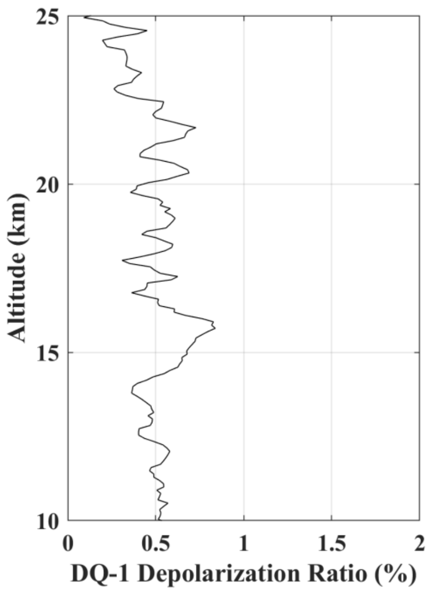


Figure 1 Retrieval results of depolarization ratio at high altitude.

Minor comments:

Point 7: Line 117 says the laser beam points off-zenith at 'a specific angle'. How far off zenith is the laser pointed and is the angle always the same, or has it changed during the DQ-1 mission?
Response 7: We are sorry for our negligence here. The laser beam deviates from the zenith at an angle of 2 degrees and remains unchanged under the control of the attitude control system. The corresponding modification was made to the manuscript at:

Line 117: The laser beam is off-zenith, pointing at an angle of 2 degrees and remains steady due to the attitude control system.

Point 8: Line 134-136: SNR is more difficult to estimate than signal magnitude, how is the threshold for 'SNR control' determined? Is the threshold actually defined in terms of SNR, or in terms of signal magnitude?

Response 8: Thank you for raising this issue and we are sorry for our negligence. As you mentioned, SNR is difficult to estimate, so we use the magnitude of the weak echo signal beneath the dense cloud cover to determine the threshold. The corresponding modification was made to the manuscript at:

Line 136: The threshold is determined by the magnitude of the weak echo signal beneath the dense cloud cover.

Point 9: Line 162-please define "S6 molecular model" or provide a reference
Response 9: We are sorry for our negligence. The corresponding references have been added to the manuscript at:

Line 162: The molecular backscatter coefficient and extinction coefficient are calculated by the S6 molecular model (Tenti et al., 1974) using the data of temperature and pressure provided by ERA5.

Point 10: Line 252-253 – the authors should be consistent in the units used for backscatter, choose either /km/sr or m/sr and use consistently throughout the paper.
Response 10: Thank you for raising this important issue and we are sorry for our negligence in our manuscript. In the revised manuscript, the unit has been uniformly changed to km/sr and double checked.

Minor corrections

Point 11:
Line 17 – 'prominently' is not the right word here; maybe 'clearly', or just say that retrieval algorithms and validation are necessary
Line 20 – 'showing' rather than 'describing'?
Line 80 – 'a' spaceborne HSRL sounds better here than 'the' spaceborne HSRL
Line 116 – 'The laser produces two …' rather than 'The laser is with two …'
Line 122 – 'high spectral resolution channels' rather than 'high spectral channels' ?
Line 207 – 'religions' should be 'regions'
Line 228 – stratocumulus clouds would not be found at an altitude of 15 km. These are likely some type of cirrus
Line 228 – rather than 'satellite's emitted laser', maybe 'laser return signal'
Response 11: Thanks for pointing out these minor corrections in our manuscript. The corresponding modifications were made to the manuscript at:

Line 17: Developing a suitable retrieval algorithm and validating retrieved results are necessary.

Line 20: The results have shown a continuous profile alignment between the two datasets, with DQ-1 showing an improved signal-to-noise ratio (SNR).

Line 80: Furthermore, under the leadership of NASA, the Atmosphere Observing System (AOS) international program analyzes the additional value provided by a spaceborne HSRL system.

Line 116: The laser produces two distinct pulses, pulse A and pulse B, to observe the atmosphere practically, both of the pulses are normalized prior to the retrieval process.

Line 122: The high spectral resolution channels function to separate Mie scattering and Rayleigh scattering in the signal, obtaining the molecular scattering profile.

Line 207: With more than 70 well-established observational stations worldwide, MPLNET has several underlying surface conditions, allowing it to collect ongoing aerosol vertical profiles in different regions.

Line 228: At an altitude of 15 km, the distribution of cirrus was observed, with the laser return signal failing to penetrate certain portions of the cloud cover.

Point 12: Line 20 – the meaning of 'continuous profile alinement' is not clear, does this mean the lidar profiles from the two instruments are similar (when the orbit tracks are close)?

Response 12: We are sorry for our wording here. What we want to express here is that the profiles from the two instruments are similar. We made modification to the sentence as:

Line 20: The results indicate that the profiles of the two datasets are in good agreement, with DQ-1 showing an improved signal-to-noise ratio (SNR).

Point 13: Lines 129-130 – Regarding "the filtered signal consists of no aerosol Mie scattering", I agree there is no significant aerosol signal visible in the filtered signal curve of Fig 1b but strong Mie scattering might result in significant leakage into the filtered signal. From the inset in Figure 1a, it looks like the unfiltered signal peak at 6 km altitude in Fig 1b will enhance the filtered Rayleigh signal by perhaps 20%.

Response 13: Thank you for raising this issue and we are sorry for our mistake here. The removal of all Mie scattering signals is an idealized assumption. In practice, a small portion of Mie scattering signals will remain. As you mentioned, these Mie signals hold no significance in the filtered signal. The revised sentence is:

Lines 129-130: Figure 1b shows the comparison of signals before and after filtering, with no significant aerosol Mie scattering signal in the filtered signal, presenting a residual portion of molecular Rayleigh scattering.

Point 14: Line 139 – Is 16.1 µsec the correct value? According to Dai et al. (2023), the time delay between pulses A and B is 200 µsec.

Response 14: We apologize for our mistake. After double-checking, we found that the correct time delay is 200 µs. The sentence have been modified accordingly at:

Line 139: There is an energy difference between laser pulses A and B, where the L2A data have been calibrated during the production, and the time delay of pulses A and B is 200 µs.

The above is the complete response to your comments. We look forward to hearing from you

regarding our responses. We would be glad to respond to any further questions and comments you may have.

Reference

Chen, W., Liu, J., Hou, X., Zang, H., Ma, X., Wan, Y., and Zhu, X.: Lidar Technology for Atmosphere Environment Monitoring Satellite, Aerospace Shanghai (Chinese & English), 40, 13-20, 10.19328/j.cnki.2096-8655.2023.03.002, 2023.