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.

Specific comments

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.

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.

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.

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.

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.

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 intercomparisons?

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?

Minor comments:

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?

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?

Line 162 – please define “S6 molecular model” or provide a reference

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

Minor corrections

Line 17 – ‘prominently’ is not the right word here; maybe ‘clearly’, or just say that retrieval algorithms and validation are necessary

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)?

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’ ?

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%.

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

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'

REFERENCES

Dai et al., 2023: Aerosols and Clouds data processing and optical properties retrieval algorithms for the spaceborne ACDL/DQ-1. https://doi.org/10.5194/egusphere-2023-2182

Tesche et al, 2009: "Vertically resolved separation of dust and smoke over Cape Verde using multiwavelength Raman and polarization lidars during Saharan Mineral Dust Experiment 2008" J. Geophys. Res. doi:10.1029/2009JD011862