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Discussions

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comment

Interactive comment on "The characterization of Taklamakan dust properties using a multi-wavelength Raman polarization lidar in Kashi, China" *by* Qiaoyun Hu et al.

Anonymous Referee #2

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General comments

This paper reports the lidar observations in Kashi, China located west of the Taklamakan Desert. The location is very interesting, and the quality of the observations with a multi-wavelength Raman lidar looks high, and consequently the results merit publication. However, the discussion on dust characterization in the present manuscript is only conceptual and very ambiguous. No strong conclusions are obtained. The authors discuss polluted dust cases, but the definition of polluted dust is not clear. The location of the observation is relatively clean except for desert dust. Is the polluted dust a mixture with anthropogenic air pollution? Is it external mixture or internal mixture?

Discussion paper

Probably, it would not be possible to characterize it only with lidar data. Variability of the characteristics of "pure" dust is not well understood. Also, optical parameters are dependent on particle size distribution even if the composition is the same.

The manuscript should be rewritten, in my opinion, with more focus on detailed comparison of the observed parameters (lidar ratio, particle depolarization ratio, Angstrom exponents) with previously reported results. The discussion with Table 3 in the present manuscript is not sufficient. Discussion on the change in optical characteristics by mixing with pollution should be given, if "polluted dust" is discussed.

Detailed comments

Line 19: T yr-1 -> Tg yr-1

Line 28-35: The authors should describe how the lidar data can be used as input and validation of models.

Line 75-77 "Moreover, there are populated cities in the neighboring countries such as Kyrgyzstan, Tajikistan and Pakistan. Under favorable meteorological conditions, various aerosol, for example, pollution, could be potentially transported to Kashi and mix with dust aerosols. ": This statement is not convincing, looking at the map.

Line 96-98: To my knowledge, the error analysis cannot be this simple. The error in extinction must be different from that in backscatter. Also, the error must be dependent on height and the background radiation. It should be mentioned that the Raman lidar measurement was limited in the nighttime, if so. In addition, it would be better to have some descriptions about the advantage of using rotational Raman instead of vibrational Raman at 532 nm.

Figure 3: The periods of Case1, 2, 3 and 4 should be indicated in Figure 3.

Figure 5: Case1, 2, 3 and 4 should be indicated in Figure 5.

Figure 3: Legend "500 nm" should be AOD (500 nm).

Line 166-168: The backscatter coefficient at 1064 nm below 1800 m should be indicated in Fig. 6.

Line 169-170: "EAE" and "BAE" are not defined.

Line 183-187: Is the description consistent with Figure 3?

Line 227-228: What is the "clear evidence of polluted dust"?

Line 229-232: The structure at around 2500 km is interesting and should be studied further. Is the type of dust (or "polluted" dust) the same in 1000-2200 m and 2400-2800 m or different? Why relative humidity was high in 2400-2800 m?

Figure 9: Captions for (c) and (d) are missing. The scales (color scale and vector) should be the same for all panels.