

Interactive comment on “Analysis of the vertical structure and size distribution of dust aerosols over the semi-arid region of the Loess Plateau in China” by B. Zhou et al.

Anonymous Referee #2

Received and published: 2 April 2012

The paper could provide a valuable contribution to desert dust characterization, in particular in view of the already existing large number of studies on Saharan dust. In particular the combination of lidar measurements and ground based in-situ measurements has a large potential to improve our present knowledge on dust particles. From this point of view, the paper can be published.

However, many of the different aspects are only discussed very briefly, and important information on the used data, the data evaluation, and the accuracy is missing. This makes it difficult to understand what the authors had really done, and to draw conclusions with respect to previous studies – for this purpose, e.g., the errors and the

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

representativeness of the data must be known. As a conclusion, I strongly recommend to add an in-depth discussion of all findings/statements/conclusions of the paper. E.g., from the lidar-researcher's point of view it would be nice to have intensive aerosol properties, e.g. the linear depolarization ratio (at 532 nm) of the particles and the color ratio (and lidar ratios); all with error bars. Only then it is possible to find out whether there are significant differences to Saharan dust particles (or previous studies on Asian dust), and to add another piece of information to a global aerosol climatology.

Some specific comments (see below) may help to improve the paper.

6114/14: "The number concentration decreases. . .increase". It would help to explain what the reference with respect to "decrease" and "increase" is. Standard conditions at the site?

6114/23: 20.95%,. . . two decimal places are certainly unrealistic! Throughout the paper!

6117/7: More details of the lidar system are required: e.g., overlap region, SNR. What is the SNR of signals from 24 km, can they be used? What is the reason for the low temporal resolution? Detail on the accuracy, the resolution, etc. of the particle sizer would help as well.

6117/17: Images of the instruments are not necessary. Anyway, they are too small for useful information.

6118/1: The brackets in "d(z)" can be omitted; same in Eq. (2).

6118/6: Maybe it is better to replace "aerosol" by "particles" (throughout the paper)

6118/9: It should be mentioned that S_2 is known.

6118/13: Why is the reference height lower in case of dust storms? One would expect that in case of a dust storm the troposphere is (entirely) filled with dust; so that below 10 km no aerosol-free altitude range can be found. Or is it a problem of the SNR? I

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

don't believe that a Rayleigh calibration is possible under these conditions.

6118/15: The lidar ratio is not related to the aerosol concentration!

6118/18: A lidar ratio of 20 sr is very low. Can this number be discussed in more detail? Maybe it would help to translate the main conclusion of Zhou et al. (in Chinese language).

6118/20: What is the “depolarization ratio”? Linear...? Volume or particle linear depolarization ratio? The definition given in the paper seems to be related to the volume linear depolarization ratio. However, the calibration constant has been forgotten; just the ratio of the two channels will not give the correct number (different sensitivity of the channels). The authors should add a paragraph how the polarimetric measurements of the lidar were calibrated! This governs the accuracy of the retrieved linear depolarization ratios.

6119/4: How is the mass of the particles determined?

6119/13: The authors mention 20 days of measurements during dust conditions. What is the reason for only selecting the three days 16-18 March 2010? At least a short summary how the other days compare to the selected days would be nice.

6120/2: Typo after “10 m s”.

6120/6: Please specify the “depolarization ratio” (throughout the paper).

6120/9: A “depolarization ratio” of up to 0.2 should be explained. What sort of particles are associated to these values? Are additional linear depolarization ratios from dust-free conditions available?

6120/11: What do “higher” color ratios mean? “higher” is not a precise statement, it could be 100!

6120/15: Figs. 4 and 3a should be very similar, as the only difference is the factor “lidar ratio”. However, they look quite different, in particular after 18:00 of the first day!

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

Interactive
Comment

What is the reason? Why is the signal almost totally attenuated above 1 km (Fig.3a), whereas there seem to be significant features in Fig. 3b and Fig. 3c at that time.

6120/19: It should be clarified that the AOD is the optical depth at 532 nm. Furthermore, it should be explained how the AOD is calculated from the lidar: minimum height (extrapolation to ground), what is maximum height?

6120/21: I don't see an AOD of 0.005 in Fig. 5! A discussion of the accuracy of the lidar derived AOD is missing (severe influence of the estimate of the lidar ratio!).

6121/2: In Fig. 5a there a negative extinction coefficients close to the surface. This seems to be an artifact due to the incomplete overlap. So, this height range should not be shown in the Figure.

6121/6: "The heights of the inflections...": I don't understand this sentence.

6121/16: The "error of 10%": how is this number derived? Is it just an estimate or from an elaborated error analysis?

6121/19: Typo: "Freudenthaler" is correct. A comparison with that study would clearly benefit from a precise error analysis provided by the authors. Thus, error bars should be included in Fig. 6.

6122/1: The time of the measurements shown in Fig. 7 should be given: is it a representative example? Is it the average over some time? What was the situation on different days/hours? Maybe, a larger set of size- and mass-distributions can be included to show the variability.

6122/20: Please indicate the size range associated to the given number- and mass-concentrations.

6123/6: It is not clear, how the percentages (88.83%,...) are calculated. Are they averages of a subset of the columns in Fig. 9? Please explain! All similar statements in the following should be discussed in view of the accuracy of these numbers and

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

whether differences are significant or not.

6123/10: “The decrease was particularly large for the moderate-mode ...numerous coarse...”. I don’t understand this sentence.

6123/20: The whole section 3.4 should be critically reviewed to make clear what the authors’ message is.

6124/3: “effective diameter” is not defined.

6124/4: The data set resulting in the scatter plot must be explained: are those measurements conducted every hour; during which time period?

6124/4: Fig. 11a shows a large number of points clearly below the 1:1 line. What is the reason for this?

6124/8: What is a “surface dust aerosol scattering coefficient”?

6124/11: That is the physical reason for the Gaussian shape of the curve?

6124/16: 20.95% + 83.88% are more than 100%.

6125/1: The last section is quite similar to the abstract. The paper would benefit from a revision of the conclusions (inclusion of new ideas, an outlook, a broader discussion of the limitations and benefits,...)

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 6113, 2012.

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)