

Interactive comment on “Aerosol direct radiative forcing during Sahara dust intrusions in the central Mediterranean” by M. R. Perrone et al.

Anonymous Referee #2

Received and published: 15 December 2009

General comment

I have severe problems with the contents of the paper, and to my opinion, the paper must be rejected. The main reason is that all the radiative transfer calculations and conclusions concerning dust-related (coarse mode fraction of aerosols) and anthropogenic-aerosol-related (fine mode fraction) effects on the radiation budget (direct aerosol effect on climate) are based on the fundamental assumption that the AERONET sun photometer products accurately describe the aerosol situation during Saharan dust outbreaks. This is by far not the case, and meanwhile well-known in the community.

AERONET derives particle size distributions from measured spectrally resolved aerosol optical properties. In the case of dust the particles are nonspherical, how-

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ever large efforts were undertaken to widely solve this problem (Dubovik, 2006, JGR). But a severe error source is left. In the inversion retrieval they use the constrain that particles larger than 15 micrometer are absent. With other words, the AERONET retrieval scheme cuts the size distribution at 15 microns, ignores particles larger than 15 microns. This is easily seen in Figures 3 and 5a of this paper. As a consequence of this neglecton, the remaining optical information in the measurement from the larger particles (larger than 15 microns, and these particles are present over Lecce) produces an artificial fine mode in the inversions (compensation effect). And this artificial fine mode is then often interpreted as urban haze fine mode, as in this paper.

So, to my opinion all the results are highly speculative, based on erroneous data, and as a reviewer I cannot accept papers that are not based on solid grounds.

The only way out is to present AERONET products by considering particle sizes at least up to 30-40 microns in the retrieval procedure (inversions).

Some further comments:

I found the paper very long, and the results (there are too many) are described just one after another. A clear summarizing, concluding discussion is missing.

In the conclusion section they state: . . .the paper highlights for the first time to the best of our knowledge, the significant role of anthropogenic particles during dust intrusions events in the Mediterranean. . . .

Especially this sentence finally forced me to make this clear statement above.

The authors use a depolarization ratio that will confuse many readers if they are familiar with depolarization ratios. Usually (90% of the lidar people) use the ratio of cross-polarized to parallel-polarized signal as depol- ratio. They use cross-polarized signal by total polarization preserving signal. Is that cross-polarized divided by parallel-polarized signal?

Now they present (volume?) depolarization ratios of 0.35 to 0.4 for dust. This means

for the dust-alone depol. ratios values above 0.5, this is much too high. What is wrong?

A rather long Table 1 is presented! Reason? Table 1 does not contain values for 22 July, listed in Table 2. Or do you mean 22 June in Table 2.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 22539, 2009.

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