Atmos. Chem. Phys. Discuss., 8, S8081–S8083, 2008
www.atmos-chem-phys-discuss.net/8/S8081/2008/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Effects of sea surface winds on marine aerosols characteristics and impacts on longwave radiative forcing over the Arabian Sea" *by* Vijayakumar S. Nair et al.

Anonymous Referee #1

Received and published: 8 October 2008

The authors present measurements of aerosol properties during a cruise in the Arabian Sea and analysis of these measurements. The data were well presented and the paper was clearly written.

However, I do not recommend that this paper be accepted by ACP. The main result, to my way of thinking, is that high values of aerosol optical depth (AOD), black carbon, and aerosol number concentration occurred near a large urban center on the coast at the beginning and end of the cruise, and much lower values occurred when the ship was far from the coast (although there was still a strong influence of continental aerosols at this location). Although the authors made a good presentation of the data,

I do not believe that it is necessary to show that higher values of AOD result from large quantities of urban aerosols; not every measurement, especially of relations that are well established, deserves a place in the permanent literature.

The wind speed range during the time series when the ship was away from the coast was quite low, and typically did not extend above 4 m/s - below the treshold for which sea salt aerosol production by breaking waves would be expected. As the aerosol occurring at these wind speeds would have been advected from other locations, it seems that little can be determined of any value from looking at the wind speed dependence over this range, and indeed the back-trajectories demonstrated that most of the time the air masses passed over or near the continent, and thus that the dominant aerosol influence was continental (the statement on p. 15865, lines 14-15 state nearly this same thought). The percentage of black carbon also showed this. I do not see that a time series of urban aerosol, with the amount being determined by small changes in previous wind history, leads to an increased understanding of atmospheric processes of sufficient importance to warrant publication.

The authors fitted their mass loading and aerosol optical depths to exponential functions of the wind speed, which is commonly done, but they make the mistake of assuming that fitting data to a given functional form is the same as the data being accurately represented by that functional form. For instance, the satatement on p. 15869, line 13 that the graph "reveals an exponential increase" is not correct; they plotted the data in Fig. 14 on a semilog plot and fitted a line to the data. The data themselves do not fit a line very well. The make a similar mistake on p. 15867, line 3, where they state the Eq. 1 (this should be equation 3) "suggests a linear increase in ln(tau) with U" whereas in fact it does more than suggest, it IS a linear increase. Whether or not the data follow this pattern is another issue, although the data in Fig. 9 show such a slight increase that a linear function of tau vs. U would be equally good. The range of values presented for b in Table 2 should put to rest any attempt to ascribe any meaningful value to this quantity.

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Some of the experimental procedures could be described a bit more; see p. 15859, lines 5-6. On line 11 of that page it was stated that the instrument was calibrated before the cruise, but it was not mentioned whether or not it was calibrated afterwards (I assume not). Was there an upper size limit of the aerosol that was sampled using the High Volume Sampler (line 27 of that page)? The determination of size distributions using the OPC (p. 15860, lines 10-11) requires additional assumptions to handle ambiguities caused by Mie resonances. These introduce uncertainties in the size distribution in the size range of interest, and need to be discussed.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 15855, 2008.

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Interactive Comment