

## ***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.**

**Vijayakumar S. Nair et al.**

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The authors would like to thank the referee for the review.

1. Thanks for the appreciation.
2. Unfortunately the referee is totally mistaken about the main result of our paper. The decrease in AOD or concentration from an urban centre towards far ocean as the ship moves off is not a new result and we also did not claim it to be. Such studies have been reported in the eighties and nineties of the last century by several investigators. We have clearly indicated in the paper, the days when the ship was in proximity to the mainland, and this data was excluded in the further analysis. What we addressed in this paper was, (i) the fairly large increase in AOD at the longer wavelengths, (ii) increase in

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the concentration of coarse mode aerosols, particularly in 0.5 to 3 micrometer regime (iii so that the total mass concentration increased significantly, though the total number did not conspicuously increase, because the increase was at larger sizes), iv) with no change in the BC concentration (means no increased advection from the mainland), all from independent measurements, at the time-series location, where the ship was confined to a very small oceanic region, quite far from the mainland. This increase was attributed to a nearly three-fold increase in the mean wind over oceans. This data (over a period of nearly a week) was used to parameterize the wind-speed dependence of aerosol characteristics. Subsequently, the impact of this increase in AOD and change in its spectral dependence on the longwave radiative forcing was estimated and it was compared with the corresponding change in short wave radiative forcing, leading to the inference that the increase in the longwave forcing significantly offsets the increase in short wave forcing. To the best of our knowledge from the open literature, such investigations are quite few, and virtually non-existing in the Arabian Sea/ equatorial Indian Ocean regions.

3. Here also we differ to the inference drawn by the reviewer. At the time series location, the wind speeds were low only in the initial part of the cruise, when all the aerosol parameters were also low. Subsequently from 28th April, there was a steady increase in several, independently measured aerosol parameters and this time the wind speed increased significantly, with the 20 min averages going as high as 8.2 m s<sup>-1</sup>, and the instantaneous values going much more higher on several occasions. As the stress produced by the wind on the water surface increases nearly as the square of the wind speed, this increase results in substantial increase in the white capping and bubble bursting, leading the observed changes. It may be kept in mind that though the increase in concentration associated with wind-production of sea salt aerosols is almost instantaneous, the decrease during the following calmer periods, will be considerably delayed, because of the finite residence time of the air-borne sea salt. This was occurring not at the urban region as the reviewer states, but more than 400 km away, in relatively far oceans, which is also justified by the very low values of BC.

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4.This comment is mainly due to the difference in the way the English language is used in different parts of the world. Yes, we too agree that the graph is more than suggestive; it is rather conforming to the exponential form. This can be corrected in the revised version.

5.This suggestion is appreciated. Yes, the instrument was calibrated after the cruise also, and it was a slip in the paper, not mentioning it. Upper size cut off of HVS is ~10 micrometer. These and the suggestions about the OPC could be incorporated at the revision stage.

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

**ACPD**

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