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Interactive Comment

Interactive comment on "Examination of aerosol distributions and radiative effects over the Bay of Bengal and the Arabian Sea region during ICARB using satellite data and a general circulation model" *by* R. Cherian et al.

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We thank the reviewer for the assessment of our study. We do not agree with the reviewer conclusion about the scientific findings from our present study. Nevertheless, we think the criticisms have helped a lot to improve the formulation and explanation of our main findings.

General comments: The authors present an interesting summary of measured (sun-photometer from a ship), retrieved (Aqua and Terra MODIS), and modeled (ECHAM-

HAM) aerosol properties. I think the paper is a good technical report of findings during ICARB, but I am not clear how this advances the scientific understanding of either aerosol properties or the simulation of aerosol loading in the region near India. I think, therefore, that the paper should be rejected. I discuss my reasons below.

Reply: In disagreement with the Reviewer's opinion, we are convinced that the present work contributes in a significant way to the scientific understanding of aerosol abundance and effects in the Indian ocean region. In the last part of the Introduction section we highlight the special aspects of the present study, including the following:

a) Regional and seasonal representativeness of observations and model predictions:

So far, studies from the ICARB campaign have used the observational data sets along the cruise track to study AOD and radiative forcing (Kedia and Ramachandran, 2008; Kedia et al., 2010; Moorthy et al., 2009), believed to be representative of the region and season. The question of regional representativeness was investigated through observations and model predictions at point locations along the ship track and over the entire region. By statistical comparison of model predictions with ship-based observations and large-scale satellite data sets was used to gain a two-way confidence, which established the representativeness of the model as a tool for regional aerosol abundance analysis. (See manuscript page 19, line 1-7).

b) Integration of model and observations for a robust regional forcing estimate:

It should be noted that ECHAM5.5-HAM includes the most advanced representations of the aerosol size distribution specifically allowing coagulation within the fine (accumulation) and coarse mode aerosols and between the modes (Stier et al., 2005). For example, this allows both fine and coarse mode dust coagulation with BC, OC and sulphate. Further, there is a combination of internal and external mixing state among species which is physically realistic. The close correspondence between the model simulated and the measured (Nair et al., 2008; Kedia et al., 2010) single scattering albedo value indicate that the representation of chemical composition and mixing state

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in the model is likely to be valid over BoB and AS region.

The model derived (direct) aerosol radiative forcing has been constrained using the measured AOD (using both the sun photometer and MODIS). This study provides a robust (measurement constrained) knowledge of the regionally and seasonally representative aerosol radiative forcing over BoB and AS. (See the manuscript page 21, line 1-22)

1. The authors conclude that underestimates in model AOD are due to poor simulation of dust, but this conclusion highlights a problem that should be tested with respect to the ICARB measurements. What changes to the dust parameterization in ECHAM-HAM would minimize the discrepancies? What about the wind fields? Are the changes in the parameterizations within uncertainties?

Reply: Indeed, it would be very valuable to get further insights into the specific parameterization deficits. We added a substantially revised section to the manuscript, on the causes of low predicted dust aerosol. However, specific parameterization modifications are beyond the scope of the present study. The following text has been added:

"The primary factors governing the magnitude and geographical location of dust emissions are the soil erodibility factor and wetness of the surface (soil moisture) treatment in dust parameterisations. In the current dust emission scheme of ECHAM5.5-HAM, global soil textures were prescribed for preferential source regions (Tegen et al., 2002), which results in a significant underestimate of the dust emission flux over Asian regions (Cheng et al., 2008). Alternate approaches, e.g. reflectivity-based soil erodibility factor (Grini et al., 2005), which show better agreement of dust flux with measurements, might improve dust parameterization in ECHAM5.5-HAM. Over arid regions, dust flux is sensitive to wetness of the surface (soil moisture), which is influenced by the amount of regional rainfall. The time required by the soil to dry after a small rainfall depends on air temperature, humidity, surface winds, and soil texture. Currently prescribed soil textures (Tegen et al., 2002) would not accurately reflect these processes. Another

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approaches, e.g. the parameterization of the soil moisture effect into the ECHAM5.5-HAM dust emission scheme (Cheng et al., 2008), which show improved dust emission in east Asia, might improve the dust emission flux over Thar desert region. Wind speed is another key variable that influences desert dust lifting and transport. The model simulated surface wind speed over the Thar Desert is found to be similar during missing dust source days compared to normal days suggesting little evidence of anomalously low wind speed influencing suppressed dust activity (Fig. 4). Other approaches (Grini et al., 2005), which represent sub grid wind speed variability as a probability density function (PDF), predict dust better over dust source-regions, but show anomalous increases in dust emissions in non-desert regions at low mean wind speeds (Grini et al., 2005). In the current scheme super-coarse mode dust particles are neglected in the emission calculation based on the assumption that they settle down very quickly because of large masses. This assumption also possibly contributes to the underestimation of dust emission when gusty wind prevails over these regions (Cheng et al., 2008)." (See manuscript page 17, line 8-23 and page 18, line 1-8)

2. Similarly, aerosol swelling could be an interesting question to explore from the modeling perspective. Could changes in f(RH) improve the disagreement in cases which are thought to be impacted by aerosol swelling?

Reply: We agree with the reviewer that changes in f(RH) may help to improve the consistency between model and remote sensing estimates. Actual model modifications, though, are beyond the scope of this paper. Rather, we added the following discussion to the manuscript:

"The inconsistency between model and remote sensing estimates mainly arises from uncertainties associated with satellite retrievals (e.g. uncertainties with cloud contamination or surface albedo) and from the inaccuracies in the model representation of aerosol and cloud processes. Other reasons are associated with relative humidity (RH). In this regard, though, a recent comparison of aerosol swelling in terms of the statistical relationship between cloud cover and AOD found a good global agreement

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between ECHAM5-HAM and MODIS satellite retrievals (Quaas et al., 2010). However, the way aerosol water uptake treated in the model by using a single humidity value rather than a distribution (Quaas et al., 2010) which would nonlinearly increase regions where RH is greater than 80% (e.g near cloud edges or oceans), as reported in previous studies (e.g. Haywood et al., 1997; Bian et al., 2009), would likely underestimate the real effect over these regions." (See manuscript page 11, line 13-23)

3. As the paper is now, I think there are two things that can be removed entirely. The discussion and figure about heating rates do not seem to add any substance to the paper, and should be removed.

Reply: The discussion and figure about heating rate is removed from the manuscript.

4. The aerosol forcing discussion is confusing. If I understand correctly, measured AOD is combined with simulated AAOD to derive a measurement model hybrid ARFE. This ARFE is then applied to retrieved AOD to get the regional forcing map in Figure 6. There is, however, no discussion of any available measurements of AAOD or SSA to verify the accuracy of modeled or retrieved AAOD. SSA is as important as AOD in determining radiative forcing, so even a literature search of previous studies in and around India might be a useful way to constrain AAOD beyond the comparison in Figure 5a and 5b.

Reply: This is an important point. In response, we have validated model predicted SSA with available measurements from previous studies over BoB and AS regions, added a table summarising this analysis (Table 2) and added the following discussion to the manuscript:

"A summary of SSA measurements carried out in the recent years over BoB and AS regions is given in Table 3 along with modeled SSA values. The mean value of model simulated SSA value over BoB is found to be 0.91 $\pm$ 0.02, which is consistent with the finding from ICARB measurements (Nair et al., 2008; Kedia et al., 2010) and from previous INDOEX measurements (Ramanathan et al., 2001). The mean SSA (0.94

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± 0.01) value (see Table 2) over AS is also found to be within the range of uncertainties of the measured SSA values (Kedia et al., 2010). The mean SSA value over AS is higher than BoB region indicate that the dominance of absorbing (BC) aerosols over BoB regions. SSA values from previous studies (Ramachandran and Jayaraman, 2002; Ramanathan et al., 2001) were found to be slightly lower over BoB and AS regions (Table 2) because all these studies were conducted in the winter monsoon season (December–March) during which the absorbing aerosols from the south Asian region get transported to these regions. The close correspondence between the model simulated and the measured (Nair et al., 2008; Kedia et al., 2010) SSA value indicate that representation of chemical composition and mixing state in the model is likely to be valid over BoB and AS regions." We emphasise this more carefully in the revised manuscript (page 20, line 16-22 and page 21, line 1-8).

Specific comments p 13918, line 5: Why did you use 0.7 to weight AOD on low retrieval days? Why not something like weighting by number of valid retrievals?

Reply: The factor 0.7 is the average ratio of number of valid retrievals to the median of number of valid retrievals (20) during the cruise days. We included this in the revised manuscript. See manuscript (Page 8, line 1-2)

p 13918, line 15: Would absorbing organic matter impact OMI retrievals in the region?

Reply: We agree that absorbing organic matter impact OMI retrievals over Indian region. We revised the text with the following sentence:

"AI is a useful qualitative indicator for tracking the presence of absorbing and elevated aerosols, such as biomass burning plumes and dust aerosols." See manuscript (Page 8, line 9-10)

p 13930, lines 15-20: Change Reaction to Region.

Reply:This change is now included.

Fig. 1 caption: Delete 'See text for details.'

Reply: This change is now included.

Figures 1-3: It may be more informative to show these figures as a difference plots.

Reply: Since the difference plots not provided more information than the mean bias and RMSE reported (Table 1) in the earlier version of the manuscript, the original version of figures is retained.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 13911, 2011.

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