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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Spatio-temporal aerosol optical characteristics over the Arabian Sea during the pre monsoon season" *by* D. G. Kaskaoutis et al.

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The paper present results on the spatio-temporal variations of aerosol optical depth (AOD) and Angstrom exponent over Arabian Sea during the Indian pre-monsoon season. In this study, the authors have used a handheld sunphotometer (Microtops-II) onboard a ship cruise during the ICARB field campaign. Using the spectral AOD data at 6 wavelengths in the spectral range 340–1020 nm, the authors have fitted a second order polynomial to log (AOD) vs log (wavelength) for two spectral ranges (i) 340–1020 nm, and (ii) 340–870 nm and found that the polynomial fit is more accurate for the spectral range 340–1020 nm. Other than this result, I don't see anything new being

reported in this study. Most of the results presented in this study have already been published by the authors (details given in the comments below). I disagree with the opinion of other reviewers that this paper is a useful contribution to enhance our understanding of aerosols over Arabian Sea. Aerosol characteristics over Arabian Sea has been the topic of intense investigations in the past several years (Babu et al., 2004, 2008; Bhat, 2006; Jayaraman et al. 1998; Johansen and Hoffmann, 2003, 2004; Jones and Christopher, 2008; Jones et al. 2009; Kedia and Ramachandran, 2008, 2009; Krishnamurti et al., 1998; Moorthy and Satheesh, 2000; Moorthy et al., 2001, 2005; Nair et al., 2003; Nair et al., 2004; Nair et al. 2008; Rahul et al., 2008; Ramachandran, 2004a,b; Ramachandran, 2005a,b; Ramachandran and Jayaraman, 2002; Raman et al., 2002; Ramanathan et al., 2001; Satheesh and Moorthy, 1997; Satheesh and Srinivasan, 2002; Satheesh et al., 1998; Satheesh et al., 2001; Satheesh et al., 2006a,b; Vinoj and Satheesh, 2003; Vinoj et al., 2007), which the authors also mention in their introduction (page 22225). There are several shortcomings in this paper that needs to be addressed.

(1) The Figures 12 & 13 (in page 22266-22267) are already published in the Journal of Earth System Sci. (Vol. 117, pages: 243-262, 2008) and Atmospheric Environment (Vol. 42, pages: 6816-6827, 2008) by the authors of this study (Kalapureddy and Devara, 2008, Fig. 4a; Moorthy et al. 2008, Fig. 4 & Fig. 7a). So what new scientific information is conveyed by again publishing these two figures? On going through these papers, a question arises: Was the same sunphotometer used in all these studies? If yes, then what is the need to show these plots again (they can be referred and cited)? However, if different sunphotometers were used, then did the authors ever attempted to inter-compare the results from the other instruments used during this campaign?

(2) The plots of airmass back trajectories (shown in Fig.11, page 22265 and discussed in section 6.2, pages: 22241-22243) has already been presented by the authors (Kalapureddy and Devara, 2008, Fig. 3a) which also uses the same database. The only difference is that in this study, the authors have examined the back trajectories for all

the days of the cruise, whereas in their previous article, they have presented the back trajectories for representative days only. So what additional information does these mass plots of back trajectories convey?

(3) In Page 22227, lines 10-12, the authors have mentioned that the typical error in the AOD estimation is ± 0.03. Is this absolute error or relative error? How did they arrive at this value? Which wavelength does this error correspond to? A positive bias (+0.03) at 340 nm and a negative bias (–0.03) at 1020 nm can effect the results and conclusions. So, it is very important to know the errors in AOD at each wavelength of the spectral range. Cachorro et al. (2004) pointed out that an inaccurate calibration can lead to a diurnal cycle of the AOD and would result in significant AOD errors at the miscalibrated wavelength. The authors should attempt to perform a detailed error analysis on the measured optical parameters and examine the results on the retrieved parameters (Angstrom exponent, a1, a2) in light of this.

(4) In page 22227 (lines 8-10), the authors have stated that "This sunphotometer provides the AOD at 6 channels .... using internal calibration". What does the internal calibration mean? Was the sunphotometer calibrated (direct sun calibration using Langley technique or radiance sphere calibration) before and after the campaign? As it is not mentioned explicitly, one can assume that the calibration was not done. If it was done, then it would be worth mentioning the results of the calibration (change/drift if any, in the calibration values before and after the campaign) and how these issues has been taken care of.

(5) In page 22227 (lines 6-10), the authors state that Microtops-II provides AOD at 6 channels, columnar water vapor and ozone. I am not aware of any such Microtops-II instrument that provides so many parameters (would need at least 10 channels). To my knowledge, a single Microtops-II can have a maximum of 5 channels. Did the authors use two different Microtops-II: one which provides AODs at 5 wavelengths (340, 440, 500, 675, 870), and the other which provides ozone retrievals (using 3 UV channels), water vapor (using 936 nm) and AOD at 1020 nm. If this is the case, then the authors

should mention this point explicitly in the manuscript.

How did the authors ensured that the errors (pointing errors, errors due to detector temperature sensitivity, etc) in the two Microtops-II were not substantial enough that could cause a bias in the results. Since Microtops-II is a handheld instrument (manually operated), there could be pointing errors and the magnitude of the errors would be much higher on moving platforms. Were these instruments operated by the same operator during the entire campaign? Since two different Microtops-II could have been used in this study (which I assume might be the case), the pointing errors could be different, and also operator dependent. The authors should justify all these issues.

(6) In page 22227 (lines 26-28), the authors state that the column water vapor (PW) was observed to be 2.22 ± 0.44 cm (Kalapureddy et al., 2008) and hence the possible water vapor absorption effects at 1020 nm have not been considered. However, on going through the article by Kalapureddy et al. 2008 (Page 3162, Fig.7), it is very clear that the PW varied between 1.0 and 5.0 cm and showed large spatial variability. PW values as high as 4.0 cm occurred over coastal regions of South West India (Arabian Sea region). Values of PW >3.0 cm can influence the AODs at longer wavelengths, which has been ignored in this study (as stated in Page 2227, lines 26-28). This definitely needs some justification?

(7) In Page 22227 (lines 18-24), the authors mention about detector temperature sensitivity as a possible uncertainty for the 1020 nm channel and admit that Microtops-II algorithm doesn't take the detector temperature sensitivity into account. Why didn't the authors attempt to re-analyse the data taking the temperature effects into account. Microtops-II does have a built-in temperature sensor which monitors and delivers the internal temperature for each set of measurement. In the absence of this information, the authors can still consider using the onboard meteorological data (presented in Kalapureddy and Devara, 2008). The authors should make use of the temperature information and re-estimate the AOD at 1020 nm, so as to minimize the errors in the AOD estimation.

(8) In Page 22227 (lines 23-24), it is mentioned that the detector temperature sensitivity can be a problem during very clear conditions (very low AODs), which the authors have ignored. An examination of Fig. 8 (page 22262) shows that the AODs were very low during the period April 30 - May 5. During this period, the AOD at 1020 was ∼0.05 on most of the days and reached values as low as ∼0.025 on few instances. So, there can be temperature artefacts in the data during the prevailing clear conditions. This definitely needs some clarification.

(9) In page 22227 (line 8-10), the authors state that the sunphotometer provides the AOD at 6 channels based on the internal calibration, while in page 22230 (lines 11-13), they say that the AOD values at each wavelength is obtained from the direct-beam irradiance measurements via the Bouguer-Beer law and extensive analysis of the errors retrieved via this methodology is described in Kaskaoutis and Kambezidis (2006). It is not clear whether the AODs used in this study was obtained based on the internal calibration or based on the post processing of raw data obtained from the Microtops-II. The authors should clarify this point.

(10) In page 22230 (lines 13-15), the authors say that the ozone optical depth was omitted in the estimation of AOD, since its contribution to the total atmospheric optical depth can be significant under low turbidities. Why did the authors ignore ozone absorption in the AOD estimation, whey they already have at their disposal column ozone values for each set of AOD measurement (they mention this point in Page 22227, lines 6-10). The authors have also published the results of ozone from this campaign in ACPD (Kalapureddy et al. 2008). Then why didn't they make use of this ozone data for the AOD estimation. Omission of the ozone optical depth can cause an underestimation or over-estimation (depending the column ozone values) of the AOD in the Chappius band (440-770 nm), thereby causing artefacts in the AOD spectra.

(11) The authors mention in page 22230 (lines 15-17) that they didn't take into account absorption due to trace gases by assuming that it will be negligible over the oceanic regions. To reiterate this point, they also cited a reference of Kaskaoutis et al., 2007. The

paper by Kaskaoutis et al. (2007) gives results of the investigation of ozone and trace gas contribution to the total optical depth over a polluted urban environment (Greece) and not over Arabian Sea. Furthermore, Kaskaoutis et al. (2007) have clearly stated in their abstract that the corrections due to ozone and trace gases optical depths is necessary for an accurate determination of AOD in the Chappuis band. How could the authors differ in their opinion in this study.

(13) The authors should provide strong justification for ignoring the absorption effects due to ozone, other trace gases (NO2) and PW in the estimation of AOD. Ignoring the absorption effects due to these species could result in significant artefacts in the AOD spectra.

(14) In pages 22235-22236, the authors have compared their results with the literature, however they didn't compare their results with those obtained using similar instrument during the same field campaign as reported by other investigators (Kedia and Ramachandran, 2008). Using shipborne sunphotometer measurements and MODIS satellite data during the ICARB campaign, Kedia and Ramachandran (2008) have reported an AOD of 0.24 and Angstrom exponent of ∼1.0 over Arabian Sea. These results are very much similar to that obtained by the authors in this study. However, it is surprising that this paper (which appeared in the ICARB special issue of the Journal of Earth System Sci. (Vol. 117, 2008) has not been cited (could be an oversight). Furthermore, the temporal variation of the spectral AODs shown in Fig. 8 (page 22262) is very similar to that presented by Kedia and Ramachandran (Page 379, Figure 2 right panel) and Kalapureddy and Devara (Page 6818, Fig. 2). So what additional information does the authors want to convey from Fig.8 which is already published by them and other colleagues/investigators?

(15) Several instruments were used during the ICARB shipborne campaign and surely several sunphotometers were in operation during this study (from the series of papers published from this campaign). One such example (mentioned in the previous comment) is the paper by Kedia and Ramachandran, (2008), which also presents re-

sults from shipborne sunphotometer measurements from the same campaign. Did the authors ever attempted to inter-compare the results from the other onboard sunphotometers? The authors also have at their disposal a Cimel Sunphotometer (AERONET instrument) which has been in continuous operation in Pune (India) since October 2004 and one of the co-authors is the PI for this instrument. The authors could have attempted to compare the AOD retrievals from Microtops-II (used in this study) with Cimel before or after the campaign? It would be worthwhile inter-comparing the AODs obtained from various instruments before presenting the results.

(16) The ICARB shipborne campaign was conducted over Arabian Sea (AS), Indian Ocean (IO) and Bay of Bengal (BoB) and the authors have definitely made measurements in all these oceanic regions (as reflected in their publications: Kalapureddy and Devara, 2008; Kalapureddy et al., 2008, 2009; Moorthy et al., 2008). Then why in particular, did they use the data from AS only? Why didn't they extend their analysis to BoB and IO region and compare the results with that over AS?

The authors have a very good database with several instruments at their disposal. It is highly recommended that the authors should try to inter-compare the retrievals with those obtained (i) using other instruments (other onboard sunphotometers, Cimel, etc) and (ii) over other oceanic regions (BoB, IO). Also, a detailed error analysis of the measured optical parameters (AOD) and its effect on the retrieved parameters (Angstrom exponent, a1, a2) should be included and discussed in the manuscript.

It is very much important for the authors to quantify the error in the measurement of the AOD at 1020 nm with high accuracy, because the authors have found that the polynomial fit is more accurate for 340–1020 nm wavelength range, as compared to the 340–870 nm range (this being one of the major conclusions of this paper). Since the inclusion and exclusion of the 1020 nm in the data analysis significantly affects the results, it is very important to have a very accurate measurements of AOD at this wavelength.

In the absence of instrument calibration and detailed error analysis in the AOD estimation, the results presented could be an artefact of the data processing. Overall, this study lacks an original and compelling experimental design. Reports on the spatio-temporal characteristics of aerosols over Arabian Sea is not new (and part of the results presented in this paper are already published by the authors), and I find the reported results are too routine to warrant publication in ACP in its current form. I therefore suggest a major revision of the manuscript, taking into account all the issues discussed above.

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