

Interactive comment on “Impact of continental outflow on chemistry of atmospheric aerosols over tropical Bay of Bengal” by B. Srinivas et al.

Anonymous Referee #3

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General comments This paper presents data on chemical characterization of both soluble and insoluble fractions of PM₁₀ and PM_{2.5} over the Bay of Bengal for one month period in winter. This work evidences that transport of anthropogenic pollutants from the continent considerably affect the composition of the marine boundary layer during the late monsoon. A major concern of this paper is that most results presented have been already published in a previous paper by the same authors (Kumar et al 2010, Marine Chemistry 121, 167-175). Thus, the major conclusion of this paper is the evidence of the impact of anthropogenic sources in the MABL in the Bay of Bengal. This was already concluded by the above cited paper. Authors should clarify which is the added value of the present paper with respect to the previous one. Another concern is that, in some occasions, interpretation of the results is not adequate. Thus it is unreasonable to quantify the contribution of biomass burning based on the TC/ EC; OC/EC

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ratios. Specific comments The aim of the paper is to provide data on size dependent chemical composition of aerosols. As authors said in the introduction is necessary to provide models with “real time data ion size dependent chemical composition of atmospheric aerosols and the associated spatio-temporal variability”. However, one major limitation of this paper is the relatively short period investigated (1 month with daily resolution). Given the short duration of the sampling period a more detailed meteorological description is needed in order to interpret the source origin of particulate matter. Thus, in the first half of the sampling period background levels are similar to those recorded during the second half. However, there are two high episodes (the 2nd and the 7th of January) showing the increment of most of the PM components. What is the origin of these two episodes? It is surprising that in the previous paper, by Kumar et al., 2010, the interpretation of results is based on a more detailed meteorological interpretation, considering IGP, SEA and MAP scenarios. However in the present paper only the spatial variation has been considered. I think it is necessary to combine both the spatial and the meteorological information. More information about sample treatment and analysis is needed. Have the blank concentrations been subtracted from the bulk sample concentrations? Have you dissolved and analyzed certified standards? What is the reason of the differences found out between the EC concentrations in this paper with respect to Kumar et al. 2008b (page 20678) The correlation between OC and EC for PM_{2.5} shows the presence of two different groups (figure 9) of N Bo B; the same is evident for PM₁₀; however, the method used for calculating R² shows a high correlation (R²= 0.87) – page 20678 lines 25-30; The four values with high contents of OC and EC in N Bo B show a different ratio and can be related to a different origin (probably related to the two peak episodes reflected in Figure 8. Ratios OC/EC >2 may be attributed to other causes than biomass burning (Page 20679; lines 10-15). Thus, oxidation and ageing of VOCs, and condensation onto other particulates may increase the ratio OC/EC. Please, update the references (Plaza et al. Atmospheric Environment 45, 2496-2506, 2011; Pio et al., Atmospheric Environment 45, 2496-2506, 2011.) It is not reasonable to quantify the contribution of biomass burning to the carbonaceous species based on the TC/EC and

the K+/EC ratios (Page 20680). These ratios may vary considerably as a function of the biomass composition. Section 3.2.3. Page 20682. This section is obscure. It is not clear if a desert dust outbreak was sampled during the study period. Ratios of Al/Ca or Al/Fe may vary considerably with respect to the crustal ratio as a function of the source origin. In addition, these components may be emitted by a number of anthropogenic sources such as traffic resuspension, industrial activities, or construction, with a different chemical signature and finer size distribution with respect to the naturally originated. As deduced from Figure 8, crustal related elements show a high correlation with anthropogenic elements; therefore an anthropogenic origin for these elements could not be discarded. For the above reasons, the estimation of crustal load from Al is not adequate; mainly if the crustal ratio is applied for the PM2.5 fraction. It would help for interpretation a correlation analysis or a single principal component analysis (PCA). In section 3.2.1, about water soluble ionic composition, a clear correlation between coarse nitrate and Na is evidenced showing the interaction between nitric acid and marine sodium chloride. However in section 3.3.1 an interaction between sulfate and Na is suggested (although not evidences are presented). These two sections should be combined and clarified. Section 3.3.2. This paragraph is not connected to the MS. Sections 3.3.3 and 3.3.4. Results are a summary of results previously published in Kumar et al., 2010. Thus, subsection 3.3.3 on "Anthropogenic sources on aerosol iron solubility", showing the enhancement of Fe solubility by the interaction with anthropogenic pollutants, is mainly based on results presented in the above mentioned paper. This subsection should be eliminated. These results could be summarized and presented as support of the results presented in this paper but the statement on iron solubility should be eliminated from the abstract, given that these conclusions were obtained in the previous paper. Technical corrections 20676 - L6; is 20682- I21- "where the dust levels were significantly low." Please include equations and regression coefficients in Figures 7 and 9 to 12 Figures 6 and 8; quality can be improved; these Figures are very similar to Figures presented in Kumar et al 2010. Table 1. Include number of samples

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