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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Organic and inorganic markers and stable C-, N-isotopic compositions of tropical coastal aerosols from megacity Mumbai: sources of organic aerosols and atmospheric processing" *by* S. G. Aggarwal et al.

Anonymous Referee #2

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General comments

The manuscript presents the results of a pretty complete set of aerosol analyses, allowing a good insight into aerosol sources in the urban area of Mumbay. The major finding of this study is that "atmospheric lifetime of tropical urban aerosols could be longer in winter than in summer" and this can actually have repercussion on the current ability of modeling aerosol concentrations and properties in the tropics. The major weak point resides in the limited number of samples used to infer information on summer and, particularly, winter conditions. The reader should be clearly cautioned that the conclusions

of this manuscript hold only if the selected short sampling periods are actually representative of summer and winter conditions over the Mumbay area. The manuscript is well written and scientifically sound, literature is cited appropriately. Given also the fact that the Mumbay area has scarcely been studied so far, I recommend publication once the following detailed comments are addressed.

Specific comments

P20594-L11. "...indicating an enhanced emission from these sources in winter season". This statement is questionable as also the different boundary layer height can explain the concentration difference, as recognised by the authors themselves later on in the manuscript.

P20598-L14. In PM10 samples the contribution of carbonate carbon can be non negligible depending on the sources affecting the sampling site. The authors should better support their assumption. For instance, do the Ca2+ and Mg2+ measurements suggest that carbonate contribution is negligible? Or at least does the nssCa2+/TC ratio show that the carbonate carbon contribution is comparable for all the samples?

P20599L20. Collecting only a blank filter does not allow for evaluating the filter batch variability. According to personal experience, I can say that filters, although coming from the same batch and treated in the same way, can be very different one another as for blank levels. This could have brought significant uncertainty to the measurements according to the signal-to-blank ratio of the samples. In the "Chemical analyses" section important information is missing: what was the signal-to-blank ratio for the analysed aerosol components? What was the overall uncertainty associated to the analysed aerosol components?

P20600L14. How do the weather conditions (T, RH, P, wind direction, rain, etc...) met during the two campaigns compare with average summer and winter conditions at Mumbay? Given the short time extent of the campaigns ($\sim$ one week), the authors should spend some words to support their assumption that the samples are represen-

tative of the whole season.

P20603L9. "As discussed above, both nssSO2−4 and EC are the major fractions of Mumbai AM". Actually it is OM from Figure 2.

P20603L13. Do the authors have any justification for the apparent increase of the anthropogenic activity in winter? The temperature is not as low as to require home heating. This is probably an effect of the reduction of biogenic emissions and photochemical activity in winter. This should be better addressed.

P20603L20. Are there any measurements or literature data on the PBL height at the sampling location or at comparable sites? Knowing the PBL height difference between summer and winter will make easy to calculate the contribution of the atmospheric dynamics to the observed higher aerosol mass loading, allowing a better analysis of the aerosol sources strength in the different seasons.

P20603L26. This sentence is apparently contradicting what the authors say in L17-18 of this same page.

Par. 3.4. The conclusion of this paragraph is ambiguous: are the authors attributing the different isotopic ratio to "(i) changes in source contribution, and/or (ii) enhanced atmospheric processing of aerosols"? If the conclusion is that probably both are contributing or that there is not enough data to answer to the starting questions, the authors should address the point more clearly.

P20604L10. The different contribution from organic nitrogen is likely the most significant difference observed in this dataset between summer and winter. I encourage the authors to discuss this more in depth.

P20604L11. Indeed, there is evidence that biomass burning is a source of water soluble organic nitrogen (Mace et al., JGR, 108, D16, 4512, 2003). Have the authors considered photochemistry as a source of organic nitrogen? This could explain the higher organic nitrogen concentrations observed in summer. What about cloud pro-

cessing also?

P20605L23. Are these correlations significant? At what confidence interval?

Par. 3.5. How do the authors interpret the day-night trend of δ13C in summer (Figure 8)? Does it not suggest some influence of local sources at the sampling site in contrast with what said previously in the text?

P20609L19. The authors should include also wet removal as a cause of the aerosol lifetime reduction in the tropical summer. If they have reasons to assume that the degradation to CO2 of organic aerosols is more effective than their wet removal, they should present them.

P20610L25. "These values and their plots against nssSO4-2/TC, levoglucosan-C/TC and EC/TC also supported the major sources identified for the Mumbai aerosols". These plots have not been presented or discussed in the text. If they are important to draw the conclusions they should be presented in the results section.

Technical corrections

P20605L6. The punctuation in this sentence seems to need double checking.

P20609L18. "Are existed"?

Figure 3. Longitude is misspelled.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 20593, 2012.