

Interactive comment on “Carbonaceous components, levoglucosan and inorganic ions in tropical aerosols from Tanzania, East Africa: implication for biomass burning contribution to organic aerosols” by S. L. Mkoma et al.

Anonymous Referee #1

Received and published: 26 December 2012

This paper presents database for PM, carbonaceous components, levoglucosan and water soluble inorganic ions and illustrates that biomass burning and atmospheric photochemical processes are the main sources in Tanzania. No matter the experiment or the long paper take authors' much effort to finish the work, while there are no new scientific findings that could arouse readers' much interest on this paper. So I am afraid it is not probable to publish on ACP. Most of the results are the same or small difference with the referenced literatures as the author mentioned in the paper. Even though some new points, such as higher nss-K⁺/EC ratios in Tanzania than other nations, the

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

authors did not explain it deeply. The paper needs to be restructured and polished more to find out the unique points.

General comments: 1. There are too many displays of the data in each subsection of the discussion part. Readers could easily find out the data you have shown in the text from the tables or figures, so it is not necessary to show all of them. It is even more complicate for readers to identify all these data. 2. Fig 2-6 (except fig 3) showed the daily variations of the analyzed compounds or the ratios. Back ward trajectories and meteorological parameters should be combined together to discuss the enhancement or decrease of the mass concentrations/ratios. 3. Fire point figures should also be shown in the study to verify that intensive biomass burning points could be observed in Tanzania. 4. Transported biomass burning aerosols from nearby regions, such as Mozambique, could also be a source for the studied region. Discussion of local and transported aerosols should be shown together in the study. 5. More specific calculation method should be applied in the last section to calculate the biomass burning contributions to the OC or PM.

Specific comments: Abstract: The last sentence in the abstract illuminates the core points of the paper. In the front part of abstract, there are too many showings of the data. “that concentrations of biomass burning tracers (levoglucosan and mannosan) well correlated with non-sea-salt-K⁺, WSOC and OC”, in fact there are no displays of correlation between mannosan and other compounds in the paper, except levoglucosan in Fig 10.

P2. Line 23: Two other biomass burning types, including forest fire and grassland fire should also be included.

P3. Line 3: Please cite some references for the last sentence to support your point.

P3. Line 17: Sea salt aerosol is also an important source of potassium. It is better to explain clearly the relationship between nss-K⁺ and K⁺.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



P3. Line 19: Levoglucosan is mainly decomposed by cellulose, but not hemicellulose. Please check carefully again the literature Simoneit et al., 1999.

P4. To show the sampling site in a separate figure with parameters such as long/latitude, altitude and cities/mountains/oceans around the site would be much clear for readers to have a general impression on the local situation. In the text, popularity and industry in the city should also be explained roughly.

P5. Line 8: Whether the samples will be firstly packed in an aluminum file? Otherwise the stuff on the filter will probably stick to the upper one.

P6. Line 25: A brief description of QA/QC in the method of analysing anhydrosugars should be introduced, including recovery, standards, linearity and so on. There is also some discussion of mannosan in the text, thus title of 2.2.3 section is not complete and it should be changed.

P10. Line 1: Could you please explain the reason why your PM10 mass concentrations are lower than the referenced data?

P11. Line 11: OC/EC ratio from biomass burning and vehicle exhaust are quite different. For example, Saarikoski et al. 2008 (Sources of organic carbon in fine particulate matter in northern European urban air) showed that OC/EC ratio derived from biomass burning was 6.6, Sandradewi et al. 2008 (A study of wood burning and traffic aerosols in an Alpine valley using a multi-wavelength Aethalometer) showed the ratio from wood burning was 7.3, but from vehicle exhaust was 1.1. More discussions need here to prove that high values of OC are from biomass burning. As for the low OC/EC ratios, why is it not from the mixture aerosols of biomass burning and vehicle exhaust?

P11. Line 19: What is the reason for the quite different r^2 in the wet and dry season (Fig 3a)?

P12. Line 5: There was a significant enhancement of WSOC in Figure 4b between 29/7 and 3/8 as the authors described. Levoglucosan was also elevated during these days

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

in Figure 5. It is better to combine the different compounds to illuminate the sources.

P12. Line 10: There is no discussion and no showing in Figure 5 of mannosan in this section, but why the title contains mannosan? At the end of this paragraph, could you distinguish these different burning sources?

P13. Line 6: “The ion distributions during the episode (1–3 August) are more pronounced with NH_4^+ and K^+ in $\text{PM}_{2.5}$, in which the abundance of NH_4^+ was comparable to that of SO_4^{2-} (Fig. 6).” I cannot find K^+ in $\text{PM}_{2.5}$ during August 1-3, so why it is pronounced? It seems that the major ions are NH_4^+ , Mg^{2+} , NO_3^- , Ca^{2+} in $\text{PM}_{2.5}$. The description in the text and figure showing are not constant. The same situation happens for the PM_{10} . Please check carefully the text and figure 6.

P13. Line 17: “low temperature and enhanced biomass burning activities (an important source of NO_x) may be responsible for the difference in NO_3^- levels between the two seasons”. In fact there is no temperature difference in the two seasons as you describe in section 2.3. Vehicle exhaust in the city nearby is not a possible reason?

P15. Line 7: “The production of WSOC was slightly more favorable during the wet season”, what is the reason? It seems inconsistent with your explanation “SOA is more hydrophilic and one would expect larger WSOC/OC in dry season than in wet season”.

P15. Line 22: EC and BC are not directly comparable due to their measurement principles. When you compare the EC/TC ratios with BC/TC in the references, you should remind reader briefly the difference.

P19. Line 7: It is interesting that nss-K^+ showed a stronger correlation with OC in PM_{10} . Could you explain briefly the possible reason?

P19. Line 19: What is the reason for the higher $\text{nss-K}^+/\text{EC}$ ratios in Tanzania? Is it due to the higher nss-K^+ or lower EC compared with other nations?

P19. Line 29: Are there any reference data of $\text{PM}_{2.5}$ $\text{nss-SO}_4^{2-}/\text{OC}$ (or EC) and NO_3^-

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

/OC from biomass burning aerosols which are comparable to your ratios? If yes, then you could conclude the last sentence in this section that Morogoro are more influenced by biomass burning but not fossil fuel combustion.

P21. Line 1: The Lev/Man ratios could be used to distinguish the biomass categories with the lowest ratios for soft wood (3-5) and higher ratios for hard wood and crop residues (>10). It is hard to say whether Tanzania has more influence from hard wood and crop residue burning if without calculation or investigation. Even though the burning amount of soft wood is the same as the other two, the Lev/Man ratios could be still > 10. So it is better to express that Tanzania is influenced by mixture aerosols from softwood, hard wood and crop residue burning.

P21. Line 16: For calculating the biomass burning contributions to OC, the authors could refer to the literatures by Sang et al. 2011 (Levogluconan enhancement in ambient aerosol during springtime transport events of biomass burning smoke to South-east China), Zdrahal et al., 2002 (Improved method for quantifying levoglucosan and related monosaccharide anhydrides in atmospheric aerosols and application to samples from urban and tropical locations), Puxbaum et al., 2007 (Levogluconan levels at background sites in Europe for assessing the impact of biomass combustion on the European aerosol background) and Zhang et al., 2010 (Chemical speciation, transport and contribution of biomass burning smoke to ambient aerosol in Guangzhou, a mega city of China).

Technical comments: Figure 3: There is Fig. 3b in the text (P11), the labels (a and b) should also be shown in the figure.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 28661, 2012.

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)