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Atmospheric Chemistry and Physics Discussions

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 #2

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This manuscript presents data on the PM10 and PM2.5 aerosol composition during wet and dry season campaigns in the year 2011 at Morogoro, Tanzania. The parameters measured were the PM mass, the carbonaceous components OC, EC, and TC (= OC + EC), the major inorganic ionic species (also MSA-), and the anhydrosugars levoglucosan and mannosan. Similar measurements at Morogoro were performed in 2005 and 2006 wet and dry season campaigns (Mkoma et al., 2009a,b, 2010a,b). For the earlier campaigns, also many elements (including important indicators for soil dust) were measured. As a consequence, there is little novel in the present manuscript.

Moreover, there is too much repetition and re-iteration of what was already written in the previously published papers of the first author. The present manuscript also suffers from a lack of focus and there is too little data analysis and novel data interpretation in it. The only really worthwhile novel data in the present manuscript are those for levoglucosan and mannosan, but much too little is done with those new data. Therefore, I cannot recommend publication of the manuscript. I suggest that the authors write a new, much more concise manuscript and concentrate in it on the biomass burning indicators levoglucosan, mannosan, and water-soluble non-sea-salt potassium (nss-K+), perhaps complemented with other ionic species, which may have a contribution from biomass burning, and relate the biomass indicators to OC (or even TC). Interspecies ratios of the biomass burning indicators and ratios of the biomass burning indicators to OC (or TC) should be compared with (and related to) literature data for tropical and sub-tropical sites that were (heavily) impacted by biomass burning and with emission factor data given in Andreae and Merlet (2001). Note that Table 1 of the latter paper contains emission factors for levoglucosan, K, OC, and TC for various types of biomass burning, which could be used for obtaining interspecies ratios. By relating these ratios (and also the ones from other appropriate literature references) to their own ratios the authors should attempt to arrive at a much better assessment of the impact from biomass burning to the OC (or TC) and of the relative impact or the various biomass burning sources.

The IMPROVE thermal protocol (with thermal-optical transmission (TOT) correction) was used to obtain the OC and EC data for the current manuscript, whereas a NIOSH-like protocol (also with TOT correction) was used for the previous campaigns at Morogoro (Mkoma et al., 2009a,b, 2010a,b). It is well known that the split between EC and OC in TOT analysis depends on the thermal protocol and that different protocols may provide quite different EC/OC splits, especially for samples that are highly impacted by biomass burning, with the IMPROVE protocol giving larger EC data than the NIOSH protocol. Considering that EC is by far the minor of the two carbonaceous components (OC and EC) in the authors' samples (and is generally the smallest of the two in other

sample sets), the impact of the protocol on the EC values will be quite large, whereas the OC data are much less influenced by it (note that the TC data are independent of the protocol). As a consequence, one should be very careful in comparing one's EC data or ratios with EC in the numerator or denominator with data or ratios published in the literature. One should also take care in comparing EC and BC data. I am afraid that the comparisons in lines 10-26 of page 28,679 are not really justified.

Numerical data within the text and tables are often given with too many significant figures. To give one example: In the Abstract, instead of "were 28.2$\pm$6.4 μgm$-$3 and 47$\pm$8.2 μgm$-$3 in wet season, and 39.1$\pm$9.8 μgm$-$3 and 61.4$\pm$19.2 μgm$-$3 in dry season", it should be "were 28$\pm$6 μgm$-$3 and 47$\pm$8 μgm$-$3 in wet season, and 39$\pm$10 μgm$-$3 and 61$\pm$19 μgm$-$3 in dry season".

On a number of occasions (e.g., page 28,666, line 15; page 28,667, lines 9-10 and lines 16-17), chemical compounds or species are both given as a chemical formula and as the name of the compound or species. This is redundant. Giving one or the other suffices.

As indicated below, there are problems with several references. The language and grammar of the manuscript also need to be improved.

Specific comments:

1. Page 28,663, lines 11-12: It is unclear what the authors want to say with "water-soluble organic species (e.g. water-soluble organic compounds)". I suggest replacing it by "water-soluble organic species".

2. Page 28,666, lines 5-6: It is stated here that "a tandem filter set-up was used to account for positive artifact particulate OC data". Some clarification is needed. Do the authors mean that the OC data in their manuscript were obtained from the difference of the OC on the front filter and the OC on the back filter?

3. Page 28,668, lines 13-15: What about the relative humidity during the night?

4. Page 28,669, line 22, to page 28,670, line 1: It is stated here that the PM10 mass data in the current study are larger than in the earlier campaigns. Yet, further on, the authors state that the current data for TC and the major ionic species are similar to those of the earlier campaigns. This suggests that soil dust contributed substantially more to the PM10 mass in the current study than it did in the earlier campaigns. The fact that the samplers were set up at 2.7 m above ground versus at around 6 m in the earlier work (Mkoma et al., 2009a) may have contributed to this. However, it seems likely that local soil contributed now more, despite the fact that the authors state on page 48,664, line 25, continuing on page 28,665, lines 1-2, that "The site and its large radius were covered by grass (vegetation), hence it is not possible for immediate local soil dust to interfere with the sampling".

5. Page 28,674, line 6: In 2010 an update was published for the 2004 paper of Putaud et al., i.e.: Putaud, J.-P., Van Dingenen, R., Alastuey, A., Bauer, H., Birmili, W., Cyrys, J., Flentje, H., Fuzzi, S., Gehrig, R., Hansson, H. C., Harrison, R. M., Herrmann, H., Hitzenberger, R., Hüglin, C., Jones, A. M., Kasper-Giebl, A., Kiss, G., Kousa, A., Kuhlbusch, T. A. J., Löschau, G., Maenhaut, W., Molnar, A., Moreno, T., Pekkanen, J., Perrino, C., Pitz, M., Puxbaum, H., Querol, X., Rodriguez, S., Salma, I., Schwarz, J., Smolik, J., Schneider, J., Spindler, G., ten Brink, H., Tursic, J., Viana, M., Wiedensohler, A., and Raes, F.: A European Aerosol Phenomenology - 3: physical and chemical characteristics of particulate matter from 60 rural, urban, and kerbside sites across Europe, Atmos. Environ., 44, 1308–1320, 2010.

6. Page 28,674, line 17: I presume that it should be "1.0" instead of "1.1".

7. Page 28,678, line 12: What is the basis for stating that "the contribution of leachable K+ from dust materials is negligible". This statement should at least be substantiated by a literature reference.

8. Page 28,680, line 7: I presume that it should be "correlations" instead of "concentrations".

9. Problems with references:

p. 28,666, l. 27: “Fu et al. (2011)” is not in the reference list. There is “Fu and Kawamura (2011)” in that list, to which no reference is made within the text.

p. 28,671, l. 1: “Gatari et al. (2003)” should be replaced by “Gatari and Boman (2003)”.

p. 28,671, l. 19: “Turpin et al., 1991” should be replaced by “Turpin and Huntzicker, 1991”.

p. 28,672, l. 3: “Maenhaut et al., 2007” should be replaced by “Maenhaut and Claeys, 2007”.

p. 28,672, l. 21: “Fu et al. (2009)” is not in the reference list.

p. 28,673, l. 24: “Street et al., 2003” should be replaced by “Streets et al., 2003”.

p. 28,678, l. 6: “Riley and Chester (1971)” is not in the reference list.

p. 28,684, l. 24-27: There is no reference made to “Fu et al. (2010)” within the text.

p. 28,685, l. 26-27: There is no reference made to “IPCC (2007)” within the text.

p. 28,688, l. 30-32: There is no reference made to “Sheesley et al. (2003)” within the text.

10. Technical correction:

p. 28,682, l. 25: “S. M.” should be deleted.

References

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