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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.

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The authors greatly appreciate the reviewer's comments that improved the quality of our manuscript. We made changes and error corrected according to the suggestions. Please find our replies given in italic and preceded by "Reply:" We also indicate the actions that were taken in the revised version of the manuscript.

Reviewer #1 (Comments to Author):

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

Reply: We thank the reviewer for the comment to restructure and polish our manuscript that improved our discussions in the revised MS. We believe that in our study we made some new finding such as the correlations of levoglucosan with nss-K+, WSOC and OC and the higher nss-K+/EC ratios in Tanzania compared to studies in other countries. These points will be mentioned in the revised MS with the discussion on possible contribution of biomass burning to organic aerosols.

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.

Reply: We agree with the reviewer's comments. The discussion part will be revised in the MS by deleting some of the data in the text that already shown in the tables or figures.

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.

Reply: Although the air mass trajectories and some meteorological parameters showed no significant variation during sampling days, we have taken the reviewer's comments

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in the revised MS. Please see the discussions in the revised MS for Fig. 2 in lines 276-279, Fig. 4 in lines 301-302, Fig. 5 lines in 312-315 and Fig. 6 in lines 332-335.

3. Fire point figures should also be shown in the study to verify that intensive biomass burning points could be observed in Tanzania.

Reply: The following figure showing fire spots will be added in the revised MS.

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.

Reply: The reviewers comment has been considered in the revised MS. Two new paragraphs describing the main local and regional land cove and cropland distributions and common biomass burning activities were added in section 3.7, please see lines 521-539 in the revised MS.

5. More specific calculation method should be applied in the last section to calculate the biomass burning contributions to the OC or PM.

Reply: We agree with the reviewer's comment that calculation of the contribution of biomass burning to the OC or PM (using levoglucosan, OC and emission factors) could be useful. However, this was not considered in the manuscript because our study was not done at near source (different sources/biomass burning activities were thought to the sources for OC). Also the emission factors used in other studies cannot be a representative for our site due to the differences in geography, climate and vegetation types between the regions. We therefore considered only the levoglucosan to OC ratios to describe the contribution of biomass burning to OC.

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

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of correlation between mannosan and other compounds in the paper, except levoglucosan in Fig 10.

Reply: We thank the reviewer for the comments. The "mannosan" was deleted in the main document hence in the abstract part of the revised MS.

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

Reply: Forest and grassland fires have been included in the sentence in the revised MS. Please see line 57.

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

Reply: The sentence was reworded and a reference was added. Please see lines 64-65 in the revised MS.

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+.

Reply: The detail explanation on the relationship between nss-K+ and K+ is give in section 3.7. The sentences have been reworded following the referee's comments. Please see lines 74-76 in the revised MS.

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

Reply: The sentence is reworded in the revised MS lines 77-79 as ". . .. . . levoglucosan, which is produced by the pyrolysis of cellulose, has been used as a unique molecular tracer of biomass burning aerosols (Simoneit et al., 1999; Fraser and Lakshmanan, 2000)."

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

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and industry in the city should also be explained roughly.

Reply: A Figure showing the sampling site and description of the site will be added in revised MS as follows: “Intensive aerosol sampling campaigns were carried out at a rural background site (300,000 inhabitants) in Morogoro (06°47’40.8"S; 37°37’44.5"E, altitude 504 m, a.s.l) during the wet season (30 May to 13 June 2011) and the dry season (28 July to 8 August 2011). This site is located at about 200 km west of the Indian Ocean and the city of Dar es Salaam, a business capital in Tanzania (Fig. 1). The site has no major industries and the possible main local aerosol sources include field burning of crop residue and waste, emissions from livestock (cattle diary or farm) and domestic/forest fires”.

Fig. 1.

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.

Reply: We thank the reviewer for the technical note. We would like to clarify that our sampled filters were not parked in an aluminum foil; instead after sampling they were folded in half face to face and placed in the jar during storage. This point is added in lines 119-121 in the revised MS.

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.

Reply: Thanks for the helpful comments. Sentences describing quality assurance and control in levoglucosan analysis were added in lines 171-176 in the revised MS. We also deleted “mannosan” in our discussion in the revised text.

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

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Reply: The PM10 mass concentration in the current study are higher than those reported in previous studies mainly due to variation of meteorological parameters especially precipitation event during sampling days. This point will be added in the revised MS. Please see lines 245-247.

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?

Reply: Our sampling site is a rural site where aerosol contribution from vehicle exhausts is not significant. However, different biomass burning activities could contribute to the variation in OC/EC ratios as suggested by the reviewer. This point is added in the revised MS. Please see lines 281-282.

P11. Line 19: What is the reason for the quite different r2 in the wet and dry season (Fig 3a)?

Reply: The reason for different r2 between seasons could indicate that the impacts of local emission on organic aerosol concentrations are significant compared to the influence of long-range transported aerosol (especially in wet season). This point is added in revised MS. Please see lines 285-289.

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

Reply: A new sentence “Similarly, those days with higher WSOC and elevated lev-

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els of levoglucosan were observed (Fig. 5), suggesting a possible biomass burning contribution to WSOC" will be added in revised MS. Please see lines 302-305.

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?

Reply: The term of Mannosan will be deleted in the title and the revised text. Detailed results on anhydrosugars will be reported in a manuscript that is in preparation.

P13. Line 6: "The ion distributions during the episode (1–3 August) are more pronounced with NH4+ and K+ in PM2.5, in which the abundance of NH4+ was comparable to that of SO42- (Fig. 6)." I cannot find K+ in PM2.5 during August 1-3, so why it is pronounced? It seems that the major ions are NH4+, Mg2+, NO3-, Ca2+ in PM2.5. The description in the text and figure showing are not constant. The same situation happens for the PM10. Please check carefully the text and figure 6.

Reply: We thank for the reviewer's comments. The descriptions of the text and Fig. 6 were corrected in the revised MS by including the following sentences: "The temporal variations of ions are shown in Fig. 6. It is noteworthy that daily variability of the ion in PM2.5 and PM10 exhibited similar patterns with higher levels during 1−3 August. The ion distributions during the episode (1−3 August) are more pronounced with NH4+ and NO3− in PM2.5 and Mg2+, Ca2+ and NO3− in PM10 (Fig. 6)." Please see lines 337-341 in the revised MS.

P13. Line17: "low temperature and enhanced biomass burning activities (an important source of NOx) may be responsible for the difference in NO3- levels between the two seasons". In fact there is no temperature difference in the two seasons as you descript in section 2.3. Vehicle exhaust in the city nearby is not a possible reason?

Reply: The reviewer is correct in terms that there was no temperature difference between the two sampling campaigns. The sentence was reworded in revised MS. Please

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see lines 334-337.

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”.

Reply: The sentences were reworded and the reason was given. Short-chain polar organic compounds have functional groups that may lead to more water-soluble fraction in WSOC. Please see line 379-381 in the revised MS.

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.

Reply: We agree with the reviewers comment. A text describing the difference between EC and BC will be added in lines 388-391 in the revised MS.

P19. Line7: It is interesting that nss-K+ showed a stronger correlation with OC in PM10. Could you explain briefly the possible reason?

Reply: The sentence will be reworded in the revised MS by explaining the reason for the correlation of nss-K+ and OC. Please see lines 485-488.

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

Reply: In this study we observed high nss-K+ and low EC concentrations. Biomass burning is the main source for nss-K+ at our site. This point will be added in several sentences including lines 468-469 and 495-496 in the revised MS.

P19. Line 29: Are there any reference data of PM2.5 nss-SO42-/OC (or EC) and NO3-/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

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by biomass burning but not fossil fuel combustion.

Reply: A sentence “On the other hand, our SO42−/OC and NO3−/OC ratios are comparable to those reported from individual fires in Brazil (Ferek et al., 1998)” and references “Gillies et al., 2001 and Ferek et al., 1998” will be added in the revised MS. Please see lines 505-508.

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.

Reply: We agree with reviewer and appreciate the comment. The phrases were reworded in the revised MS in lines 546-549 and 559-562.

P21. Line 16: For calculating the biomass burning contributions to OC, the authors could refer to the literatures by Sang et al. 2011 (Levoglucosan enhancement in ambient aerosol during springtime transport events of biomass burning smoke to Southeast 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 (Levoglucosan 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).

Reply: We thank the reviewer’s comment for providing us with potential references in calculating the biomass burning contributions to OC. However, as replied for the former comments, our study has not be done at near source and emission factors used in other

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studies cannot be a representative for our site due to differences in geography, climate and vegetation species between regions. We therefore feel that the levoglucosan to OC ratios should better give a picture of biomass burning contribution to OC.

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

Reply: Fig 3 and the text (P11) were modified in the revised MS.

Anonymous Referee #2

We thank the reviewer#2 for his/her detailed and constructive comments on our manuscript. Below are the point-by-point replies to the comments and suggestions.

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.

Reply: Thank you for the comments, however we would like to point out the originality of this work as follows: (1) there had been no extensive studies conducted on source implication for organic aerosols in Tanzania, Africa, (2) the study on levoglucosan in aerosols has been conducted in this study for the first time in Tanzania. These points will be added in the revised MS. Please see lines 84-86 and 92-93.

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 lev-

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oglucosan 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.

Reply: We appreciate the reviewer's comments. For better clarity and focus on data analysis in the revised MS, we deleted/shorten some sentences particularly in the introduction and result & discussion sections. Please see the revised MS, in which removed sentences/phrases are shown. Although inorganic ions from the Tanzanian aerosols have been reported and discussed in Mkoma et al. (2009a,b; 2010a,b), the sampling sites are not the same. Thus we believe that it is worthwhile to discuss the data of this study for the further atmospheric implication in this MS. Unfortunately, our study cannot explain the emission factors for various types of biomass burning. We appreciate the reviewer's comments. However, we feel that those points would be our future research target in the area. Based on the reviewer's comments, however, sentences have been added to compare the various ratios of biomass burning indicators reported by Andreae and Merlet (2001) with our study to explain the contribution of biomass burning to OC. Please see lines 555-562 and 581-584 in the revised MS. We also added K+

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and LG/K+ ratios from our study in Table 2. Please see lines 967.

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.

Reply: We agree with the reviewers comments. A new sentence describing the difference between EC and BC was added in lines 388-391 in the revised MS. Also, we deleted comparisons made between EC and BC and those that used different OC EC analysis protocol. Please see lines 475-496.

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±6.4 μgm-3 and 47±8.2 μgm-3 in wet season, and 39.1±9.8 μgm-3 and 61.4±19.2 μgm-3 in dry season", it should be "were 28±6 μgm-3 and 47±8 μgm-3 in wet season, and 39±10 μgm-3 and 61±19 μgm-3 in dry season".

Reply: We revised all numerical data in the text and tables following the reviewer's suggestion. Thank you for careful look and technical comment.

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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.

Reply: We agree with the reviewer comment. We deleted redundant chemical formula or names in the revised MS. Please see lines 138, 151, 181-182, 187.

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

Reply: We appreciate the reviewer's comment. The few grammatical problems and errors with several references have been corrected in the revised MS.

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 ÂĄ"water-soluble organic species".

Reply: We reworded the sentence and used "water-soluble organic species". Please see lines 70-71 in revised MS.

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?

Reply: We appreciate the reviewer's comment. Yes, the reported OC data were obtained from the difference of the OC on the front filter and the OC on the back filter. A sentence "The "artifact-free" particulate OC data was obtained in a tandem filter set-up as the difference between the OC on the front filter and that on the back filter (Viana et al., 2006)." added in the revised MS. Please see lines 141-143.

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

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Reply: Unfortunately, we did not measure the relative humidity during night hours.

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 28,664, line 25, continuing on page 28,665, lines 1-2, that ÂĄ"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".

Reply: We agree and are thankful to the reviewer's comments. The sentences were reworded in lines 110-111 and 245-246 in revised MS to include the local soil dust contribution to the PM10 mass.

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., Huglin, C., Jones, A. M., Kasper-Giebl, A., Kiss, G., Kousa, A., Kuhlbusch, T. A. J., Loschau, 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.

Reply: We thank the reviewer for providing us with the update paper of Putaud et al., 2004 that will be included in revised MS. Please see lines 353 and 804-812.

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

Reply: The error was corrected. Please see line 364 in revised MS.

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.

Reply: The statement was deleted in the revised MS.

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

Reply: Error was corrected in line 512 in the revised MS.

9. Problems with references: Thanks for the careful reading. All the problems of references were corrected in the revised MS.

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.

Reply: Error was corrected, a reference Fu and Kawamura (2011) was added in the revised MS. Please see line 164.

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

Reply: The reference “Gatari et al. (2003)” was replaced by “Gatari and Boman (2003)”. Please see line 268 in the revised MS.

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

Reply: The reference “Turpin et al., 1991” was replaced by “Turpin and Huntzicker, 1991”. Please see line 285.

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

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Reply: The reference “Maenhaut et al., 2007” should be replaced by “Maenhaut and Claeys, 2007”. Please see line 299-300.

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

Reply: The sentence was deleted

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

Reply: The reference “Street et al., 2003 was replaced by “Streets et al., 2003”. Please see line 345.

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

Reply: The reference “Riley and Chester (1971)” was added in the revised MS. Please see line 823.

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

Reply: The reference “Fu et al. (2010)” was deleted in the reference list.

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

Reply: The reference “IPCC (2007)” was deleted in the reference list.

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

Reply: The reference “Sheesley et al. (2003)” was deleted in the reference list.

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

Reply: “S. M.” was deleted in the revised MS.

References Andreae, M. O. and Merlet, P.: Emission of trace gases and aerosols from biomass burning, Global Biogeochem. Cy., 15, 955.966, 2001. Mkoma, S. L., Chi, X., Maenhaut, W., Wang, W., and Raes, N.: Characterisation of PM10 atmospheric aerosols for wet season 2005 at two sites in East Africa, Atmos. Environ., 43, 631.639,

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Interactive Comment

2009a. Mkoma, S. L., Maenhaut, W., Chi, X.,Wang,W., and Raes, N.: Chemical composition and mass closure for PM10 aerosols during the 2005 dry season at a rural site in Morogoro, Tanzania, X-Ray Spectrom., 38, 293.300, 2009b. Mkoma S. L.,Wang,W., Maenhaut,W., and Tungaraza, C. T.: Seasonal variation of atmospheric composition of water-soluble inorganic species at rural background site in Tanzania, East Africa, Ethiopian J. Environ. Stud. Manage., 3, 27.38, 2010a. Mkoma, S. L., Chi, X., and Maenhaut, W.: Characterization of carbonaceous materials in PM2.5 and PM10 size fractions in Morogoro, Tanzania, during 2006 wet season campaign, Nucl. Instrum. Methods Phys. Res. B, 268, 1665.1670, 2010b.

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

3. Fire point figures should also be shown in the study to verify that intensive biomass burning points could be observed in Tanzania.

Reply: The following figure showing fire spots will be added in the revised MS.

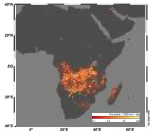

Fig. 1. fFgure showing fire spots

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Interactive Comment

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.

Reply: A Figure showing the sampling site and description of the site will be added in revised MS as follows: "Intensive aerosol sampling campaigns were carried out at a rural background site (300,000 inhabitants) in Morogoro (06°47'40.8"S; 37°37'44.5"E, altitude 504 m, a.s.l) during the wet season (30 May to 13 June 2011) and the dry season (28 July to 8 August 2011). This site is located at about 200 km west of the Indian Ocean and the city of Dar es Salaam, a business capital in Tanzania (Fig. 1). The site has no major industries and the possible main local aerosol sources include field burning of crop residue and waste, emissions from livestock (cattle diary or farm) and domestic/forest fires".

Fig. 1.

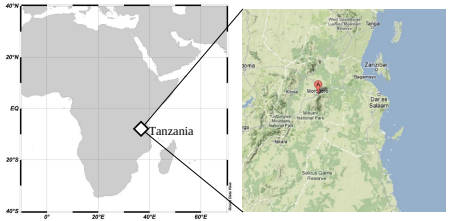

Fig. 2. A Figure showing the sampling site

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