Atmos. Chem. Phys. Discuss., 12, C10249–C10254, 2012
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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Size distributions of dicarboxylic acids, ketocarboxylic acids, α-dicarbonyls and fatty acids in atmospheric aerosols from Tanzania, East Africa during wet and dry seasons" *by* S. L. Mkoma and K. Kawamura

Anonymous Referee #2

Received and published: 6 December 2012

The paper "Size distributions of dicarboxylic acids, ketocarboxylic acids, α-dicarbonyls and fatty acids in atmospheric aerosols from Tanzania, East Africa during wet and dry seasons " by Mkoma and Kawamura deals with the sources of dicarboxylic acids and related compounds in atmospheric aerosols from Tanzania. The study is based on two different field campaigns conducted during wet and dry seasons in 2011 at the same rural sampling site in Tanzania. Even though there are no scientific findings in this manuscript, the measurement site is quite interesting because very few diacids and related compounds data are available in Africa region. However, interpretations of results are quite speculative and vague. Hence, the reviewer does NOT recommend publication of this manuscript in the Atmospheric Chemistry and Physics in its current form. A major revision is required before publication.

Major comments

-P2, L40: Diacids and related compounds can only explain ~2% of total organic aerosols as you mentioned in the abstract. Thus, the term "organic aerosols" is not suitable. It is better to confine to organic acids which you identified in this study.

-P2, L41-47: Both total diacids carbon fractions in TC and water-soluble OC are higher in PM10 size mode compared to PM2.5. For the reviewer's knowledge, if photochemical oxidation is important for diacids production, those ratios in PM2.5 mode should be higher than PM10 size mode. Additionally, authors mentioned that heterogeneous reaction on aerosols under high humidity is important for the production of diacids. However, RH during wet season is much higher than dry season.

-P6, section 2.4: Please add S.D. to average value.

-P6, L169-170: Average temperature during the warm wet (avg. 26 °C) and cold dry (24.6 °C) seasons are comparable. The difference is ~1.4°C. What is the threshold value for the separation of warm and cold season?

-P6, L171-172: Please add some evidence of temperature inversion. References or sonde data.

-P7, L210-214: Authors mentioned that higher abundance of oxalic acid may suggest enhanced emission from biomass burning, photochemical oxidation, and aging of organic acid during the long-range atmospheric transport. However, the interpretation is not sufficiently underpinned by the data. Authors have to explain more in detail to support your interpretations. Throughout the manuscript, interpretations are too much speculative and vague. Using only one compound data, authors simply characterized

ACPD

12, C10249–C10254, 2012

a certain phenomenon without deep scientific consideration.

-P8, L222-223: Interpretation is not sufficiently supported by the data.

-P8, L223-226: Too much speculative. How do authors know C9 is mainly produced from biogenic unsaturated fatty acids emitted from local vegetation? In late part of section 3.1, authors mentioned that fatty acids are emitted from ocean or long-range transported. Which is correct?

-P8, L240-243: Interpretation is not sufficiently supported by the data. The reviewer cannot understand how level of ketoacids can explain emission of volatile biogenic precursors from local vegetation. This is certainly not the only possible explanation. There may be several reasons for enhanced concentration of ketoacids during the dry season.

-P9, L257-259: The reviewer thinks that photochemical production of a-dicarbonyls may be enhanced under strong solar radiation during the dry season compared to the wet season.

-P9, L276 and L280: How did authors know lipid compounds are long-range transported? How about local emission? Can you separate local and long-range transport?

-P9, L284-286: Interpretation is not sufficiently supported by the data. Too much speculative. There may be several reasons of lowest C9/C18:1 ratio during the wet season. For example, depressed photochemical degradation of C18:1 during the wet season compared to the dry season and etc.

-P10, L290-291: Photochemical degradation may be enhanced under strong solar radiation. However, sky is mostly covered by clouds during the wet season.

-P10, L303-304: The reviewer cannot understand the statement in L303-304.

-P10, L304-307: Enhanced photochemical production is acceptable. However, increased emissions of their precursors are not scientifically supported by the data.

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12, C10249–C10254, 2012

Interactive Comment

-P10, section 3.3: Authors mentioned PM2.5 and PM10 as fine and coarse mode particles, respectively in the MS. However, coarse mode particle is defined as a difference between PM10 and PM2.5. Is the data shown in the MS for PM10 calculated from a difference of PM10 sample and PM2.5 sample? If not, authors have to avoid using the term "coarse" in the MS.

-P10, L319-321: Interpretation is not sufficiently supported by the data. How about a correlation between Ca2+ vs. a-dicarbonyls.

-P11, section 3.4: In section 3.3, authors mentioned that total diacids mostly present in fine mode. If it is so, total diacids to PM2.5 mass (or PM2.5-TC, PM2.5-WSOC) ratio should be higher than those to PM10 mass (or PM10-TC, PM10-WSOC) ratio. However, authors showed opposite results in section 3.4. Authors have to clarify them.

-P12, L359-365: This kind of intercontinental comparison has a high risk. Because total diacids are minor compounds in total PM mass, relative abundance of diacids to PM mass is governed by other major compositions such as inorganic ions.

P12, L378-380: Too much speculative. Interpretation is not sufficiently supported by the data. In L241-242, authors mentioned than elevated ketoacids during the dry season is due to additional emission of BVOC from local vegetation.

P13, L418-420: The statement is vague. The reviewer cannot understand how high C4/C3 ratio suggests both biomass burning activities and oxidation of diacids.

P14, L422-426: Interpretation is not sufficiently supported by the data. The reviewer thinks that C3/C4 ration at this site is relatively lower than other sites over the world. Thus, photochemical degradation of C4 is not important for the production of C3 in this study.

-P14, L439: The reviewer cannot understand the term "primary" in L439. Can correlation analysis among diacids and related compounds give information regarding primary emission of C2?

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12, C10249–C10254, 2012

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-P14, L446: Can correlation analysis among diacids and related compounds give information regarding formation of C2 from BVOC via aqueous phase reactions? Too much speculative.

-P15, L458-459, L466-467, L480-482: Three sentences are conflict each other. Authors mentioned that biomass burning is important for the production of C2 and related compounds in L458-459 but not important in L466-467 and again important in L480-482.

-P15, L470-472: The reviewer cannot understand how ratios of dicaids to biomass burning precursor between two observation sites can give the information regarding local and regional transport of biomass burning aerosols.

-Section 3.7: The interpretations of PCA analysis are quite vague. In the reviewer's opinion, 10 and 11 data set are not enough for PCA analysis. Thus, the reviewer suggests deleting whole section 3.7 and corresponding tables and figures.

-Section 4, Conclusion: It is better to summarize major findings of the study and their interpretations. For example) -Mass concentrations of C2, C9, wC9, and fatty acids in coarse mode (PM10-PM2.5) are highly enhanced during the wet season than the dry season. -Two biomass burning tracers showed different regression patterns with total diacids in PM2.5 during the wet and dry seasons.

-Figure 6: C6 concentration in PM2.5 is much higher than those in PM10 during the dry season. Because PM10 means sum of PM2.5 and coarse mode (PM10-PM2.5) particles, mass concentrations of compounds in PM10 should be higher than those in PM2.5. There might be errors on data processing. Similarly, Ph concentrations in PM2.5 are much higher than those in PM10 during the wet season.

-Figure 10: nss-K+ showed different regression pattern with total diacids in PM2.5 during the wet season. No correlation was observed. However, levoglucosan correlate well with total diacids in PM2.5 during the wet season. Can authors explain reason of

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12, C10249–C10254, 2012

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this result in the text?

Minor comments

-Throughout the MS, authors have to check subscript and superscript.

-Please check abbreviation. AVG, S.D., MIN, MAX, etc in text and tables.

-P2, L26: "gas chromatography (GC)" ⇒ "gas chromatography (GC)/ flame ionization detector (FID)"

-P2, L31: The term "C2" have to be defined first.

-P2, L37: "higher to" => "higher than"

-P3, L57: Remove a comma after dot.

-P3, L68: Add "and" after a comma.

-P4, L87: "reading" => "leading"

-P4, L116: Remove a parenthesis in the phrase (PM2.5 and PM10)

-P5, L147: Add "/Flame Ionization Detector (FID)" after "HP 6890"

-P6, L158-160: It is better to delete lines starting "Lower field blanks . . . Yasui (2005)."

-P6, L183: It is better to remove the web address of HYSPLIT.

-P7, L205: Add "and" before "29.2.4". The term "29.2.4$\pm$164.8 ng m-3" should be corrected.

-P14-16, L447-461: The authors mentioned that OC and EC can be used as a tracer of biomass burning. However, the reviewer cannot understand how EC can be used as a tracer of biomass burning. EC alone cannot be used as a tracer of biomass burning whereas elevated OC/EC ratio can be used as a tracer of biomass burning.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 25657, 2012.

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12, C10249–C10254, 2012

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