

## ***Interactive comment on “Aerosol and VOC emission factor measurements for African anthropogenic sources” by Sekou Keita et al.***

### **Anonymous Referee #2**

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This work presents the Emission Factors (EFs) from fuel combustion in West Africa estimated from field and combustion chamber measurements. EFs were estimated for black carbon (BC), primary organic carbon (OC), total particulate matter (TPM), and 50 non-methane volatile organic carbon (NMVOC) species. In addition, measurements in combustion chambers were used to estimate particulate EFs by size, namely for PM<sub>10</sub>, PM<sub>2.5</sub>, PM<sub>1</sub> and PM<sub>0.25</sub>. This work was conducted within the framework of the DAC-CIWA (Dynamics-Aerosol-Chemistry-Cloud Interaction in West Africa) FP7 program. Field measurements were conducted in two places of West Africa, namely Abidjan in Ivory Coast and Cotonou in Benin, while measurements in combustion chambers were conducted in Toulouse, France, and in Edinburgh, United Kingdom. The considered emission sources are wood and charcoal burning, charcoal making, open waste burn-

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ing and vehicles including cars, trucks, buses and two-wheeled vehicles. As mentioned by the authors in the introduction, one of the sources of uncertainties in emission inventories are uncertainties in the emission factors and therefore research contributing to reduce these uncertainties are necessary to better quantify the impact of given source sectors on air quality and climate. However, although I recommend this paper to be published in ACP some important aspects need to be addressed before its publication.

### General comments

1. The authors mention the role of uncertainties in the uncertainties of emission inventories (EI) in the introduction, however they do not address this issue again in the rest of the manuscript. How does this work contribute to reduce uncertainties in current EI in Africa? What are the main sources of uncertainties in EF in the region?
2. After reading the article it is not clear to me what the real contribution of this article is. The authors make an attempt of this in the last paragraph of the conclusions but not supported in the text. The authors should make an effort and discuss the implications of their results for current Emission Inventories. Are emissions currently over or underestimated if these new EFs are considered?
3. It is not clear to me that the title is the most appropriate considering the scope of the study. Measurements are conducted only in two places of tropical West Africa but the authors claim (not explicitly) that the results are valid for Africa. How representative of other countries in Africa and/or West Africa are these results? Can they be extrapolated for the entire region? The authors show with their results that EF are sensitive to multiple factors therefore applying these emission factors to other countries is not straightforward. The scope of the study should be consistent with the title and the content of what is presented. It is not only a matter of the title but also how the data are presented and their representativity.
4. The authors are not thorough when presenting the results and formulation is unclear in certain places. Authors should review the manuscript for consistency and improve

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formulation. For instance, in page 9 the numbers provided from lines 5 to 7 do not match the numbers given in the table indicated in the text. Also in page 11, line 18, the values in the text are approximated to one decimal while in the table it is with two. Authors should review the text and correct inconsistencies between the data provided in the text and the ones given in the tables and figures.

5. The authors make an attempt to put the results in perspective in the last paragraph of the conclusions but without elaborating on it in the text. The authors should elaborate on the impact of the EFs and provide more evidence on the importance of their findings for current and future emission inventories.

6. Authors should make sure that all methodological description is presented and described in section 2. At present, section 4 also includes important methodological aspects in terms of composition of sources and how the data are combined to obtain total EFs per source. This should be moved to section 2 and more information should be presented. It should be made clear to the reader how the data are aggregated.

#### Specific comments

Page 3, lines 18-21: formulation is unclear and should be improved.

Page 3, lines 22-25: Unnecessary information is provided on work packages of the DACCIWA project but no general information of the project itself is provided. I suggest removing the unnecessary information and include a short description of the project and then linking it to the work presented, without necessarily tying it to a WP.

Page 3, lines 31-33: Section numbers are not correct, in addition the paper has five sections and only two sections are presented and described. The authors should complete this paragraph and make sure that section numbering is consistent.

Page 4, line 1: remove “in the frame of the DACCIWA WP2” since this information has already been provided and information on the WP is not really informative and relevant.

Page 5, lines 4-7: formulation is unclear and should be improved.

Page 5, line 31: What is the unit of the provided uncertainty, is it % or ppm?

Page 5, lines 2-23: The description of the field measurements needs to be improved. It is unclear whether the same measurements were conducted in each one of the 3 field campaigns or if they were different. For instance, when it is said that eight measurements were carried out at Akouedo was it in each one of the three campaigns or only in some of them. Please clarify, manuscript should be clear in terms of the measurements that were conducted in each campaign.

Page 5, line 27: Provide a reference of the instrument QTRAK-7575.

Page 6, section 2.4: Section 2.2 indicates that two types of African hardwood were tested in combustion chambers, namely Hevea and Iroko, however in section 2.4 only Hevea is mentioned and it is not clear whether Iroko was tested at all. Was Iroko included in the study? Results corresponding to wood burning correspond only to Hevea? Please clarify.

Page 7, line 9: Section 3 should in fact be part of section 2. Why do the authors consider it should go in a section by itself?

Page 7, line 26: ...in Detourny et al. (2011) and Ait-Helal et al. (2014).

Page 8, line 20: First table mentioned in the text is Table 7! But no mention was done before to the previous 6 tables. Numbering of the tables should be done in order they are referenced in the text.

Page 8, lines 20-21: What exactly is meant by this? Does it mean that in addition to the number of measurements indicated in section three additional measurements were conducted to reflect reproducibility or are these three measurements part of the total number of measurements conducted? This should be made clear and should be included in section 2 and not in the results and discussion section.

Page 8, lines 21-22: What are the arithmetic and geometric method the authors mentioned, is it just averaging? Again, this should be made clear and included in section 2

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where the measurements are described. See general comment made above.

Page 8, line 26: DL and MO have not been defined in the text. They are defined in the caption of figure 3 but should also be defined in the text.

Page 8, lines 28-29: How was the MCE of biofuels from 0.6 to 0.9 from Iroko wood obtained? Was it from the combustion chambers or from the literature? This should be clarified.

Page 9, lines 1-2: the  $\Delta\text{CO}/\Delta\text{CO}_2$  ratio has not been introduced as a quality indicator before, the statement should be reformulated.

Page 9, lines 2-4: remove the coma after 0.32 and replace “showing” with “show”.

Page 9, lines 5-7: Values provided in the text do not correspond to values in Table 1!!! This needs to be corrected and the authors should make sure that the numbers given in the text are consistent with those in the tables and figures.

Page 9, line 15: Three sources are mentioned but only two symbols are provided in parenthesis. Furthermore, the authors make a too simple analysis of the results presented in Figure 4. Above 0.2 BC/TC one could agree with the authors that the larger the  $\Delta\text{CO}/\Delta\text{CO}_2$  ratios the smaller the BC/TC ratio. However, below 0.2 BC/TC the data suggest that regardless of the  $\Delta\text{CO}/\Delta\text{CO}_2$  ratio, the BC/TC ratio is mostly constant. The authors should elaborate on these two regimes and explain the reasons behind it.

Page 9, line 24: What calculations do the authors refer? How were these calculations made and by whom? Authors should provide more information about this.

Page 9, line 27: Hasn't there been anything more recent than the references provided in this line? The authors should look for more recent EFs estimates.

Page 9, lines 27-28: The literature review of EF in West Africa is not thorough enough to make this kind of statement. Make the statement relative to the studies included in

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table 2 and not general.

Page 9, lines 31-33 and Page 10, line 1: Very little is said about Charcoal EF in contrast to wood and charcoal making. The authors should elaborate more on Charcoal. Also, the authors should make it a separate paragraph from wood and not split it on two paragraphs as it is now.

Page 10, lines 4-5: Put parenthesis after years of publication for each reference.

Page 10, line 19: Again the values given in the text do not correspond to values provided in Table 3 and here again it's a matter of significant numbers considered in the text and in the table. The Authors should check the paper for consistency and use the same criteria with regards to significant numbers when presenting results.

Page 10, line 19: I do not agree that the estimated EF(BC) for new LDGVs is within the range of literature values. It corresponds in fact to the lower limit of the range of values provided in the table. The authors should make the statement consistent with the data in the table. Also, it is unclear whether the literature values presented in the manuscript are from the same country or region or from elsewhere in the world. Although the measurement method is presented, not the country where the study is conducted and this information should be provided or at least considered when comparing the estimates.

Page 10, line 30-31: Where are these percentages of old and recent vehicles taken from? Are they based on statistics from the literature, governmental documents, etc? The authors should explain where these numbers come from. Furthermore, this should be included in section 2.

Page 11, line 7: Again where are these numbers of 77% of light duty vehicles and 23% heavy duty vehicles? Same as comment before, these numbers need to be justified somehow and also moved to section 2.

Page 11, lines 8-15: Again values presented in the text do not match those in the corresponding table referenced in the text (Table 4). Furthermore the data are compared

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to different COPERT EURO Standards without even presenting them and explaining them. Furthermore, why are these data included in table 4? I would strongly suggest the authors to rewrite this analysis including the different COPERT values in table 4.

Page 11, line 18: Again value of 26.0 +- 1.10 g/kg given in the text is not the same as the one given in table 5 (25.71 +- 1.1). If the authors decide to use a certain criteria of significant numbers for the table, they should use the same for the data presented in the text. There is absolutely no reason why numbers should not be exactly the same. Again, please be consistent!

Page 11, lines 19-20: What exactly is meant in the sentence “The same difference is observed for old...”. The difference in EFs(OC) in fact for old cars between two and four stroke is much smaller than for recent ones. The authors should correct the analysis and make it consistent with the data presented by them.

Page 11, lines 20-21: The statement that two stroke engines emit more OC than four stroke engines is made only for old vehicles, why is not the same analysis done for new vehicles? Also, the data to support that claim is not the OC/BC ratio but the actual EF, authors should correct this.

Page 11, lines 27-29: Where are the percentages of two and four stroke engines taken from? As before, these numbers need to be justified and also be moved to section 2. Also, the Table 5 is referenced here, but shouldn't it be table 4? Finally, rather than saying that the values of this work are in agreement I would suggest to reformulate and say they are comparable.

Page 15, lines 21-22: Authors conclude on the dependence of EF from traffic to vehicle age and maintenance, although the latter was not included in any of the analysis presented. The authors should base the conclusions to the results presented in the article. I suggest maintenance is removed from the conclusions or the corresponding data to support that claim are provided.

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Page 16, line 6: Authors claim that EFs obtained are representative of African sources but do not provide any evidence or analysis of this in the paper nor a reference to support this claim. I suggest the authors provide some evidence to support this statement or remove it from the conclusions.

Page 16, lines 7-9: “This unique database. . .” why is the database unique? No mention to its uniqueness has been made before. How will it improve emission inventories in Africa? What will be the impact of these database on emission estimates? Again, no mention of the impact of these EFs on emission inventories is made in the text to support this. Finally, how will this new EFs help decision makers? Wouldn't this usually be come from new emission inventories? These kind of statements, although tempting should be supported by the facts presented in the text which I believe are not. Please reformulate.

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Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2017-944>, 2017.

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