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Atmospheric Chemistry and Physics Discussions

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Interactive comment on "Personal exposure to $PM_{2.5}$ emitted from typical anthropogenic sources in Southern West Africa (SWA): Chemical characteristics and associated health risks" *by* Hongmei Xu et al.

Anonymous Referee #2

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Megacities in Africa are pollution hotspots, for which very little data have been published. Personal exposure in such environments have also received virtually no research attention. Therefore, the authors are commended for this work.

General comments: I regard the language in this paper as poor. In many instances, this prevents proper knowledge transfer to the reader and can cause misunderstanding/interpretation of the text. Additionally, it makes the paper difficult/cumbersome to read. It is not the job of the reviewer/editor to do language and/or text editing. In my opinion, this paper should not have been published in ACPD before the language and text was acceptable. Therefore, I recommend that final review of this paper should only be considered once the language/text is improved. In the current form, too many uncertainties exist in the paper, because of the poor language.

I am also not 100% convinced that the content of this paper fits into the scope of ACP. According to the journal, ACP "... is an international scientific journal dedicated to the publication and public discussion of high-quality studies investigating the Earth's atmosphere and the underlying chemical and physical processes." My uncertainty arise from the fact that this paper is focussed more on personal exposure and not on "... underlying chemical and physical processes." Would the paper not fit better into a journal specifically considering atmospheric exposure and/or health impacts? I leave the decision to the editor. This comment should not be considered as negative in any way and it is also not a reflection of the science presented.

Specific comments: The authors must please not use the name "South West Africa" as they did in line 105, but rather keep to the term "southern West Africa", as it the rest of the paper. "South West Africa" was the name for modern-day Namibia from 1915 to 1990. I would even go so far as to recommend that the term "southern West Africa" that is abbreviated at "sWA" ("southern" in lower case) be consistently used, instead of "Southern West Africa" that is abbreviated at "SWA" ("Southern" in upper case), to ensure that the reader does not confuse the area investigated with "South West Africa" that was abbreviated at "SWA".

The author refer to "... garbage spontaneous combustion..." a couple of times. Is the garbage really combusting spontaneously, of are garbage dumps being set alight to reduce the volume of waste, to reduce pests (rats and mice) and prevent disease?

The quality of the Google Earth images presented in Figure 1 and the photos presented in Figure 2 are not good and might deteriorate further in page setting during publication (e.g. if the images are printed even smaller). I encourage the authors to ensure the best possible quality for these images.

In its current form, the paper is long. If the authors and editor agree, I would suggest that Appendixes A, B, C and D, which present the questionnaires, rather be included as supplementary material, instead of appendixes. Appendixes are published as part of the paper, while supplementary material are published separately. Readers who want to assess the content of the questionnaires can download the supplementary material, instead of the paper becoming excessively long.

I agree with Referee #1 that the authors cannot interpolate their results obtained from individuals with specific occupations and at specific locations to the wider southern West African region. All such statements should be revised.

In general, there is little comparison of the results presented in this paper to results obtained elsewhere. I appreciate that very little, if any, personal exposure data have been presented for African cities. However, even if the results presented are compared to ambient/indoor air quality results obtained in the rest of Africa (or Asia, or some other developing settings, if African reference cannot be found), the reader will be able to easier contextualise the exposure concentrations reported here. For instance, indoor air quality in semi- and informal settlements (low-income households) in South Africa (Kapwata et al., Atmosphere 2018, 9(4), 124; https://doi.org/10.3390/atmos9040124) could be compared to "Night" personal exposure of individual in this study. Also, characterisation of the plume of fire grilling of meat in an African context (Venter et al., S. Afr. J. Chem., 2015, 68, 181–194; DOI: http://dx.doi.org/10.17159/0379-4350/2015/v68a25) could possibly be compared to the exposure of woman by Domestic Fires (DF) ("grilling meat or roasting peanuts") in this study. Such comparisons will help the reader to contextualise the results presented – currently only comparing the different groups with one another does not enable the reader to contextualise the results. There might be many more references, such as the afore-mentioned, these two are just examples that I found with a quick online search.

Line 415. The author state that "The previous studies (Cao et al., 2008; Li et al., 2009; Tian et al., 2017) suggested that average OC/EC characterizes 1.1 as motor vehicle exhaust, 2.7 as coal combustion and 9.0 as biomass burning. The OC/EC in the present study points out that biomass burning emission was the main contributor to carbonaceous aerosols for women at DF, and the mixed emissions from biomass and coal burning, even or/and motor vehicle exhaust dominated the carbonaceous aerosol sources for students at WB and drivers at MT." However, the authors should clarify these statements, since OC/EC ratio will change in a plume with aging, with the formation of secondary OC and deposition of EC. Therefore, if the above OC/EC ratios are used to characterise fresh emissions/plumes, it should be stated as such and not left to the reader to interpret.

Line 247. Fe and the heave metals reported (V, Cr, Mn, Co, Ni, Cu, Zn, Sb,473 Ba and Pb) were analysed with ED-XRF. How sure are the authors that some of the heavy metals were not part of the GM and are therefore partially double accounted for in the mass balance (Figure 5, line 493 onwards), i.e. accounted as heavy metal mass and also contributing to the mass of the GM?

Line 511. Although the authors give a citation (i.e. "Taylor and McLennan, 1985") to support the use of Fe as a tracer for geological material (GM), how accurate is this method? The authors state "Fe constitutes about 4.0% of the Earth's crust in dust of the earth's crust (Cao et al., 2005)". Are there any indications of Fe contents of local soils (and the variation on Fe contents) and how it differs from the global average value of 4%, which was used. Basically, I am asking how accurate the method is. Can the authors give any indication of accuracy? This is important, since "... it is found that GM contributed 35.8%±2.1%, 46.0%±3.7% and 42.4%±4.7% of PE PM2.5 mass concentrations for women at DF, students at WB and drivers at MT, respectively."

Line 529 "From Figure 5, evident diurnal distinguishes are observed in two major chemical compositions (OM and GM) in this study. We can see that GM exhibits the lower proportion at night (35.3%) than daytime (47.5%), indicating its close relationship with human activities." However, does meteorology not also play a role? In Line 483 the authors imply that precipitation is higher during night-time, i.e. "... spontaneous combustion of waste occurs frequently during the day, because of less precipitation and higher air temperature at daytime. . .”

Line 539 “. . . due to the influence from the damp wood burning at the working time.” I could not find any place where the wood moisture content was reported. Therefore, this statement and previous, as well as subsequent deductions, based on this statement, are not fact based. However, I do agree with later statements (line 566) that the wood will be damper in the wet season, i.e. “. . . increase in humidity (moisture content) of the wood used for grilling meat in wet season. . .”.

Line 584. “Students at WB: nighttime PE PAHs were higher in dry season and lower in wet season compared with daytime levels, with the average D/N ratios of 0.7 and 1.8, respectively. The higher concentrations of combustion markers-BbF and BeP were observed during the day, while the higher concentrations of gasoline vehicle emission markers-DahA and BghiP were found at night (Baek et al., 1991; Wang et al., 2006), which was related to the garbage truck for waste transportation from city to the landfill during night.” I am not sure that the latter explanation can be so simple, i.e. only due to “garbage truck”.

Line 706. Can the authors please explain the selection of species included in the “Non-cancer risks”, wherein only “four heavy metals (Mn, Ni, Zn and Pb)” were considered?

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