

Interactive comment on “Mass concentration, optical depth and carbon composition of particulate matter in the major Southwestern Africa cities of Cotonou (Benin) and Abidjan (Côte d’Ivoire)” by Julien Djossou et al.

Anonymous Referee #2

Received and published: 14 January 2018

This manuscript presents aerosol (PM mass, EC/OC and AOD) measurements from two cities in West Africa, namely in Cotonou and Abidjan. These are multi-year continuous datasets for an understudied region, and are thus very valuable and provide new information. The current version of the manuscript presents the data, however is limited in analyses and interpretation of these data. Because of this, it is not clear to me what the main finding(s) are yet of this manuscript. I do believe that a manuscript that only presents aerosol data is not within the scope of ACP. However, I do believe that with further analysis and interpretation, this manuscript can be improved to the

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level of ACP. I would recommend major revisions and additions before it is resubmitted for review. I do believe the potential of the dataset is very relevant to ACP. The dataset is novel and very important.

I would recommend that the title is changed to avoid confusion. To me "southwestern Africa" is the region around Namibia. A suggestion is "southern West Africa..."

Specific Comments

There is not adequate reference to previous literature, especially related to AOD measurements and previous campaigns. There is a strong AERONET network in West Africa that has been analysed in many papers. For example, Horowitz et al., (2017) provides a seasonal analysis of long-term AOD measurements; this paper and the references therein may provide a good starting point for additional literature on AOD measurements in this area. In addition, I think that it would be helpful to include a summary of the other studies mentioned in lines 77-79. These are stated to be rural studies, but they may help to put these results in context, especially if the AERONET sites are in same region, as some sources of pollution that drive max concentrations as per the manuscript (such as dust and biomass burning) are regional. Finally, a quick google search did find some previous literature on Cotonou that, while from 2006 and earlier, could help to provide some background on the knowledge of pollution in the areas, the impacts of air pollution, and perhaps how it has changed (Fourn and Foyomi, 2006; Fanou et al., 2006, Boko, 2003).

Line 91, what is the cite for the large impact of 2-wheel vehicles? This difference does form the basis of some of the analysis, and thus the reference for this is critical.

Line 92, the seasonal cycle of emissions should have a reference. Is there no known seasonal-dependence of domestic fuel burning?

I would recommend that section 1 (line 102) is moved into the Methods section.

Figure 1, I would recommend that a box is drawn to highlight the region that was consid-

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ered for the burned are (this is stated in lines 277, but it would help to see it visually). Also, I would recommend that the authors consider mapping the cities as well in a zoomed in map in figure 1. It would be helpful to see where they are relative to each other and how far apart they are. Figure 1 also needs a scale on the map to define distance and relate it to km.

Line 123, was the flow measured constantly? This is an important measurement for the calculation of concentration, so it would be helpful to give more information on how it was measured, how often, was it steady, etc?

Line 126, how were the samples stored after collection and before analysis. Where was the analysis performed exactly?

Line 142, a cite is needed for the statement that the public dump negatively affects the environment and people. Line 144, how often does burning occur?

Line 144, at what sites exactly were the AOD measurements taken? I would include this on the zoomed in addition I recommend to Figure 1. Was it close to one of the sites already explained? Line 147, how consistent was the time when the AOD measurements were taken? Where they at exactly 13:00 UTC every day? Is this consistent local time throughout the sampling period (i.e. no change for daylight savings time)? What is the diurnal variation of AOD expected to be at the site? If there is a diurnal cycle, then relatively small changes in sampling time could have an impact on AOD measurements. In Line 181 it is stated that these are considered daily AOD values, but how can one point in the day capture the daily average since I assume there must be variability within the day?

Line 148, were measurements “performed” or “analysed” only for cloud-free days? If it is performed, then how in the field did you determine a cloud-free day to only take a measurement on cloud-free days? If it is analysed, what data did you use to determine a cloud-free day?

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Line 154, where did the weighing occur? At site?

Line 171, for the detection limit, what are the units (i.e. ng of what? Carbon?). What does the uncertainty (+/-) refer to?

Line 182, why was the AOD changed to 550nm from 540nm?

Section 2.4, the bullet list does not flow well, I would recommend that these are paragraphs, and that you include more information for each point. For the weather data, was there any validation of the NOAA measurements (either in this study or previous studies)? How well do they perform for this region? Are there any local measurements that can be used? What site is the NOAA data for (coordinates)? How close are these sites to the aerosol measurement sites? For MODIS data, how and where did you access it?

Figure 2 is small and blurry. Is the box temperature and RH controlled? This box sits outside at all sites, correct?

What quality control and assurance procedures were applied to the measured data. How many data points were collected and how many remained after these procedures (for every measurement)?

Section 3.1, I would recommend this section is expanded. As stated above, there is a network of AERONET sites in the region, the past data could provide context for this site and a comparison point(s). The temporally coincident data during this campaign could provide additional information for the analysis of the source of the aerosols, as well as a comparison point(s). For example, what is causing the seasonality? Also, what impact might having the measurement at 13:00 have on the analysis (i.e. impact of diurnal cycle)? Line 200-202 is not complete.

Line 204, is dust the only driver? What about difference in aerosols aloft? Would that be expected?

Figure 4, I would recommend adding the vertical line for W1, D1', etc. as in other

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figures. I do not believe the “(-)” is necessary as per ACPD style rules.

Line 207, these are time series of weekly PM_{2.5} mass, correct? I would recommend that the authors are very precise in the use of the averaging time throughout in order to avoid confusion. This is particularly important in comparison to WHO guidelines and other studies.

Line 213, what explains the peak times at ADF?

Line 218 provides an example of the need for more interpretation that I referred to in the opening comment. The PM mass concentrations are stated, however there is not then some information on what this might mean.

Line 221, since the site was not downwind from the dump, and the data do not show impact from waste burning, I would recommend the authors consider changing the name of the site from the beginning. I would think it would make sense in the methods to state that this site was originally placed in hopes of capturing waste burning, but did not in the end, so it is labelled something other than waste burning. I do believe that referring to it as “waste burning” throughout the manuscript can lead to confusion.

Line 224, I would recommend that the authors include the OC, EC and TC time series in a figure as it would be helpful to see the time variation in these. This could be added to figure 5.

Figure 5, AT and CT are similar in PM_{2.5} mass but not AOD, that is interesting, why might that be? Are the spikes (i.e. ~150 ug/m³ in AT in W1' and D1 in CT etc) 1-week spikes? Or are they multiple weeks? Are they large-scale events? Why are these sites so similar even though they are impacted by different local emissions and the CT site is very far away?

Line 252, how was the humidity of wood considered to be the largest driver of this pollution? This needs additional analyses to prove that this is the case, or citations from literature to prove this is the case.

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Line 255-261, this is an interesting analysis. However, the relationship may be easier for the reader to understand if this is graphed (e.g. AOD vs PM_{2.5}), as if these are linearly related or not also provides information.

Line 262, I would recommend that a more detailed analysis of wind speed (and direction, if possible; e.g. wind roses) is performed of the sites to see if these analyses provide any information that is helpful to characterize the drivers/sources of pollution.

Line 270, cites are needed for the statement on impact of dust and biomass burning. Low Angstrom exponents can also come from sea salt aerosol, and as these are coastal sites, there is a chance they may be impacted.

Line 275, the lowest AE are actually in W1 and not a dry season. Why might that be? Cites are needed on why 0.8 was defined as coarse particles.

Line 275-283, I would recommend that the EC/OC results (and possibly AOD) are used in understanding the impact of biomass burning emissions on PM.

Line 284, the OC/EC ratio seasonal cycle is not very pronounced and there are large variations in the data per averaging period (Figure 6). In Figure 6 caption, please include an explanation of what level/percentile the boxes and lines reach to.

Figure 8, the AOD values do not seem to be the same as in Figure 4. In Figure 4, Cotonou goes above 2, but not here. Also, I would recommend using the same colors as in Fig 4 for Cotonou and Abidjan to avoid confusion. I also would recommend extending the W1, D1, etc lines to all figures.

Table 1, I would recommend having a column showing how many days in each season. This would help to understand the rainfall measurements as they are total per season, however the seasons (as shown on the figures) are not all the same length.

Table 2, why is there no std dev reported for the ratios? I would expect this to also be reported. I would recommend showing the number of points for each measurement in each season.

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I would imagine with the additional analysis that the discussion and conclusions would change, and thus won't comment extensively. As stated above, I would recommend that the averaging period of the measurements is always stated to avoid confusion (e.g. are all studies reporting annual averages?). Also, I do find comparison of these measurements to other sites helpful, however I would recommend that the reason for using these selected sites as comparison points be discussed (e.g. other African cities, European cities, rural areas, etc.).

Line 329, "norm" should be "guideline"

Line 339-340, what is the link between variability and diesel use?

Technical comments

Line 49-53 is a long sentence, I would recommend re-wording.

Line 73, biomass burning and burning seems repetitive. I would recommend qualifying the second burning with what type of burning it refers to.

Line 81, I would recommend deleting "and especially the work package 2..." I think this is specific to the structure of DACCIWA and not needed in this article, as readers who are not in DACCIWA do not know the work packages.

Figure 7, a legend would be helpful. The units of rainfall on the axes should be more precise. Is this total mm per week?

Line 306, which "fuel" is this referring to?

Line 301, "precisely" should be removed.

References

Horowitz et al., (2017), Evaluation of climate model aerosol seasonal and spatial variability over Africa using AERONET, ACP

Fourn L, Fayomi EB., Air pollution in urban area in Cotonou and Lokossa, Benin, Bull

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Fanou et al., Survey of air pollution in Cotonou, Benin—air monitoring and biomarkers Science of The Total Environment, Volume 358, Issues 1–3, 1 April 2006, Pages 85-96

BOKO, Gbètoho M. Joachim "Air Pollution And Respiratory Diseases In African Big Cities: The Case Of Cotonou In Benin" in Martin J. Bunch, V. Madha Suresh and T. Vasantha Kumaran, eds., "Proceedings of the Third International Conference on Environment and Health, Chennai, India, 15-17 December, 2003. Chennai: Department of Geography, University of Madras and Faculty of Environmental Studies, York University. Pages 32-43

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