Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2017-973-RC1, 2018

Atmospheric Chemistry and Physics Discussions

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 #1

Received and published: 9 January 2018

This work reports on in situ measurements of two locations in west Africa that include daily integrated filter measurements of PM and a daily measurement of AOD. This region is very much understudied, and these are an important set of measurements. However, there are a number of significant limitations to this work, addressed below, that need to be evaluated and considered for further consideration by this reviewer. These include concerns with extensive editorial revisions, a generally superficial analyses of a novel dataset, and a need for additional methodological information for this data to be interpretable. Though the authors should be commended for collecting a

reasonable extensive primary dataset, the interpretation and utility of this data, as presented, is quite limited. It is my view that this would should be rejected, as the needed revisions are probably too extensive to warrant publication in ACP.

Specific comments are as follows. Where appropriate, line numbers are offered.

Major comments: In a number of cases, the authors focus on the importance of dust contribution to haze in the region, which is a reasonable interpretation given proximity to Sahara, and the presence of regular Harmattan. Yet, the focus on chemical analysis is on EC and OC, typically a trivial fraction of crustal materials. While the available of EC and OC data can be useful for assessment of biomass burning, anthropogenic combustion, and the like, it does not make sense to use them to explain crustal affects on PM. On lines 90-92, the authors seem to conclude that PM anthropogenic emissions from primarily from '2 wheeled vehicles' whereas 'cars and buses' dominate emission in Abidjan. Is there any evidence to support this statement?

Please describe the sampling approach you took to collect PM2.5. Where appropriate size selection devices (cyclones, impactors) used? Given the semicontinuous monitoring scheme, what was used to assess flow rate to ensure accurate size selection and total volume sampled?

At the AWB site near Abidjan, the samples were collected at 12 meters (line 140). Is this accurate? How can we be sure ground-based emissions from the waste transfer industry were captured by a sampler located so high off the ground? And how frequently was this burning conducted?

In the methods section (line 171), the authors report EC, TC, and OC detection limits that includes an uncertainty. It is unusual to report uncertainty with imputed detection limits, which are typically defined as three times the standard deviation of a blank, or some other standardized approach. In either case, more information towards how these MDLs were calculated is necessary.

In Lines 197-200, the authors seems to concluded that AOD is different between the two locations based chiefly on population size differences between the two. This statement needs to be supported with evidence from either the literature, or the data that was collected.

More information is needed on sampler placement at each location (pictures in the figures, though aesthetically interesting, are not particularly helpful). Further, presuming these samplers were stationary, it would seem to make sense if the authors would conduct some sector analyses where known wind speed/direction is used to censor data to show how concentrations change when the sampling is likely downwind of the source, or when it is not. The authors have lumped all data together, and any useful signal is likely to be lost to averaging or noise.

Did the authors make any assessment (modelling or otherwise) of high altitude aerosols which would affect AOD, but not ground-level PM?

The percentage of OC and EC to PM2.5 contribution (Lines 299) are unusually high. For example, at the ADF site, 11% of PM was from EC, and 51% was from OC. In order to be more meaningful, OC really should be converted to OM (typically 1.5*OC = OM, though it does vary). If one makes this assumption, then more than $\sim$90% of PM2.5 is explained by OM and EC, leaving very little for crustal, metal, or ionic contribution to PM2.5.

In Lines 311-359, the authors make comparisons to other sites found around the world (Paris, Milan, Morocco, Agra, Querol, etc). What is the point of this comparison? How does this support your findings? The conclusions in this work are largely unfocused, and are overly generic. The authors, quite unexpectedly, include new arguments (Line 391) to suggest that 'humidification of wood fuel' is driving the relationship at one location. There is only a limited analysis of AOD measurements, which could be an important and useful lower-cost measurement of PM25 in this locations. But as presented, one only sees a time series of AOD. It would be far more useful to include back

trajectory analyses or receptor models to support your data.

Minor comments: Some of the information in the introduction (e.g. lines 60-65) is too elementary.

Missing references for recent campaigns listed in lines 77-78 and for the IMPROVE protocol used (line 161)

Were the filters (line 120) used actually Teflon filters (which are trade names of a specific brand) or more generic PFTE filters?

Unfortunately, the paper needs to be deeply edited by a native-english speaker. There are many instances of typos, unfinished sentences, grammatical errors, and improper punctuation; these are probably too numerous to completely list here. Identified errors include lines: 30 31 45 47 87 106 108 142 200 201 223 310 317 318 368 372 384 388

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