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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Remote biomass burning dominates southern West African air pollution during the monsoon" *by* Sophie L. Haslett et al.

Anonymous Referee #1

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This paper discusses airborne aerosol particle measurements over a coastal region from Cote D'Ivoire to Benin. Three extensively-equipped research aircraft are used and data is collected from marine as well as coastal and inland regions during a three-week campaign in 2016.

The focus of the analysis is on chemical composition measured with AMS (on board three aircrafts) and size distribution measured with SMPS (on board one aircraft). The main finding of the paper is that aerosol chemical composition and accumulation mode size distribution within the lowest 2km are very similar over the ocean and inland. This is an important finding as it characterises a significant regional background aerosol load, on top of which urban emissions are emitted. However, the claim that this background aerosol originates from central and southern African biomass burning is not

supported by the data.

Major comments

The average chemical composition observed for <4km aerosol has high SO4 (and NH4) content compared to biomass burning smoke observations - see e.g. Capes et al. (2008) and Vakkari et al. (2014) for observations from African fires. And there is a wealth of observations from e.g. North and South American biomass burning. Actually, the composition of the <4km aerosol is quite close to dry season average composition outside Johannesburg, South Africa (Tiitta et al., 2014). Only the >5km composition looks like pure biomass burning aerosol (OA>80%).

In my opinion the similar chemical composition for urban outflow and upwind marine is an indication of urban emissions being transported over the ocean. A sea breeze circulation can extend up to 150 km from shoreline and could be responsible for such transport, as shown by Flamant et al. (2018) for DACCIWA domain. CALIPSO aerosol profiles might help to show where the observed aerosol layer originates – if it is limited to within 100-200 km of coast or is really connected to the westward transport of central African biomass burning.

Two parameters are missing from the biomass burning aerosol analysis: BC and CO. It would greatly increase the value of this data set if you can provide the PM/CO ratio (and OA/CO) for the different layers, as this value can be (and should be) compared to other biomass burning observations. Likewise, if you have BC (e.g. from aerosol absorption measurements) it would be highly valuable to include it in Fig. 4. BC can easily make 10% of BB aerosol.

Regarding the mass spectra presented in Fig. 5: please apply PMF to the data to check if the same sources contribute as much to the different geographical regions. Please include also >5km BB layer in the analysis.

Regarding COSMO-ART modelling, the only purpose of the model seems to be show-

ing that BB emissions have an effect on cloud droplet size distribution. However, this much is evident from the previous studies cited in the manuscript (see also Rosenfeld et al., 2019). Fig. 8 comparison is relevant only if the model (with BB emissions) gets the aerosol properties correct for the study domain. If you decide to keep the model section, please provide a comparison with observations, and a more detailed description of the model setup. For instance, the model domain (Fig. 2) seems to exclude a substantial fraction of the central African active fires (Fig. 7a).

Minor comments

Line 2. "which releases large concentrations of aerosols into the atmosphere" I think here the amount of aerosol is more relevant than the concentration near source. Same for line 34.

Line 108. Please define the "upwind marine" regime as distance from the shoreline and/or indicate it on the map in Fig. 1.

Line 131. Did you scale the emissions by 3.4 as recommended by Kaiser et al. (2012)?

Line 145. Please define the size range for Aitken and accumulation mode.

Line 164. Are the "N" and "M" for panels b and c correct?

Line 171. Density of 1.3 g/cm3 seems quite low to considering that 40% of the aerosol is inorganic. Please justify the value.

Line 191. Are the CN number concentrations averaged over all CPC's or only for the CPC that had similar cut-off diameter? 14nm cut-off diameter may result in a much lower concentration compared to 3nm cut-off in some cases.

Line 197-201. Please compare with biomass burning plume chemical composition measurements. For regional aerosol Tiitta et al. (2014) measurements in South Africa may be more relevant comparison than global average – please check if there are other more recent observations available.

Line 239-240. "The m/z 44 contribution here correlates well with the total organic mass" Please include correlation coefficient.

Line 279. Should "(a)" and "(b)" be "(b)" and "(c)"?

Line 290-291. I disagree: the large contribution of SO4 suggests significant contribution of non-BB sources.

Line 321-322. "However, results presented here show that in addition, a significant proportion (up to 10%) of the aerosol mass from the biomass burning plume is being entrained into the boundary layer" This, especially the 10%, is not shown here. Please elaborate.

Line 332. The number of CCN is the relevant parameter; aerosol number concentration can be dominated by nucleation mode particles that are too small to act as CCN.

Line 341. Y-label: what is "cloud number concentration"?

Line 407-410. "Previous research has shown that during the monsoon season, submicron particles in southern West Africa absorb moisture and can easily grow to more than double their dry diameter (Deetz et al., 2018; Haslett et al., 2018). This would therefore enhance the aerosol mass loading from these particles, potentially close to the 10 μg m-3 annual exposure recommended by the World Health Organisation (WHO, 2005)." I think that the air quality limits (and epidemiological studies) are based on dry PM. Please check.

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