Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2020-617-RC1, 2020

Atmospheric Chemistry and Physics Discussions

Interactive comment on "PM$_{2.5}$ surface concentrations in southern West African urban areas based on sun photometer and satellite observations" *by* Jean-François Léon et al.

Anonymous Referee #1

Received and published: 28 August 2020

General Comments This study analyzes aerosol properties and PM2.5 concentrations using sunphotometers (handheld and automatic-AERONET) and samples at selected sites (coastal and inland) in the south west Africa. This region is very interesting for aerosol studies due to seasonally changing meteorological conditions i.e, Harmattan north dry winds during winter and monsoon humid south winds in summer. This reverse atmospheric circulation, along with the influence of dust events and forest/agricultural fires result in contrasting aerosol types and properties. From this point of view, the current work may have its own importance, taking also into consideration the very good correlation between satellite AODs and PM2.5 concentrations over selected sites that allows the PM2.5 estimates at a long-term period and examination of the trends. All

the above issues, along with a rough classification of aerosol types are examined in this paper. However, the analysis, discussion, linkage of the present results with previous ones over the region and physical explanations of the aerosol properties related them with seasonality, meteorology and sources are missing or are rather poorly discussed throughout the manuscript. The analysis and discussions for each figure seem rather brief and do not emphasize on important issues of aerosol properties, mixing processes and connection of AOD with surface PM2.5 concentrations. In the following, I have specific comments for authors in a way to improve the scientific quality of the paper. In synopsis, detailed discussion of the results, enrichment of the literature and new analysis in some issues are necessary.

1) The methodology for the PM2.5 estimations from space is not described in detail. Authors should provide the analysis or even the scatter plots between MODIS-AODs and ground-based PM2.5 concentrations for each site and present the linear regressions and the converted factors.

2) The accuracy of the handheld sun-photometer measurements of the spectral AOD is very important, not only for the validation of the MODIS AODs and the PM estimations, but also for the extraction of intensive aerosol properties like the Angstrom exponent... So, these retrievals can be assured as of high accuracy. Authors may check it by applying a 2nd-order polynomial fit to the sun-photometer wavelengths (Eck et al., 2001; JGR). Since the only three wavelengths will give an excellent (R2=1) polynomial fit, the two constant terms A2, A1 may be used and compared with the Angstrom exponent at same wavelength band and in case of very low errors the A2-A1 should equal to Angstrom exponent (Schuster et al., 2006; JGR). You may see this application in Sharma et al. (2014, Aer.Air.Qual.Res), Tiwari et al. (2018, Env.Sci.Poll.Res.) and references therein. With such sensitivity test, you assure the accuracy of the manual sunphotometer retrievals, which may have perturbed due to invisible clouds or even due to not exact matching of the sun disc in the instrument's FOV.

3) Why did you use so long period (1 week) for filter samples? Usually, they are taken

on daily basis... Is this a technical reason or just to smooth the correlations with MODIS AODs, in order to avoid a larger daily variation? It is recommended to write something and discuss it more.

4) A critical point that has to be discussed regarding the measurement time series is the availability of sun photometer observations throughout the year and if there is an extent period (months, or season) with data missing. These large gaps may modify the AOD seasonality.

5) The present one constitutes a rather crude aerosol classification, as also recognized by the authors. It is based on three AE groups, while there are not thresholds at all for the AOD. Alternatively, it is recommended to use the classic AOD vs AE scatter plot for all the examined sites to identify major aerosol types, where the dominance of dust and urban pollution will be defined, especially if such a plot is applied for the dry and wet seasons. That plot would constitute a much better representation of the aerosol types and is highly recommended. There are numerous studies over the globe (some are cited in the manuscript) dealing with such analysis. Finally, the aerosol classification based on AOD vs AE scatter plot is a first rough classification able to discriminate between major aerosol types i.e. biomass burning, desert dust, sea salt, but not between absorbing urban aerosols from various combustion sources, i.e. such a classification is incapable of determining absorption aerosol properties (see Giles et al., 2012, JGR; Cazorla et al., 2013, ACP). Furthermore, at the end of the results, this classification is expanded on long-term periods using the MODIS AE values, which are not accurate enough for such aerosol classification studies and the results and trends may be subject of biases. All these should be clearly discussed in the manuscript.

6) Personally, I do not remember any other study that examined the PM2.5/AOD ratio and also there are no references for such studies here. However, I really doubt about the importance of this ratio, since it is strongly affected by the seasonally changing AOD and PM2.5 values. So, a similar ratio may define very contrasting aerosol regimes of low or high aerosol loading. Furthermore, I do not see a standard variation depending

on season or aerosol type, which may indicate specific characteristics of aerosols in a certain period. For example, in periods with high dust activity, I would expect a lower AOD ratio, since dust is mainly transported above. Also, I would expect higher values for the "urban-like" types, since urban pollution mainly confines within the boundary layer and not in the vertical, so the PM2.5 is usually increasing with higher rates than AOD. However, all these may be highly variable from site to site and here, the averaged values from all the sites mask the results. Furthermore, authors fail to discuss in detail the physical meaning of their results and/or the importance of them. In case they want to maintain this analysis, they should be more detailed in the discussion of the main results and what a low or high ratio value represents. In the current version, this analysis and discussions are not considered important for the paper.

Specific Comments Line 29: Add “in Chad” after depression.

Line 35: You may also see the recent global study about aerosol hot spot regions of dust, polluted-dust and smoke by Mehta et al. 2018. Mehta, M., Singh, N., Anshumali, 2018. Global trends of columnar and vertically distributed properties of aerosols with emphasis on dust, polluted dust and smoke - inferences from 10-year long CALIOP observations. Rem. Sens. Environ., 208, 120-132.

Line 36: You may refer some of the important studies that linked the ARF with the monsoon circulation and precipitation redistribution in the Arabian Sea and India, since such studies are rather rare or even absent in the south west-Africa.

Lines 36-39: These sentences can be merged.

Line 44: Especially for the issue of aerosol-type classification, authors may also see the global study by Hamill et al. (2016; Atmos. Envron.), which also covers the study region.

Lines 53-54: Another study (Sinha et al., 2015; Intern. J. Rem. Sens.) relates this AOD-PM regression with vertical profile of aerosols and meteorological parameters

(RH, Theta, wind) that strongly affect the PM vs. AOD correlation.

Line 63: Add “and” after time series.

Lines 91-92: Revise this sentence.

Lines 105-107: It is not clear if these are your results or other ones from previous validation studies... Please, clarify it. Also, a revision is needed in this sentence.

Line 143: “The overall range of AOD is (0.07, 3.8).” This does not make sense. In what station do you refer here? Also, the parenthesis confuses the reader that something else is missing here.

Lines 148-149: The rather significant variability in the mean AODs at Lamto site due to different periods of the measurements and number of observations necessitates discussing the availability of measurements throughout the year and if a specific season dominates (in number of measurements) against others...

Lines 156-157: Add ", respectively" at the end of the sentence.

Line 160: Revise as "... R=0.82, being R=0.90 between ..."

Lines 167-169: These characteristics should be discussed in view of aerosol sources, transport routes and dominant aerosol types in each season. Only presentation of the results without any explanation about their physical meaning is rather awkward.

Lines 170-172: Also, these results should be further discussed about aerosol types, sources and physical meaning. For example, why AE values are, on average, higher at the coastal stations despite the relative higher influence from sea-salt aerosols that are known to have low AEs? Furthermore, all sites throughout the year present rather low AEs, well below 1.0, except of some few cases, which has to be further commented by authors.

Line 178: Add “values” after RMSE.

Line 192-195: Some further discussions are needed here, regarding the suitability of using MODIS-derived AE values. According to my knowledge and several previous studies, MODIS-AE over land is highly biased since both Deep Blue and Dark Target algorithms used standard models and mixtures of them for the determination of the AE, which is not a measured but a computed parameter. So, the uncertainty increases significantly and this is also shown from the frequency distribution of the AE values from MODIS and sunphotometers. It would be nice, authors to present via graph, or even to give some values of comparison between MODIS-AE and sunphotometer AE in order to reveal the magnitude of the bias.

Line 197: AE does not depend on the aerosol optical properties. It is an intensive aerosol optical property by itself.

Line 209: Correct as “enables”

Line 210: Correct as “category”.

Lines 215-216: This sentence needs revision in English grammar, syntax and typos errors.

Lines 217-218: For aerosol type classification in west Africa, authors may see the results by Hamill et al. (2016; Atm.Env.) about dominant aerosol types at several sites of the region.

Line 229: Correct as “ratios”.

Line 235: Correct as “remain”.

Lines 235-236: Which are the moderate PM2.5 and significant AOD values reported here? There is not a clear view of the levels of these values, which should be mentioned here.

Lines 236-238: This is only true for the "dust-like" aerosols and not for the other types. So, this is not a main findings of the analysis.

Lines 240-241: There is no analytic description of the satellite-derived PM2.5 concentrations. The methodology of these retrievals should be included in the manuscript. This analysis, based on weekly averages, is rather rough and not analytic about the association between dominant aerosol and PM2.5 levels. The scatter plot of AOD vs. AE may also include as third variable (color coded), the PM2.5 concentrations, so the reader would be able to see what type is associated with highest PM2.5. Statistics of such analysis may be also included in a Table. Authors may also provide the seasonality of the aerosol types and correlations between AOD and PM2.5 for each type separately.

Lines 247-251: The current excellent results regarding the PM2.5 estimates from satellite measurements should be compared with other studies over the globe, several of them cited in the Introduction section. In general, there is a lack of any comparison or even discussion of the present results with previous studies in the region. This constitutes a main drawback in the scientific presentation of the present study.

Line 254: It is not clear, since there is no discussion in the method, if authors used a unique PM2.5/AOD conversion fraction for the whole data or seasonal-dependent conversion factors, based on AOD vs PM2.5 scatter plots in each season. In other places over the globe, this seasonal dependence has been shown to be very important for the accuracy of the retrievals.

Lines 255-257: The critical here is to provide information about the PM2.5 levels above 25 μg m-3, which is the threshold for pollution established by EU. Authors should provide analysis about the seasonality of the PM2.5 exceedances and to associate them with the dusty period and the monsoon circulation.

Line 257: Correct as “Maximum is…”.

Line 261: Explanation is needed for the much higher PM2.5 values in Cotonou, which also reflect higher AODs than those in Abidjan. Also, correct as “Abidjan”.

Line 263: “PM2.5 concentrations are then multiplied by a factor of about 3 in 4 months.”. This sentence is rather confusing without a clear meaning. It should be rephrased.

Figure 9: Why did you mix the observations from both sites? I think that this smooths the variability and possibly the trends. At any case, the trends should be examined separately for these sites. Also, authors are based on MODIS-AE values for the determination of the urban-like aerosol type. As discussed above, the MODIS-derived AEs are significantly biased and this should be mentioned in the text. However, in annual basis, the errors and biases are significantly reduced and trends may be examined but with caution. At any case, a full discussion about the biases occurred in such approaches should be given.

Line 265: Correct as “represents”

Lines 269 and 271. This is Figure 9.

Line 271: For such an explanation you may see the differences in MODIS AODs (likely significant lower values in 2018) or in the frequency distribution of the urban-like type. However, the annual PM2.5 and the trends in the two sites should be analyzed separately, so the current figures should be changed.

Lines 272-274: These results should be re-evaluated separately for the two sites. Also, here it is not referred that this increasing tendency corresponds to the urban-like aerosols. Also, there is no discussion about increasing trends in PM and anthropogenic pollution in the urban areas of the region, or even any comparison with previous works.

Conclusions section should be revised in view of the new results and discussions in the text.

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