Anonymous Referee #2

This paper combines satellite (MODIS) and ground-based (AERONET and hand-held)AOD with PM2.5 data to (1) evaluate MODIS AOD and (2) investigate the relationshipbetween MODIS AOD and PM in southern West Africa. Empirical relationships between AOD and PM are created (based on season and an aerosol type proxy) and used to estimate trends in ground-level PM in the region. The topic is in scope for the journal and the subject matter is important. The hand-held and PM data were presented by the authors in a previous paper in ACP (Djousse etal., 2018); this work is a natural extension of that (using the data collected and morein concert with MODIS) and contains sufficient novel material. The quality of language is quite good, and the analysis, while fairly simple in parts, is explained well and uses statistics fairly appropriately (for example the authors acknowledge the skew in AOD distributions and treat this appropriately, while many authors do not). There are some bits that are a bit unclear, I have a few thoughts on the data use, and I found some typographical issues. I recommend major revisions, mainly due to more detail needed in the AOD/PM ratio part. I would like to review the revised version.
Comments are as follows:

Dear Reviewer,
Thank you for your time. We greatly appreciate your review and the references you have provided. The paper has been significantly modified and you will find below point-to-point answers to your review.

First, some general comments and information about the revised version for both reviewers.

- Firstly, we want to make clear about the region. We focus on the northern coastal part of the gulf of Guinea. Western Africa has a marked latitudinal gradient in ecosystems that largely impacts the emission and deposition of particules and trace gases (Adon et al., 2010). We define South Western Africa as delimited by the shore of gulf of Guinea and 9°N in agreement with previous authors (Kniffka et al., 2019). Most the previous studies on AOD observations were performed north of our region of interest, i.e. in the Sahel. The observations we report here are unprecedented in this area of the world so the comparison with previous studies is very limited.
- Reviewer #1 has raised a very important point on the misuse of MODIS AE over land to classify aerosols from AOD observations. So the methodology used for analysing the sun photometer data can't be directly transposed to MODIS record. Nonetheless similar conclusions can be drawn from the seasonal analysis.
- The methodology to estimate PM2.5 from AOD is based on a simple relationship of proportionality between both quantities that depends on the season and the aerosol type. The description of the method has been improved. Accurate estimate of

the impact of the aerosol type on PM2.5/AOD ratio is limited by (i) the weekly frequency in the ground sample while aerosol type can change daily, (ii) the classification of aerosol type using Ångström exponent in a geographical area where mixing of aerosol type is high and pure characteristics are seldom observed. Notheless, the effect of the dust layer uplift on surface concentrations can be observed during the intermediate period when the monsoon onset occurs.

1. As a general point about the MODIS retrievals: the authors use daily level 3 (L3) products (1 degree) from Aqua as the basis for both the comparison with Sun photometers and the PM prediction. For validation against Sun photometers it is more usual to use level 2 (L2) data with an averaging radius around 25 km to decrease discrepancies arising from real spatial and temporal variability. The authors might acknowledge that here. It is probably ok to use the L3 data if the goal is to make regional-scale PM analyses. But the purpose here (and the data collected) seems focused on the two cities. So I would suggest the authors might get L2 data and perform the same sunphotometer comparison to see whether the same patterns hold. I suspect they might but without seeing the data we don't know. I acknowledge that this might not be feasible dependent on the computational resource available to the authors. I suppose doing both is a good way to test whether there is significant variability below the L3 scales.

We agree with the reviewer that validation exercises are usually applied to the MODIS level 2 pixels rather than gridded products. However our objective is not to valide the retrieval algorithms but to address the ability of MODIS data to reflect aerosol changes in a specific area. Doing the validation of both L2 and L3 products will lead to further investigations in the statistical representativity of L3 versus L2 that is not the purpose of the paper.

Please refer to L125 in the revised version.

2. The authors are also using (from their statements) an outdated version of the MODIS product: Collection 6 rather than 6.1. This was released several years ago now (in late 2017: `https://atmosphere-imager.gsfc.nasa.gov/documentation/collection-61`) so it is unfortunate to see Collection 6 still being used. Collection 6.1 has some algorithm updates as well as calibration updates which might affect the results. It would be preferable to repeat the analysis with the latest data version.

You have rised an important point. Our archive was not uptodate and actually a mixed between Version 6.0 and 6.1. This problem has been fixed although with there is no large impact on the results.

3. It would also be interesting to add a second satellite data set for an additional point of comparison. One option would be MODIS Terra, as the earlier overpass time might mean different sampling due to cloud cover changes. Alternatively the authors might consider a different sensor or algorithm. There are many available during the 2014-2017 study

period, but for the longer-term PM trend analysis the options are fewer. MISR has a narrower swath so there will be many fewer matchups with the Sun photometers, but its retrieval of aerosol properties has some more flexibility so it might perform better. OMI also has a nice smoke/dust aerosol type identification which might be useful here since part of the analysis involves relating AOD to PM based on aerosol type. So that could be a good addition. Another alternative is using a reanalysis product (e.g. MERRA2) which might also have surface concentration estimates. I am not saying this should be a requirement for publication, just something for the authors to consider.

Thank you for your suggestion. We agree that a mixed of different satellite products along with reanalysis like MERRA2 could provide interesting information on AOD-PM relationship in our area of interest. However there are not so much options for long-term analysis as you have noticed. We have selected the MODIS record as it is comprehensive and well referenced. CALIPSO would be also an interesting alternative that requires further investigations. We have followed Wei et al. (2019) recommandations of using AQUA products.

Please refer to L127 of the revised document.

4. Line 6: “Angstrom” should be written as “Ångström” here and throughout the paper. The paper is not always consistent. Line 110 and Table 1 have the ö but not the Å,for example, and Figure 3 has neither. This is not needed on line 119 though because there the authors are referring directly to the variable name.

We now use the correct writing of Ångström in the text, bibliography, and in the figure captions. AE is used for figure labels.

5. Line 16: I think “S=” can be removed here.

Done.

6. Figure 1: again, not essential, but rather than have a greyscale map the authors might consider using e.g. a population map upon which to show the site locations.

Figure 1 has been updated with the geographical location and population of cities having more than 1,000 inhab.

7. Line 124: I would add parentheses around the EE expression as the interpretation of the +/- is ambiguous as written. I believe the correct representation is +/-(0.05+0.15xAOD) and not (+/- 0.05)+0.15xAOD.

Done.

8: Line 137: this should be IQR not IRQ.

Done.

9: Lines 144-145: the sentence says that the highest AOD was 3.8, but then says it was 3.7. This should be checked and corrected.

Correct value is 3.76. The text has been modified.

See revised text L170

10. Lines 152-153: the offset in AE between two measurement types could well be related to calibration; the authors may wish to mention the study by Wagner and Silva (2008) on this topic: https://acp.copernicus.org/articles/8/481/2008/

Thank you for the reference. Very interesting. Rather than related to calibration, the difference might be due to the difference in the AOD statistical distribution. Indeed the observations were not acquired during the same period and the AOD distributions are not the same. We have added this possible explanation in the text with the corresponding reference. Moreover, we have better discussed the comparison on coincident observations between AERONET and CALITOO in Lamto. Despite a short sample period, the agreement is excellent (see added figure). Note there is an error in the sampling period in Table 1 but Figure 2 and 3 shows the overlap period.

See revised text L181.

11. Line 156: AERONET collects data from dawn to dusk, while the hand-held instruments say they were used twice a day. Are these daily averages from all points or from the same times as the hand-held instruments?

AERONET observations are daily average. Now mentionned in the text L84.

12 Figure 5: my assumption is that the AE shown from MODIS Deep Blue and ocean algorithm here are for all points of the domain, and not only for the grid cells wherethere are sun photometer data. Is that correct? If so, some differences might also be expected due to real spatial variability in the AE at locations without sun photometer data. This should be mentioned.

The comparison of MODIS AE values have been revised to better show the difference at both the coastal and inland sites. We used only the measurements corresponding to the grid cell where the sun photometers are located.

Please refer to revised Figure 6 and text L215.

13. Table 3: I am not sure I fully understand this as it took a few readings. It seems that the numbers not in brackets are the percentage of days from each category, using only the AE to split them. Then, the numbers in columns are the same, except considering only those days where the AOD was above the third quartile for that location, i.e.days where AOD was particularly high. So the table is contrasting the optical "type" ofaerosols between sites, and also between the data set as a whole and those particularhigh-AOD days of concern for air quality purposes. Is that correct? I wonder if thenumbers in parentheses should be given their own 3 columns with own subheader inthe table. This is because it is the tendency as a reader to look at the number and the one next to it, and in this case they're not directly related, as the relevant comparison is between columns.

We agree that the information provided in the Table 3 were not easy to read. We have simplified this table and now provide only the percentage of daily observations for each category and each site. Please note that the 'Dust' category has been updated by using a threshold on AOD.

See new table 3.

14. Figure 6, and associated discussion: this should be fleshed out more. First, is this figure showing mean and standard deviation of the ratio? This should be stated. However, another concern is whether mean and standard deviation are the right metrics to show; I suggest median and IQR could again be more appropriate. The authors do not show the raw data so there's no way to know whether there are e.g. outliers which are throwing off the mean ratios here. I suggest adding plots of the data (i.e. weekly AOD and PM for different seasons and type classifications). In theory, given constant meteorology and composition, it is true that the ratio between AOD and PM should be a constant (the mass extinction efficiency and a factor based on height). However in practice factors such as changing composition, variations in aerosol vertical structure (e.g. whether or not the aerosols are mostly in the boundary layer), and moisture (as AOD is dependent on ambient RH while PM is not) will be important. See for example Sayer et al (2016) for the dependence of the ratio on some of these factors for smoke in Thailand: https://doi.org/10.4209/aaqr.2015.08.0500 The situation is a bit different here because the present study is weekly filter measurements (not continuous) but the general point remains. The authors need to show the data to provide justification that adopting a direct ratio approach, and reporting the mean and standard deviation (or changing to median and IQR), is an appropriate empirical parametrization here. It is not possible to judge from the material presented in the paper. As this figure is key to most of the rest of the paper, changes to this part of the analysis could affect the later results and discussion as well.

We agree with your remark that the actual PM2.5/AOD ratio is subject to a large variability due to the modification of meteorological conditions or changes in aerosol chemical composition. We have added your proposed reference (Sayer et al., 2016) in the introduction section where we discuss previous studies. So far there is no scientific agreement on a universal method to convert aerosol optical parameters into surface concentrations however it is generally admitted that AOD and PM2.5 are correlated. Conversely, for steady surface concentrations and columnar AODs, a poor correlation between both quantities doesn't mean that a PM2.5/AOD ratio is not appropriate. Errors on the PM2.5/AOD ratio can then be estimated from the standard deviations of both quantities. The time series of coincident weekly observations already reported in Figure 9 (now for each site) provide posterior justification that the PM2.5 is proportional to AOD by period of time and that the aerosol type may also has some influence on the proportionality. So the coefficient are estimated by season and by aerosol type. The standard error reported on Figure 6 reflects that any change in AOD is not associated to an exact proportional change in PM2.5 over time. However and as you mentionned, there is an additionnal difficulty due to the different sampling period (weekly PM2.5 versus daily AOD). We handle this difficulty by estimating the relative influence of each aerosol type during a week.

Please see additional information in the method section L298 and Figure 9.

15. Figure 7 caption: "weakly" should be "weekly".

Done.

16. Figure 9 and associated discussion: given the high seasonality in AOD (and PM),as well as the potential for uneven satellite sampling through the year (due to e.g. cloud cover variations), I am not convinced that it makes sense to examine only annual trends in PM. I suggest adding seasonal analyses as well. This will provide more insight as to any changes. The authors might also add a plot showing e.g. the number of days with data in the average Jan, Feb etc from MODIS, so we can see whether sampling variations exist. A second point about trends is that rather than talk only about p values, it would be useful to mention the uncertainty estimate on the trend as well. Statistical significance and importance are not the same thing. For example, a statistically significant result might have a small magnitude which is not important for practical purposes. And a result that is not significant might be because either the estimated magnitude is small and the uncertainty is also fairly small (i.e. we can be confident there is not a large trend), or because we have a large uncertainty so can't tell whether an effect is large or small (i.e. we can't be confident about the magnitude and/or sign of any trend). Reporting best estimate of trend and p value without the uncertainty estimate means we cannot directly tell which of these is the case here.

Correct. The trend analysis has been totaly revised. Following your recommandation, we now provide the monthly mean concentrations and the seasonaly adjusted trend. We have reduced the potential impact of uneven sampling by selecting a broader area along the shore line. This larger area has at least one observation per day. Yes we totally agree with your remark however the uncertainty on the Thiel-Sen's slope were already reported in original text L273. The monotonic increasing trend is significant however the exact magnitude remains highly uncertain.

17. Data availability: the authors give Giovanni as the MODIS data source. This is mostly a visualization portal and may not have the latest/official data versions. I just checked there and they list collection 6.1 which makes it more surprising that the authors used the older collection 6. Note the main NASA search tool is `https://earthdata.nasa.gov/` and the actual MODIS data portal for this product is LAADS, `https://ladsweb.modaps.eosdis.nasa.gov/`.

Correct. Actually we did a first attempt of using MODIS L3 products from the Giovanni portal that also provides an extraction tool. Then we used the a mirror archive but the archive wasn't uptodate. Now we use the data from the LAADS portal `https://ladsweb.modaps.eosdis.nasa.gov/`.

Thank you for your warning on the MODIS Version. We have changed the data availability section.

References

Adon, M., Galy-Lacaux, C., Yoboué, V., Delon, C., Lacaux, J. P., Castera, P., Gardrat, E., Pienaar, J., Al Ourabi, H., Laouali, D., Diop, B., Sigha-Nkamdjou, L., Akpo, A., Tathy, J. P., Lavenu, F., and Mougin, E.: Long Term Measurements of Sulfur Dioxide, Nitrogen Dioxide, Ammonia, Nitric Acid and Ozone in Africa Using Passive Samplers, Atmos. Chem. Phys., 10, 7467–7487, doi:10.5194/acp-10-7467-2010, 2010.

Kniffka, A., Knippertz, P., and Fink, A. H.: The Role of Low-Level Clouds in the West African Monsoon System, Atmospheric Chemistry and Physics, 19, 1623–1647, doi:10.5194/acp-19-1623-2019, 2019.

Sayer, A. M., Hsu, N. C., Hsiao, T.-C., Pantina, P., Kuo, F., Ou-Yang, C.-F., Tsay, S.-C., Holben, B. N., Janjai, S., Chantara, S., Wang, S.-H., Loftus, A. M., and Lin, N.-H.: In-Situ and Remotely-Sensed Observations of Biomass Burning Aerosols at Doi Ang Khang, Thailand during 7-SEAS/BASELInE 2015, Aerosol and Air Quality Research, 16, 2786–2801, doi:10.4209/aaqr.2015.08.0500, 2016.

Wei, J., Peng, Y., Guo, J., and Sun, L.: Performance of MODIS Collection 6.1 Level 3 Aerosol Products in Spatial-Temporal Variations over Land, Atmospheric Environment, 206, 30–44, doi:10.1016/j.atmosenv.2019.03.001, 2019.