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Interactive comment

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 #2

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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 relationship between 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 et al., 2018); this work is a natural extension of that (using the data collected and more in concert with MODIS) and contains sufficient novel material. The quality of language

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Discussion paper

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:

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 sun photometer 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.

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.

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.

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.

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

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.

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.

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

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.

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/

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?

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 where there 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.

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" of aerosols between sites, and also between the data set as a whole and those particular high-AOD days of concern for air quality purposes. Is that correct? I wonder if the numbers in parentheses should be given their own 3 columns with own subheader in the 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.

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.

15. Figure 7 caption: “weakly” should be “weekly”.

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.

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/ .

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