Thank you for your thoughtful comments. Including your suggested revisions has improved the quality of the manuscript. Our responses are indicated below in blue text.

Reviewer 1

Interactive comment on

"Understanding the seasonality and climatology of aerosols in Africa through evaluation of CCAM aerosol simulations against AERONET measurements"

Anonymous Referee #1

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This paper evaluates the performance of the CCAM model at simulating aerosols over Africa, by comparison to AERONET data. The paper's title and some of the text set it up to be primarily a description of the aerosol cycle in Africa. However most of the real content is in the evaluation against AERONET, where we see that there are some shortcomings for CCAM's representation of dust. As a result, I don't think it makes sense to present this as a paper about the seasonality of aerosols in Africa. It's really a model evaluation exercise, which establishes some problems with dust and the timing of biomass burning, but better performance for other aerosols. So perhaps there will be a follow up in a few years when these issues have been improved and the model is more in the application phase than the evaluation phase. As a result this paper might fit better thematically in GMD than in ACP, but it is within scope for ACP as well.

The paper is interesting and scientifically does not have major problems. However, the organization should be improved. There are parts where it is a bit lengthy and unclear, and contains statements which are either slightly incorrect or information that is not necessary (it reads as very descriptive and not very analytical, sometimes, if that makes sense). This makes it difficult to read and pick out the main points. The whole paper could be streamlined to improve readability and clarity. I have included some suggestions for where to do this in my comments below. These rewrites should make it easier to judge the paper and pull out the main conclusions, which I have a bit of a hard time doing now. As a result I recommend major revisions since some of the suggested rewrites will alter the structure of the paper somewhat and some things may become clearer. I would like to review the revised version.

Title: See above comments. I recommend changing the title to make clear that the focus is the evaluation of the model, rather than "Understanding the seasonality and climatology of aerosols in Africa".

We revise the title to: "Evaluation of climate model aerosol seasonal and spatial variability over Africa using AERONET".

Abstract: This should ideally be one paragraph which concisely summarises the key points of the paper. This abstract is three long paragraphs covering about a page. I suggest that this can be

condensed somewhat. For example, the entire middle paragraph is more or less well-known results (e.g. where and when dust comes from) and can be deleted. I would then merge the remaining two paragraphs, which contain more overview and then the main results of the paper.

We condense the abstract following these recommendations.

Section 2.1: In my Quick Report comments I had suggested adding more AERONET sites; the authors added most of these (thank you for this effort), but not one of the key Saharan dust outflow sites which I had suggested (Capo Verde). I see that this is just outside of model domain listed here in the paper, so perhaps that is why it is not included. But presumably the model was run globally so perhaps the analysis domain could be extended another few degrees to include this site? It is one of the key long-term sites which has been used by many researchers to examine Saharan dust and evaluate models (among other things) so would be useful to have the comparison there as a point of reference, if possible. While not essential, I mention this specific site again for this reason. It could help confirm the hypothesis about dust lifetime in CCAM, since this site is a way away from the sources.1

The reviewer is correct that the African domain was extracted from global runs of CCAM. In those original runs, we would have been able to extract Cape Verde. We did in fact try to address this comment for this round of reviews; unfortunately, those original global runs were mistakenly deleted and therefore no longer available. We only have stored the African domain as in the current manuscript, and only for selected number of variables.

Izana is not a useful site for model evaluation and can be removed. It is on the top of a mountain and not representative of the surrounding area. See e.g. https://aeronet.gsfc.nasa.gov/new_web/photo_db/Izana.html .

We agree with the reviewer that the height of the observation site is an important consideration, which we include in Table 1 and discussed in the context of Izaña on page 8, lines 19-23 (Section 3.1). La Laguna, Santa Cruz Tenerife, and Izaña are in the same model grid box. In selecting sites for the observation-model comparisons where there were multiple sites in such close proximity, we originally selected sites that had the largest dataset available for the comparison in order to ensure the comparison was robust (which in this case was Izaña). In light of your comments and the differences in α_{ext} and the magnitude of AOD at Izaña, we instead evaluate the model with AERONET data from Santa Cruz, which has the second largest number of months with valid data available for comparison on the island. We revise Table 4, Figure 6, Figure 7, and the discussion and Section 4.2.3 accordingly.

Page 5 line 26: Strictly AERONET does not measure AOD. It measures the direct so- lar irradiance, and then does a (very accurate) retrieval to determine AOD. Even this direct-Sun AOD product is a retrieval, not a direct measurement. Also, the wavelength range given here is wrong (the range depends on the specific instrument). I suggest rewording to say that AERONET provides spectral AOD at multiple wavelengths, depending on instrument, from the UV to the swIR. A key point being changing the word “measured” here and in line 28 (plus other places I might have missed) to a more correct term such as “provides”.

We revise the sentence in page 5 lines 25-26: “The global network of AERONET stations measure aerosol optical properties at multiple wavelengths ranging from the UV to shortwave infrared using a ground-based Cimel sun-photometer (Holben et al., 1998; Dubovik et al., 2002).”

we change "measured" throughout the manuscript.

Equation 2: The definition of AOD seems superfluous here so can probably be deleted as assumed background knowledge.

We remove the definition and equation 2.

Page 6 lines 30-31: “was considered, and were aligned as possible” does not make sense. I suggest rewording this paragraph (perhaps it is just this first sentence which is causing confusion). If I understand correctly then the model provides 6-hourly output and a daily average was constructed from the output from ‘daytime’ hours over this domain. The key point being here is that sampling is daytime only to match AERONET, but the specific AERONET days are not being matched directly. Is that correct?

This sentence has been removed and the entire paragraph clarified following your and the other reviewers’ suggestions, as follows (page 6, lines 23-28): " Daily average AOD from AERONET is calculated for a minimum of 3 time points from sun photometer measurements, which can only be made during daytime, while modeled AOD is reported at 6-hourly resolution. Therefore, only CCAM AOD between 06:00 and 18:00 UTC was averaged for monthly and multi-year means (similar to other AERONET-model comparison studies; (e.g., Tegen et al., 2013). Model monthly means were, however, insensitive to the choice of daylight cut-off (see Fig. 2), which gives confidence that the instantaneous 6-hourly values from CCAM can represent the range of daytime hours sampled by AERONET.”

Page 7, lines 12: Likewise, I think the definition of Pearson correlation coefficient is not necessary. For the specific analyses performed in the paper (i.e. assessing to what extent the seasonality of AERONET is reproduced by CCAM), the coefficient of determination (r2) may be useful than r anyway.

We have removed the definition.

Page 7, lines 22-23: since AOD distributions are not Gaussian, might it be better to show interquartile range or similar rather than standard deviation?

In order to address this comment we have split up the previous Table 1 into two tables. The new Table 1 has the average AOD and Ångström exponents with the standard deviation. We have also added the median, 25th and 75th percentile values to Table 1. We report averages and median values in the text as well. The timing of the maximum and minimum values has been moved to Table 2.

Page 7, lines 24-25: This is another example of a slightly misleading/inaccurate statement. Ångström exponent (AE) is related to the optical dominance of fine vs. coarse aerosols in the column. This is subtly different from what is written in the paper which says that it gives information on size. For example, an AE around 1 could be either an indicator of monomodal mid-sized aerosols, or an

indicator of a column containing similar amounts (in optical terms) of fine and coarse aerosols. These are quite different things. I suggest rewording.

Thank you for your help in clarifying our explanation of the use of the Angstrom exponent as a proxy related to aerosol size. This sentence has been updated following this suggestion and that of the other reviewer (now page 7, lines 16-18): "The Ångström exponent is an empirical proxy related to the relative contribution to optical thickness from coarse vs. fine aerosols, with values varying between approximately 0 for pure coarse dust particles to 2 for predominantly fine particles (Leon et al., 2009; Hamonou et al., 1999)."

Page 8, lines 2-3: "regional trends". It would be better to say "regional patterns" or something, since the term "trend" is most commonly used to refer to analyses of time series for changes.

We change "trends" to "patterns" as you suggest.

Section 3: I don't think that the general description of aerosol seasonality in the model is that necessary, since the main aerosol sources in Africa and their timings are reasonably well-known, and the model has some biases anyway. (Really, the evaluation should have come before this descriptive section anyway, since you have to establish the validity of the model before you can use it to answer science questions.) It would be better in my view to present and discuss model and AERONET seasonality for each region simultaneously. Then we can get to the interesting stuff of whether the model is reproducing the patterns seen in AERONET. Essentially, merge in the current Section 4.2 with the existing Section 3 and rewrite.

We agree with the reviewer that model evaluation should be performed prior to using the model to inform processes. Section 3 is not presenting model results, but rather the observational data from AERONET. The model evaluation is performed in Section 4, after the observational data are discussed. We felt it was beneficial to discuss the entire suite of available AERONET AOD at sites influenced by African dust and biomass burning independent of the model first. We feel that clarifications added throughout the paper to address the confusion resulting from misinterpreting Section 3 and Figures 3 and 4 as model results solves this issue.

Section 3.1: as an example of some stylistic issues throughout the paper (applicable to much of the discussion, not just here): 1. The word "values" appears a lot here and can probably be deleted. There isn't a real difference between saying "the AOD values" or just saying "the AOD", for example, and the latter is more concise and readable.

Values was removed throughout the paper wherever appropriate.

2. Similarly, the subscripts for AOD and AE are the same all the time so can be omitted for brevity and clarity. (For example, just say once at the start of the data set description the wavelength or wavelength range being considered and don't repeat it every time).

We retain the subscripts in the figures but remove from all text following the initial description in the methods section.

3. The text in this section also doesn't specify whether AERONET or model data are being referred to. The related Figure 3 caption also doesn't say. This should be listed explicitly. I infer it is the model.

We apologize that the title of this section and caption of Figure 3 were not clear. Figures 3 and 4 and Section 3 are AERONET data. We rephrase the caption of Figure 3a and 3b: "Multi-year mean seasonal cycle of observed AERONET AOD_{550nm} at long-term sites", and add "observed…from AERONET" to the captions of Figure 4a and 4b. We further clarify the title of Section 3 (added text underlined): "Climatology of AERONET AOD and α_{ext} observations over Africa…" and add "AERONET AOD and α_{ext} observations" to the title of subsections 3.1, 3.2, and 3.3. We also explicitly state more frequently throughout Sections 3 and 4 when we are referring to AERONET AOD observations vs. model results.

Page 14, line 5: As another style example, "The Pearson's correlation coefficient" could have "The" and probably "Pearson's" deleted as well.

We implement this suggestion (now page 14, line 6).

Page 14, line 16: is the beta here intentional? If so, what does it mean?

This was a typo and has been removed – thank you for catching it.

Figure 3: In general I don't see the point of these figures. Seeing one line per site here is not very informative. If the purpose of the paper is to compare with AERONET, the same basic information for AOD is repeated in Figure 6. Or am I misunderstanding something? It would be better to show, for each site, the model and AERONET together so a direct comparison can be made. So something like Figure 6, for both AOD and AE.

As stated earlier, it appears there was a misunderstanding of Section 3 and Figures 3 and 4, which we clarified in Section 3, the Figure captions, as well as Section 4 following your helpful comments. Figures 3 and 4 present the observations only. We felt this complete record of observed AOD at sites influenced by African dust and biomass burning could stand on its own outside of the model evaluation. Therefore, we include more sites in these figures even though they have limited data coverage - e.g., not a full seasonal cycle, or only a single year of observations - which make them not very useful for evaluating the climate model but still informative to get the broader picture of observed AOD across the African continent and outflow regions. We explain in section 2.2, page 6 lines 8-11, how we selected observational sites with which to evaluate the model, and discuss the temporal resolution limitations of modeled emissions of aerosols and their precursors from CMIP5 in section 2.1, page 5 lines 9 to 16.

We clarify Figure 6 within Section 4.2 (now page 11, line 15): "Figure 6 shows the same multi-year mean seasonal cycle for observed AERONET AOD as in Fig. 3 (here in red triangles)…". We think this and the clarifications made within Section 3 and to the captions of Figures 3 and 4 described earlier should address your comment.

Modeled AE is not possible to obtain. Modeled AOD is only calculated at 550nm.

Figure 7: It would be better to overplot the AERONET AOD on top of the model component lines, rather than shifting it off to the right, to allow a more clear visual comparison of aerosol amount and seasonality.

We edit Figure 7 following your suggestions.

Table 1: It would be useful to perform the AERONET/model comparison at ALL the sites shown, not just a subset. Otherwise what is the point of including them in the paper if the AERONET data are not used?

See response to previous comment above regarding Figure 3, and earlier comment regarding Section 3.

Table 3: I am not sure it is useful to report significance of correlation coefficients here. I don't think that it adds anything to the analysis or discussion, and due to strong autocorrelation of the data (which I don't think is accounted for) it is possible that the significance estimates are incorrect anyway.

We have performed an autocorrelation test on the model output; the observational dataset had many missing monthly averaged values, and thus the analysis was performed on the model output that has a complete dataset. In order to ensure the annual cycle does not have a role in the autocorrelation analysis, we performed the analysis per month per site (e.g., assessing autocorrelation in all January means for Skukuza site). As this analysis was per site and per month, the n=360. At a time lag = -1, there were only 6 instances out of these 360 (1.6%) where the autocorrelation was statistically significant at a 95% confidence interval. This is a very small fraction of the data analysed, and thus the autocorrelation in the model output can be considered not statistically significant. As no autocorrelation was found in the model output, it is assumed that there is not autocorrelation in the observed data as well.

General: as noted in my Quick Report, I suggest the authors also perform some analysis using daily (rather than monthly) data. This can be simple visual scatter plots for each site, or something similar to Table 3. This will help to tell to what extent biases in the monthly data are due to aerosol events that are missed in the model, and to what extent they are systematic biases in component loadings or optical properties. Going to daily data here also helps to avoid some of the sampling differences.

We add a comparison of daily data (now Figure 8) and corresponding discussion (new Section 4.2.4). We introduce the daily comparison at the end of Section 2.3 (Page 6, lines 5-10): "We also compare modeled daily average AOD_{550nm}, using the same daylight hours previously described, to observed AERONET daily average AOD_{550nm} for the specific days with available data at each site. As described in Section 2.1, outside of the dust parameterization, the experimental setup of the model following CMIP5 does not take daily variations in emissions into account, and thus the daily variation in modeled AOD from all other aerosol types will be due to daily variations in transport and removal only. Even with these limitations, the daily comparison is useful for further investigating model biases."