

Reply to review comments #2

This is my second review of Peng Xian et al.: Arctic spring and summertime aerosol optical depth baseline from long-term observations and model reanalyses.

The authors have followed my suggestion to split their work in two papers, which somewhat helps with taking in all the provided information. However, they have dismissed most of my other suggestions without providing convincing rebuttals. Therefore, I am sorry for having to repeat much of my earlier criticism. I have worked through both versions and it took me considerable time to do so. My suggestions are therefore focussed on distilling the provided information down to what is essential and to make it easier for potential readers to take it in. I start with general comments that apply to both papers before providing specific comments for the individual parts.

Reply: We really appreciate the reviewer's detailed and constructive comments.

General comments

The authors should make a serious effort in improving the quality of their writing. The current version of the papers does not make for enjoyable reading.

- There are plenty of statements that span over 4, 5 or even 6 lines and are very hard to follow. Please carefully go through the manuscripts and shorten or split those sentences.

Reply: We've made significant efforts splitting long sentences or shortening them.

- The authors' use of parentheses to specify a statement made just before is particularly annoying. Why not go with the text in the parentheses in the first place? Or make up your mind which one is the preferred phrasing. There are so many of these statements that I won't list them. Please revise.

Reply: We've also removed dozens of parentheses by incorporating the information explicitly into sentences or removing the unnecessary/repetitive information.

- The authors should properly define all the used terms in the beginning or when first used and stick to those terms rather than providing their definition whenever they are used. This occurs particularly often for the term MAM, JJA, and high or low Arctic. Please make sure that all acronyms are introduced only once in the text. No need to re-do so in the summary. The readers should also be able to remember the extent of the covered time period without repeating it at almost every instance.

Reply: Thanks for pointing this out. The first author defined the acronyms again in the summary section, besides where they were first used, as a convention (due to requirements of some other journals and that often times readers will just read the conclusions). We read the ACP submission guide and don't see such a requirement, so we have followed the reviewer's suggestion and dropped the definitions of acronyms in the summary section and a few places in the middle of the text. Following the reviewer's suggestion, we've also removed over a dozen of "2003-2019", which is the extent of the covered time period.

- The authors give values in the text that are often put behind a ~, i.e. maximum AOD of ~1.2. Do you know the specific value or do you provide estimates? Please be specific.

Reply: We have now provided specific numbers whenever applicable.

- The text is very repetitive. For instance, the point that biomass-burning smoke is the dominating contributor to Arctic AOD in summer is repeated at multiple occasions. Please identify the instances where repetitive statements fit best and move them there. You can expect the reader to remember. Please thoroughly go through both manuscripts to get rid of redundant text, figures and tables. Some examples are given in the specific comments below.

Reply: We have removed repetitive sentences as suggested (see our replies to specific comments) and in other identified places in the text.

- The abstracts of both papers – but particularly of the second one - are much too long. Please try to reduce to the essential findings.

Reply: We've shortened the discussions as suggested.

- Please make sure that there is a proper connection between the two parts of this study. While part 1 is referred to in part 2 there is just one mention of part 2 in the introduction to part 1. The authors are missing the opportunity to properly connect their work.

Reply: Thanks. We have now added a sentence in Sect. 5.2.1 "Interannual variability of AOD" to make closer connection of Part 1 to Part 2. The sentence reads "The statistics of extreme AOD events, and implications for the impact of regional biomass burning processes are provided in Part 2".

Paper 1 on climatology and trend

Major comments:

- Redundant text: lines 160-164 (this is clear), 195/196 (the general time period has already been defined), 215-217 (repeats what has been just stated), 220-224 (reference to Toth et al. 2018 is sufficient, also why consider them in the analysis when you just made the point that artificial AODs of zero are unphysical?), 246-253 (no need to discuss a parameter that has not been used in your work), 280/281 (you can assume that readers can draw this conclusion themselves), 431-433, 917-936 (redundant or should be part of the data or methodology sections)

Reply: We have removed the lines listed, except 220-224, 246-253, line 280/281 and line 917-936. Line 220-224 is a discussion added after the first round of reviews in response to the other reviewer's comment on the impact on result from the QA process of the CALIOP data. We prefer keeping line 246-253, as our FMF (fine mode fraction) result in Table 1 is different from SMF (sub-micron fraction) results shown in other Arctic AOD studies as cross references, and here we discuss the fundamental cause of the difference in methodology. The Table 1 result in terms of difference between FMF and SMF is discussed in the Sect. 4 line 515-526. For line 280/281, we just want to be explicit without relying on reader's own assumption that MAN data was obtained over water on ships. Line 917-936 is the first paragraph of the Discussion Sect. Here we stress the importance of quality control process of remote sensing data, and provide a parallel comparison with other Arctic AOD studies using off-the-shelf satellite retrieval data. We think the discussion is valuable for future Arctic studies tending to use satellite retrieval AOD, and thus keeping it. We keep this paragraph in the Discussion Section as it is a discussion of the result, while QA processes are already provided in the data and method section.

- I still don't see the need to include Section 2.10 and Figure 12 in the paper. None of the other biomass-burning emission inventories is referred to in that much detail. Also, FLAMBE is used as input to NAAPS-RA and it is not clear why showing FLAMBE maps provides added information to showing findings for BB aerosols from NAAPS-RA.

Reply: We still think FLAMBE biomass burning emission climatology and the trend is an important support for the BB AOD climatology and trend in the Arctic, and it provides BB source information for Part 2, as opposed to the AOD coverage-which is typically not a linear relationship (figure 12 is explicitly referred in Part 2). The added information of providing FLAMBE maps in addition to BB smoke AOD maps is that smoke AOD trend is due to the first order of significance to emission trend in the lower latitudes. And other factors, e.g. mid-latitude to Arctic transport, if they play a role in smoke AOD trend in the Arctic, would be a second order significance. We have added the following discussion in the new subsection 5.3.4 "Possible causes of BB smoke AOD trends". All other text of this subsection is moved from the previous section 5.3.2 "AOD summertime trends".

"Compared with the BB emission trend, trend in the atmospheric processes, e.g., transport and removals, probably plays a secondary role in the Arctic smoke AOD trend. This is illustrated by the similarity in spatial patterns of smoke AOD and BB emission trends, and the coincidence of peak years for emissions and the high Arctic area-mean smoke AODs. For example, 2012 and 2019 are associated with JJA peaks in emission and high Arctic smoke AOD, while 2003 and 2008 correspond to MAM peaks in both (Figs. 12 and 13)."

- I suggest to stick with fine mode and coarse mode rather than introducing FM and CM. This would increase readability a lot. Right now, the authors switch between using fine mode, FM, an even FM mode...

Reply: Thanks for the suggestion. However, "FM" appeared 60/50 times and "CM" 54/46 times in the text of Part 1/Part 2, which, we think, makes a good reason to abbreviate them. Also these abbreviations in tables help the tables to fit in space. And actually we tried to replace "FM" with "fine mode" and "CM" with "coarse mode" and that increased the length of Part 1 by 1 page (including shifts of figures and tables). So we keep using the FM and CM abbreviations, but we've made sure that these abbreviations are defined when they first appear in the text, and there is no switch back and forth between the abbreviations and the full expressions after that.

- Please make sure that the description of a figure or table is confined to the figure or table caption. The main text should not be used, e.g., to describe what a line of a certain colour represents.

Reply: Thank you. We have now removed such descriptions from the main text, including these for Fig. 2, Fig. 6.

- I expect that most readers are interested in the general findings of the authors' work rather than the peculiarity of individual reanalysis models. I therefore still think that the paper would be much improved if the authors were to focus on the multi reanalysis consensus (MRC) in the figures of the main text. Presenting just the plots for the MRC in Figures 2-7, 10, 11, and 13 doesn't prevent to authors from pointing towards differences in the considered models. If the plots (2-7, 10, 11, 13) and tables (2, 3, 4) for the three individual reanalysis are moved to the supplement, they would still be accessible to readers that are particularly interested in these differences.

Reply: We prefer keeping the results from individual reanalysis along with the MRC, because the diversity and similarity of these reanalyses is indeed part of the main result of the paper. By showing the individual reanalyses side by side with the MRC, we also intend to avoid the consensus being dominated by any specific reanalysis product. We think this makes our result more convincing while providing information for readers interested in the difference of the reanalyses. In addition, although indirectly, this study also serves as an inter-comparison of model performance for the listed models over the Arctic

region. We believe those results should also be of interest to readers who are users of any of the models.

- Please make sure that you properly and specifically refer to figures you are discussing rather than just providing a figure number or a list of figures at the beginning of a paragraph.

Reply: We tried to avoid leading a paragraph with a figure number following the suggestion. Thanks for your suggestion.

- Section 5.2.1 doesn't really provide an objective assessment of interannual variability and is largely based on referring to individual events that should be discussed in the introduction to Part 2. I suggest to omit this section.

Reply: Section 5.2.1 provides general features of AOD interannual variability and explains some of the large interannual variability signals in monthly AOD time series from AERONET and MRC shown in Figure 2 by providing corresponding known biomass burning cases. Some of the cases were recorded during field campaigns and were well studied. We think these are useful information for understanding the cause of interannual variabilities. To make the purpose clear and connect to Part 2, we have now included the following two sentences below. Also some individual events are already mentioned as examples of extreme AOD events in the introduction of Part 2.

“Some of the BB smoke events cause short-term record-high AOD, and some lasted weeks to months, resulting in high monthly mean AOD. The statistics of extreme AOD events, and implications for the impact of regional biomass burning processes are provided in Part 2.”

Beside the above changes, we've also removed a redundant paragraph in this subsection.

- It is not clear to me what is shown in Figure 8 or how the plot has been compiled. Please provide a better description.

Reply: Figure 8 shows the percentage of interannual total AOD variability explained by speciated AODs. We have now added a paragraph to explain how this (and other statistical variables) is calculated in the “Method” section. It reads “For verification purpose, bias, root-mean-square deviation (RMSE) and coefficient of determination (denoted r^2) of reanalysis AODs compared to AERONET/MAN AODs are calculated. r^2 equals the square of the Pearson correlation coefficient between the observed and the modeled AODs. When estimating contributions of individual species to total AOD interannual variability, r^2 is calculated as the square of the Pearson correlation coefficient between the seasonally-binned modeled speciated AOD and total AOD. In that form, r^2 provides the percentage of “explained variance” of total AOD by a speciated AOD. The statistical definition and interpretation of r^2 can be found https://en.wikipedia.org/wiki/Coefficient_of_determination. “

- There is no discussion of Figure 9. What about moving this figure to an earlier position after Figure 3 so that the presentation of the satellite data is all finished before moving on to the models?

Reply: We discussed Fig. 9 in Sect. 5.3 and mentioned “Fig. 9” twice there and once in Sect. 6. We have now referred Fig. 9 seven times (not adding text, but explicitly referring it in a few more places where applicable). We keep the trend analysis with both the satellite data and the reanalyses under the same section (i.e. Sect. 5.3), so that the trends derived from the two different types of datasets can be compared conveniently.

Minor comments.

- Please update the reference IPCC (2013) to IPCC (2021)

Reply: updated.

- Ice nuclei are now generally referred to as ice nucleating particles (INP)

Reply: updated.

- Line 335: on the other hand requires an earlier on the one hand

Reply: “On the other hand” is now changed to “Furthermore”.

- Lines 392-400 should be moved to the introduction

Reply: This part states the reason why biomass burning smoke is treated as a singularly important species in this study, so we think it belongs to the “Methods” section.

- Line 414: What is it, hourly or daily data?

Reply: Changed to “either hourly or daily”

- Lines 476-481: this should be moved to the methods section

Reply: This part is to explain the difference between the mean and the median as shown in Table 1. Thus we kept the discussion as a part of the result section.

- Caption Figure 1: Add that the size of the circles refers to the magnitude in AOD.

Reply: Thanks for the suggestion. Added.

- Caption Figure 3: omit second sentence. This has already been stated in the Data section.

Reply: Thanks. The 2nd sentence is now removed in the figure caption.

- Lines 588-605: Remove reference to Figure 3 and move a generalised version of this text to Section 2

Reply: This paragraph describes partial results of Figure 3 and explains the coverage patterns of the sensors for different seasons. The coverage pattern is better explained in reference to Fig. 3. Therefore we keep this paragraph here.

- Line 626: CALIOP has a footprint of 70 m.

Reply: Thanks for spotting this. We have updated the text to “The swath for MODIS and MISR is on the order of a few hundred to a few thousand kilometers, while the “beam diameter” for CALIPSO is on the order of 70m (Winker et al., 2009; Colarco et al., 2014).” The reference paper, Winker et al., 2009 is added.

- Line 630: no need to use an acronym for data assimilation. It’s used only once and I forgot what DA was supposed to stand for by the time it was used...

Reply: All “DA” in the text are now expressed explicitly as “data assimilation”.

- Figure 4: lines 645-650 should be moved to suitable places in the text. They don’t belong into a figure caption.

Reply: We have moved the text to “Methods” section.

- Figure 5: move the second sentence of the caption into the main text.

Reply: We have removed the 2nd sentence, as the information is already included in the “Methods” section.

- It seems that Figure 7 is discussed before Figure 6. Also the discussion of Figure 7 doesn’t seem to be quite objective: no change can be extracted if the error bars were to be considered!

Reply: Figure 7 is discussed after Figure 6. Figure 6 first appears in “Speciated AODs have more variability than total AOD among the three reanalyses, and a little more so for MAM than for JJA (Fig. 4, 5, 6).”, and two paragraphs earlier than Fig. 7 in sect. 5.1.2. For Fig. 7, error bars represent monthly AOD variability. It is clear that interannual AOD variability for July and August is much larger than other months.

- Lines 868/869: not clear which figure the authors are referring to

Reply: The text was “For the high Arctic, AOD trends are hardly seen with the same color scale as those for the lower latitudes because of lower AOD. Thus, they are shown separately in Fig. 13....”. We have changed “are” to “will” after “AOD trends”.

- Line 939: what climate models? Please specify.

Reply: We specify the climate models as AEROCOM and CMIP5 models and give details right after the sentence.

Part 2 on extreme events

General comments

- Please shorten the Abstract to present just the essential findings.

Reply: The abstract is shortened.

- Please treat part 2 as a stand-alone paper. As such, the introduction should give a short review of the findings of part 1.

Reply: Thanks for the suggestion! A brief summary of Part 1 findings is now added at the end of Part 2 introduction section.

- I suggest to restructure the paper to first discuss all findings from AERONET (particularly Table 2) as the observational foundation for your methodology to identify extreme AODs. This would then allow to clearly define which sites are affected by biomass-burning aerosol to which extend. Afterwards, you can move on with the comparison to the reanalysis data (Figure 1 and Table 1) and the contribution of different components (Figure 4).

Reply: Thanks for the suggestion. We seriously thought about restructuring the paper as suggested. However we ended up using the current structure as we would like to provide readers with an impression of NAAPS-RA’s performance and then involve NAAPS-RA in later discussions, including general statistics of extreme events as the reviewer proposed. This is also because there are only limited AERONET sites over/near the Arctic region (10 from our study), which may not provide a comprehensive picture of extreme AODs over the region, and thus AERONET data are used as an evaluation tool and supporting dataset in this study.

- Redundant text: 95-102 (not needed and repeated later anyway), 119-121 (method section), 197-200, 425-429, 457-459 (should be clearly described in the methods section)

Reply: Thanks! Line 95-102 is actually not repeated anywhere in Part 2, despite that aerosol cloud impact and albedo impact was introduced in Part 1. But different from what was introduced in Part 1, the two sentences here list the “observable” impacts from extreme aerosol events (Part 1 lists some modeling studies). So we are keeping the lines. We have moved Line 119-121 to “Data and Methods” section, removed line 197-200, line 425-429. Line 457-459 is rewritten and provided in “Data and Methods” section.

- Section 3.3 should be omitted. Parts of its content – when authors list earlier observations of extreme events – should be moved to the introduction. Figure 5 and the brief discussion don’t add much insight and should be removed.

Reply: It is our strong preference to include Section 3.3 where Figure 5 (now Figure 4 after removal of Figure 2 following the reviewer’s suggestion) resides. Figure 5 gives readers a visual look of an example extreme smoke event from satellite imageries and lidar. We are reluctant to remove this vivid example (and solely extreme smoke example that is discussed in relative detail) in the paper. We’ve now mentioned the big field campaign examples in the introduction, and left the long list of extreme events in this section. We also added “More extreme BB smoke cases in the Arctic can be found in Sec. 3.3.” after the field campaign examples in the introduction section.

Specific comments

- Line 59: Omit Arctic.

Reply: Done

- Line 82: TOA introduced twice but there’s no need to use the acronym at all

Reply: TOA acronym is removed.

- Lines 125-135: move text to the respective subsections in the data section

Reply: Subsection “2.4 Methods” with the text is added.

- Line 153: it’s MODIS imagery

Reply: Corrected.

- Line 171: what’s the difference between quality controlled and quality assured?

Reply: To avoid confusion, “quality assured” is removed.

- Figure 2: Why does the figure include data from 14 stations when only 10 are listed in Table 1? Is there any discussion of Figure 2? Also, why not just add a line with the regression parameters for all data points to Table 1 and omit this figure?

Reply: We’ve accepted your suggestion, and removed Fig. 2. The regression parameters from Fig. 2 is now incorporated into Table 1. “14” was a typo.

- Figure 3: The definition of pairwise (or better temporally and spatially matched) should be provided in the method section and not in a figure caption.

Reply: Now the definition of “pairwise” is in “Methods” subsection.

- Figure 6: It is not clear what is shown in the maps. Please properly describe the data treatment in the methods section.

Reply: With the removal of Fig.2, Fig. 6 becomes Fig. 5. In 2.4 “Methods” subsection, there are descriptions as “We define extreme events as those corresponding with AOD exceeding the 95th percentile mark in 6 hr or daily AOD data relative to climatological means at a specific location or across a given region (the region north of 70°N for example)..... To simplify some of the discussion below, we frequently employed the symbol “AOD_n” to represent the AOD associated with the n% percentile of its cumulative (histogram) distribution.” We have now added “AOD₇₅, AOD₉₀, AOD₉₉, AOD_{99.5} and maximum AOD are also calculated to show AOD gradient for high AODs.”

- Figure 9: please define spread in the methods section. No stars are visible in the plot.

Reply: With the removal of Fig. 2, Fig. 9 is now Fig. 8. We’ve added, in the figure caption, “The box and whiskers represent AOD at 95, 90, 75, 50, 25, 10, and 5% percentiles.” We’ve also replotted the figure to increase readability by increasing the size of circles, adding stars and moving legend from the right to the bottom. We’ve removed the “spread” sentence in the figure caption to avoid confusion.

- Table 3: There is no need for Table 3 as it doesn’t add to what is shown in Figure 10. Also, there’s no discussion of this table except for a brief reference such as see also Table 3

Reply: Thanks. Table 3 is now moved to the supplement as Table S1.

- Conclusions should be renamed to Summary. Also, please don’t re-introduce all acronyms.

Reply: “Conclusions” is now renamed to “Summary”. And all the definitions of acronyms are removed in the summary section.

Reply to review comments #2

Thank you for addressing and responding to my comments as well as the other reviewer's comments. Overall, the manuscript is improved, especially with the separation of the material into two manuscripts.

I recommend publishing as is. My only suggestions for revisions are a handful of minor typos I found in the revised manuscript listed below:

- Figure S2: "seaonal" to "seasonal"
- Line 293: space in-between 6 and hrly ("6hrly" to "6 hrly")
- Line 1191: I think "value" should be "values"?

Reply: We thank the reviewer for the positive review comments. All the listed typos are corrected.