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Comment on acp-2022-743

Anonymous Referee #1

Referee comment on "OMI UV aerosol index data analysis over the Arctic region for future data assimilation and climate forcing applications" by Blake T. Sorenson et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2022-743-RC1, 2023

Review of "OMI UV aerosol index data analysis over the Arctic region for future data assimilation and climate forcing applications" by Sorenson et al for consideration for publication in Atmosphere Chemistry and Physics.

The paper identifies the utility of the OMI aerosol index for observing absorbing aerosols over the Arctic. The virtues of the OMI dataset for this region lie in its wide swath and sensitivity to aerosols even over bright (ice and snow covered) surfaces, overcoming some inadequacies of visible sensors like MODIS and narrow swath of lidar like CALIOP. Several issues with the OMI data are identified, however, and the paper uses various screening criteria to quality assure the data for its use in a previous written up data assimilation methodology. This quality assurance-corrected dataset is used to discuss trends in absorbing aerosols over the Arctic region, and to identify recent significant increases in the frequency and size of aerosol events that are attributed to high latitude biomass burning.

The paper is fairly cleanly written and but requires some modification before it can be accepted for publication.

The identification of the seasonal "ring" features shown in Figure 1 are curious. Have these not been previously identified by the OMI team? And if not I suggest amplifying that this is an original finding as it would be a significant (i.e., important to know) aspect of the dataset that has not previously gotten scrutiny.

It does not appear from the figures that you are excluding any OMI data for sub-pixel cloud contamination. You note on line 70 that the AI is calculated in clear and cloudy conditions, but there is a QA screening in the Level 2 that attempts to identify mainly clear pixels (QA=0) from cloud-contaminated (QA=1). Why is that QA consideration seemingly not used in this study?

Line 93: "which seemingly latitude dependent" is not grammatically correct; please correct.

Line 103: Please be more precise about the "northern end" of the swath that is meant in screening for unreported bad scan rows. The discussion of this issue seems to suggest that it is a high latitude feature and not significant at lower latitudes (al a Figures 2a & b). Is that correct? Why would that be the case if it is indeed a physical obstruction of the sensor?

Section 3.2: In the discussion of other observing condition related defects in the dataset there is little discussion about limitations of the algorithm beyond the fact of different algorithms used over different surface types. You might consider Colarco et al. (2017) who identified other issues with the algorithm that are perhaps relevant here: there are biases in the OMI aerosol index visible to the extent that the surface pressure of the actual atmosphere differs from the static dataset assumed in the retrieval, and maybe more relevant to this discussion there are identified issues with the radiative transfer used in the retrieval algorithm having to do with the calculation of the Rayleigh atmosphere scattering over terrain where non-linear RT impacts were nevertheless linearly interpolated between two extreme pressures and that were manifest in bias in the AI. This could be relevant over topographically variable regions.

Colarco, P. R., Gassó, S., Ahn, C., Buchard, V., Silva, A. M. da, and Torres, O.: Simulation of the Ozone Monitoring Instrument aerosol index using the NASA Goddard Earth Observing System aerosol reanalysis products, Atmos Meas Tech, 10, 4121–4134, https://doi.org/10.5194/amt-10-4121-2017, 2017.

Line 203: The description of the climatology construction requires some further elaboration. If I want to know the AI at a particular latitude/longitude point, does your climatology tell me that? Is there a multi-dimensional histogram at each lat/lon point binned as described in SZA, VZA, etc? Is there no time dependence then in the climatological value at a given point? I think this just needs some additional clarification. (And how many bins of SZA, VZA, …?)

It is not clear to me from Figure 7 that the conclusion there is no sensor drift is justified. Looking at the blue line are we supposed to conclude that it is stable after 2011? Because I see a lot variability in the maximum and minimum of the seasonal cycle (not to mention the high value after 2020 that is noted in the text). This analysis seems incomplete, or anyway not very convincing.

Line 295 and past: I don’t understand this spatial sampling bias between the perturbed and screened assessments. Why would OMI rows 56-60 necessarily and systematically miss smoke events at high northern latitudes?

Line 302: What is the rationale for appealing to lower boundary condition issues? What does that even mean in this context?

Suggestions for the figures:

Polar projection plots in Figures 1,2,3,5,6,8: Please put some lat/lon lines on the plots. In most cases you are referring in the text to specific latitude regions, so that would be helpful. Something like in Figures 9 & 10.

The continuous color bar in Figure 9a should be replaced with a discrete one since the years are in discreet colors (I think).

Figure 11d: Suggest changing the y-axis label to show integer only labels since it is an integer quantity plotted.