Atmospheric Chemistry and Physics Discussions

Atmos. Chem. Phys. Discuss., referee comment RC1
https://doi.org/10.5194/acp-2022-176-RC1, 2022
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Comment on acp-2022-176

Anonymous Referee #1

Referee comment on "Monitoring multiple satellite aerosol optical depth (AOD) products within the Copernicus Atmosphere Monitoring Service (CAMS) data assimilation system" by Sebastien Garrigues et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2022-176-RC1, 2022

This paper is an evaluation of the Copernicus SLSTR and NOAA VIIRS near real time aerosol optical depth (AOD) products for upcoming data assimilation purposes (for the CAMS system), based on statistical comparisons to CAMS as well as the AOD products which CAMS currently assimilates (MODIS and PMAp). The evaluation is global, for two 3-month periods that were characterized by different aerosol regimes. Offsets between the various data sets are explained, and interesting points about how scattering angle sampling differences between sensors (i.e. it's not just the spectral/spatial characteristics, it's when/where it's looking) are also raised. A follow up paper will go into more detail on the effects of assimilation of these products on CAMS.

The topic is in scope for the journal and is scientifically relevant. I appreciate the authors' reworking of the text based on comments at the Quick Report stage, which makes this version more readable and clearer. The overall quality of writing and presentation is now fairly good, and the paper's length is more manageable. I have a number of comments, below. All are fairly minor corrections/clarifications, with the more substantial comment being that the Conclusion should be rewritten. As a result I recommend minor revisions before the manuscript is suitable for publication in ACP. I am primarily a satellite person and so recommend at least one other reviewer of this manuscript is a data assimilation modeler in case there is something I miss on that front. Comments are as follows:

- Line 59: think this should be 2000, not 2001?

- Line 65: a citation for the GCOS requirements should be added. I think the numbers for some geophysical quantities change periodically.

- Table 1: the MODIS DT ocean uncertainty is missing from the table. I believe it is 0.04+10%*AERONET in Collection 6 onwards (based on Levy et al 2013 paper cited in the manuscript).

- Section 2.1.1: might be good to specify here again that this is the NOAA VIIRS product. There are NASA DB and DT VIIRS products as well. I see people cite the NASA papers using the NOAA products sometimes, and vice versa, so doesn't hurt to add a sentence saying directly that NASA products exist but are not used here. I don't know the NRT status of the NASA VIIRS ones at present, and see it makes sense to use the NOAA ones given they are NRT (and the resolution is nice too).

- Line 162: I think this is the first time the acronym TOA is used, I know what it means but it should be defined.

- Line 169: is there a paper or tech report citation about the issues with PMAp and SLSTR over land?

- Line 170: is this really how DB and DT are combined here? If so, why not just use the merged product provided within the files? It is not the same as gap filling DB with DT, there is some averaging and QA comparison done too. See the Levy (2013) paper mentioned earlier.

- Line 222: for non-modeler readers, it would be useful to state what the TL511 model resolution corresponds to in km or degrees. Is this the same as the 40 km resolution mentioned on line 230 or something different?

- Paragraph beginning line 248: if I understand correctly, the first guess departure will be useful for the absolute evaluation of the satellite products if the (un-assimilated) model itself is somewhat skillful. If the model is not good at a certain place/time then you wouldn't necessarily know whether the difference is due to model or observation errors, and conversely if the model were perfect you could use it perfectly to diagnose observation errors (but then assimilation itself wouldn't bring a benefit). Is that right? I suggest adding a sentence or two here for non-modeler readers to explain more why this is a useful metric and what the caveats/assumptions are. Presumably the fact that first guess departure is based on the model field including MODIS/PMAp assimilation from previous time steps, makes up for some potential errors in the model (assuming in that case that MODIS/PMAp in the previous time step were good).

- Figure 1, 2, 5, 6: the paper says the analysis is only for date with latitudes smaller than 70 degrees. The maps include data above 70 degrees (except for MODIS which seems to have a cutoff). So it's not clear if that data is used in the discussion of this figure or not. If data above 70 degrees are not used, I would suggest not plotting it in the maps.

- Mapped figures: these still say SDD for standard deviation, not SD like in the rest of the paper. I also wonder if it's possible to put the mean and SD on the same line as the sensor name, with the two-line plot titles there's a lot of space between panels which makes it a bit harder to visually compare than if they were closer together.

- Line 374: I wonder if here (or earlier) you could introduce an acronym FGD or a symbol for "first guess departure". The phrase appears a lot in this section, it would be easier if there were some shorthand for this term. I counted 22 uses on page 22 alone, and about a dozen on the corresponding land section (page 24).

- Line 376: if I understand correctly, negative first guess departure means the satellite is lower than the model field. Is that right, or do I have it backwards? For a non-modeler reader, as this is the first example given in the paper, it would be good to state this clearly to make sure people don't get the conclusions backwards. If that is correct then would it imply the model AOD is higher over ocean (since we know most satellites are also too high) – possibly because of the assimilation of biased MODIS and PMAp observations in the previous time step?

- Figures 7, 16, D5, D9: it would be useful to add the horizontal line y=0 here, as a reference for zero mean departure.

- Section 4.2: how are the unphysical negative AOD retrievals in the Dark Target land product handled here? Are they set to 0, set to invalid data, or something else? From

the Levy (2013) paper again, it happens about 20% of the time over land (see e.g. Figure 10 of that paper). As a non-assimilation reader it would also be useful to know how these are handled in the assimilation process because of course a negative aerosol mass would not make sense. This should be explained as it's an important long-term issue with that data product which is relevant for assimilation.

- Line 618: the Schugens reference should be Schutgens.

- Line 620: Sea Surface Temperature does not need to be capitalized.

- Lines 644-646: This comment about resolution seems speculative and unsupported to me. At those transport distances, I don't see why MODIS would not see the transported smoke at 10 km retrievals but VIIRS would at 750 m. From looking at imagery of the event, the source plumes fairly quickly spread out to more diffuse areas tens to hundreds of km wide. I think it is more likely that the differences in the model-scale aggregate are influenced by different populations of high vs. low AOD retrievals (real or artefact) in these two products, i.e. a pixel screening issue, not a retrieval resolution issue. If the authors want to make a claim here, evidence should be shown to back it up. For example showing examples of the source L2 data and of the L3 data for such a transported smoke case would quickly show what pixels from each sensor are available and what the retrievals look like.

- Line 675: again, this seems speculative and needs to be better supported with evidence or deleted. The water-leaving signal is not that large or variable at SLSTR wavelengths (green and longer), so the lack of a blue band would not be so important here. The MODIS DT ocean retrieval does not use the blue band either so it would not explain the MODIS-SLSTR differences. I don't know if the VIIRS one does, but either way, I don't think it dynamically accounts for pigment variations. So the sensors are all basically

using the same spectral information, i.e. green to swIR wavelengths. I think that differences in the Southern Ocean are more likely due to different tolerances of cloud contamination and 3D effects, which may influence SLSTR differently because of the dual view and resulting parallax difference in location for clouds. See Toth (2013) for a discussion of MODIS Aqua in the Southern Ocean: https://doi.org/10.1002/jgrd.50311

- Line 689: Sirish reference should be Uprety (Sirish is the given name, Uprety the surname).

- Line 710: I don't think "exploits" is the right word here. Rather, the MODIS retrieval LUT contains nodes at those wind speeds.

- Line 748: that Sayer (2018) reference is to the NASA VIIRS data products, not the NOAA VIIRS data products used in this study, so may not be directly relevant to this point (other than to show another algorithm as a point of comparison). It was not clear to me reading whether this was intended to be an example of surface influence or an attempt to explain the results of the present study.

- Section 6: as written, I did not find this useful as there was a lot of repetition with the immediately preceding Section 5. I suggest this is shortened and rewritten to focus more on what the abstract says the paper is about: evaluating the SLSTR and VIIRS data sets. I understand the actual assimilation will be analyzed in a follow up paper. But I think that the Conclusion here should maybe present a few brief expectations of how useful the data may/may not be, instead of repeating the previous discussion. For example, my guess is that the SLSTR product might not be useful to assimilate over ocean as it seems to be unusually low compared to the other ones. VIIRS on the other hand might be ok from NOAA20 but not SNPP, because SNPP seems to have radiometric calibration issues. So my (non modeler) takeaway from the results

presented is that neither of these products are likely to help CAMS much, at least so long as the current MODIS products remain available. Is that a fair assessment, and if not, why not? This is the sort of content I think the conclusions should be giving, i.e. don't repeat the results of the analysis but more talk about what they mean. There is some of that in the current Conclusions but not much.

- Appendix A: thank you for moving this section out of the paper into an Appendix, it makes the main paper more readable, and now if someone wants to know more details but not read the algorithm/validation papers this gives a summary.

- Appendix B: it's not obvious to me what the blue lines on the plots represent, what is it? I'm not sure they are useful and maybe they can be deleted to reduce clutter. I also think it's more useful to show the 1:1 line than what I guess is a regression line here (again, the Appendix doesn't say). That way one can more directly see whether one data set is higher/lower than another by whether they are above/below this line, without having to cross-reference the existing lines to the labels on both axes. The regression lines seem skewed by offsets at low-AOD conditions (as most of the points are there) whereas as a reader I am more interested in whether one is lower or higher than the other across the full range of AODs. That is less clear when showing the regression line than showing the 1:1 line would be.