Atmos. Meas. Tech. Discuss.,
doi:10.5194/amt-2020-124-RC2, 2020
© Author(s) 2020. This work is distributed under
the Creative Commons Attribution 4.0 License.

Atmospheric Measurement Techniques Discussions

Interactive comment on "TROPOMI Aerosol Products: Evaluation and Observations of Synoptic Scale Carbonaceous Aerosol Plumes during 2018–2020" *by* Omar Torres et al.

Anonymous Referee #2

Received and published: 10 June 2020

This paper briefly introduces a TropOMI aerosol data set based on heritage OMI UV algorithms by the Torres group (OMAERUV and OMACA). This provides UV aerosol index (UVAI), aerosol optical depth (AOD), and single scattering albedo (SSA). A comparison of AOD and SSA against data from selected AERONET sites is presented, along with a few case studies of extreme events. The concept of the paper is in scope for AMT. The quality of language is good. The topic is important because OMI is ageing and TropOMI is the next generation of this type of sensor (OMPS on SNPP and JPSS has some aerosol capabilities but is in other ways worse than OMI).

However, honestly, the current paper feels more like a conference proceedings or an

article for a Letters journal than a full scientific paper. It is brief and does not go into much detail. For a focused journal like AMT I think something much more technical is needed. Though I realise I am proposing a fair amount of work, I prefer that the authors expand this analysis rather than resubmit elsewhere, because I think a thorough accounting for TropOMI's capabilities for UV aerosol remote sensing is needed and is more or less missing from the literature. The authors are the right people to do this because they are the most expert with their data products. I know it is annoying when reviewers ask to do more work, but there is not enough content here to justify publication and I don't think that the article as written satisfies the scope a reader would reasonably expect. Case studies are one thing but by nature are typically unusual events and so looking at them may not give a representative picture of the data set as a whole. I recommend major revisions and would like to review the revision.

My main suggestion for expansion is to give a detailed comparison between OMI and TropOMI results. Users familiar with OMI need to know whether we can use TropOMI for the same types of research, and to what extent the same caveats/biases are found. Right now this is not answered in a thorough way. One big advantage of TropOMI over OMI is the spatial resolution. I would expect that this is important because those cases where the UV technique works well (absorbing aerosols) are also often strong and heterogeneous events. So the finer spatial resolution might mean both (1) less cloud contamination and (2) better AOD/SSA retrievals, because top of atmosphere radiance is not linear in AOD, so by resolving more spatial structure you become less sensitive to sub-pixel variations. If this is true in practice, great. If not, this needs to be shown and understood. It is briefly discussed in Section 3.1 but not supported by the plots shown, only by briefly mentioning other references. Here are some suggestions for relevant analyses to include:

(1) Show global maps so we can see how similar the big picture looks from both sensors. In my view the time series in Figure 5 isn't sufficient here because both data sets are heavily spatially and temporally averaged in it.

(2) Include OMI in some of the case studies (e.g. visual inspection of maps).

(3) OMI validation results could be presented alongside the TropOMI data. I know the validation has been published elsewhere but it will be clearer to the reader if plots are shown next to one another with the same axis range, etc.

(4) Directly plot (as a scatter density diagram) the AOD and/or UVAI from OMI and TropOMI, for collocated pixels (i.e. same scene, same time, similar geometry) at level 2 resolution. The orbits should overlap frequently. Then we can see if there's much scatter, if it's a straight line or not, etc. I don't know how much collocated data is needed to get a meaningful comparison – perhaps the case studies give enough, perhaps it has to be done on a month's worth of data. MODIS or VIIRS data could be useful for extra context (and filtering); I know and the manuscript mentions that the TropOMI orbit choice makes it possible to take advantage of SNPP VIIRS for e.g. cloud masking.

The above comments and suggestions all apply (potentially) to the DSCOVR-EPIC sensor, too, although OMI is the more well-known and mature record so probably makes better sense to baseline against. Though I would certainly be happy to see a three-way (OMI, EPIC, TropOMI) comparison!

Other comments on the study are as follows:

Introduction or section 2.1: somewhere here it would be good to contrast TropOMI capabilities (e.g. spatial/spatial) with OMI and maybe TOMS and EPIC, since those are the main comparative products. The introduction mentions GOME and SCIAMACHY but those are less relevant since the authors' algorithms are from TOMS/OMI heritage and EPIC data are shown later. Maybe mention OMPS too as while a step backwards from OMI in terms of spatial resolution, it is used for UVAI and is the US operational follow-on for that. I know that there are TropOMI products in development on the Dutch side too – it's not clear to me whether those are public yet, but if so, there may be value in comparing and contrasting with those too.

Section 2.2: if I understand correctly this section states that (1) there is a 5-10% calibration difference between OMI/OMPS and TropOMI in the relevant bands in the standard calibration, and (2) because of this the authors do their own vicarious calibration. Is that right? Either way, this could be worded a little more clearly. What is the difference between the sensors after the vicarious calibration?

Section 3: clear statements and references about AERONET data products and versions used need to be made. For example, I assume this is version 3 level 2.0 direct Sun (Giles et al AMT 2019) and inversions (Sinyuk et al AMT 2020). However this does not appear to be actually stated in the paper. If this was not the versions used, the analyses should be repeated using the latest data versions.

Section 3.1: if the authors really believe that a relative uncertainty of 30% on TropOMI AOD is true, then by definition they should not be using linear least squares regression fits, because a relative uncertainty means that the assumption of constant variance of errors is broken. See for example standard statistics textbooks or web pages such as https://statisticsbyjim.com/regression/heteroscedasticity-regression/ . This issue could be addressed with weighted least squares. Ideally also the uncertainty on AERONET AOD (I think 0.02 in this spectral region) should be accounted for in the fitting. Also, if you expect a relative uncertainty then RMSE is not the best metric to be reporting since that is scale-dependent. . . others like relative RMSE would be more appropriate to quote instead/as well (and this would help tell you if it is really 30%). The statistical analysis here is not very appropriate. The authors may have used this type of analysis before but that does not mean it is ok to do something again if it is wrong.

Section 3.2: the authors use a 6 hour time window (3 hours each side) for the SSA comparison because morning/evening almucantar inversions have lower uncertainty than midday ones. The untested assumption here is that SSA does not vary much throughout the day. Ok, but version 3 also introduced hybrid scans which were specifically developed to solve this problem by sampling a larger air mass and scattering angle range during the middle of the day. This could be checked by using the hybrid inversions as well and seeing if you get the same results. Also, an explanation is needed for how the authors split the data into the three aerosol type categories for Figure 2 and the discussion.

Section 4: this feels like advertising. I agree that TropOMI results look impressive but (aside from a brief mention of AERONET AOD) there is no way to know how 'real' they are. This section feels like something you might put on a webpage or brochure to attract attention to your new data set, rather than a detailed scientific analysis. I am not sure what is best to do here. For a journal like AMT I'd rather than space was devoted to more technical, large-scale comparisons. Perhaps this aspect could be split off for a Letters journal. Or, expanded with more context from meteorology and other (space or suborbital) data records and submitted separately to ACP. I know this is a joint special issue but the content still needs to match the journal. It does not really fit here, and there's not enough detail presented to consider this paper an authoritative reference for these case studies.

Figure 6 and associated text: I'm not sure that it makes sense to show the EPIC results on the left panel. That's a different sensor, different resolution, different observation geometry (backscatter for EPIC). UVAI is sensitive to all of these things. Also, what is the scaling referred to in the left panel? That is not mentioned in the paper. I expect that the general point about the two events will still stand but it's not clear how much of the systematic difference (and scatter on the left panel) are a function of real differences in the smoke in the two events and how much is contributed by sensor differences. The paper is far too sparse in detail for a reader to judge, which makes the comparison less instructive.

Figure 6 legend: is the black dot in the left panel legend (12 km) meant to be a black line like in the right panel? If so, formatting should be consistent. If not, the difference needs to be explained.

Section 5: "The NASA TropOMAER aerosol algorithm is a modified version of the one

applied to OMI observations." Wait, what? Section 2 describes the OMI approach but doesn't clearly state that there are modifications. What are these modifications, why were they made, what effect does this have on the results, and will they be back-ported to OMI? This all needs to be addressed in the paper.

Interactive comment on Atmos. Meas. Tech. Discuss., doi:10.5194/amt-2020-124, 2020.