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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 #1**

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Summary: This manuscript introduces the TropOMAER aerosol retrieval algorithm. The algorithm is essentially the heritage OMAERUV algorithm from the OMI collection, now modified to be applied to TropOMI data instead. In this adaptation process, the ability to retrieve above cloud aerosol OMACA has been included. The introduction to the algorithm itself is quick. The authors point out two major differences from OMAERUV: (1) TropOMI's finer spatial resolution (2) still evolving radiometric calibration. There is a quick evaluation section showing TropOMAER retrievals against 12 selected individual AERONET stations for aerosol optical depth (AOD) and an aggregation of all 12 stations for single scattering albedo (SSA). Then the bulk of the manuscript demonstrates TropOMAER in three interesting and newsworthy biomass



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burning events.

Assessment: There is much merit in this manuscript. The three examples, especially the third example, are scientifically extremely interesting. However as currently written, it is missing too much detail for publication in AMT. AMT is where algorithm developers, such as these authors and myself “talk shop”, and where we document the details of algorithms and validity of our products. While the heritage algorithms are well-documented in the literature, porting an algorithm to a new sensor introduces new challenges that are very interesting to other algorithm developers and should be included in a paper like this one. This manuscript could easily be adapted into a form that would be appropriate for AMT, if that is what the authors want to do.

These are the points that would make the manuscript ready for publication in AMT:

(1) much more description of the algorithm itself, even if that description were partly reiterated from previous publications.

(2) highlight differences between OMI and TropOMI instruments, between OMIAERUV and TropOMAER algorithms, most importantly between results from each sensor. Of prime interest to potential users of TropOMAER products who have been using OMI products is how do the products from the new sensor compare with the products from the old sensor. The only place I see a hint of that is the plotting of OMI retrievals with TropOMI retrievals on the time series in Fig. 5. However, that figure is not satisfying. Much more interesting than the 15-year time series would be a difference time series during the TropOMI era and a scatter plot of TropOMI against OMI, even on a monthly mean basis.

(3) evaluation of TropOMAER should be expanded. There should be an effort to trace the consequences of the finer spatial resolution and issues with calibration to the evaluation. Right now the authors skirt these issues without really proving anything.

For example they mention subpixel cloud contamination being absent in most of the





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validation sites. However, when I look at the 12 panels in Figure 1, I see no qualitative difference between the 3 sites mentioned as having subpixel cloud contamination and the other 9 sites. If there was marked improvement from Ahn et al., 2014, then that improvement should be demonstrated in this paper. I should not have to call up that paper and run my eyes between two different figures in two different papers to see the improvement.

Later they mention needing a finer resolution surface albedo map, and there is also mention of the calibration causing some of the offset in the validation plots. Each of these issues is very interesting to another algorithm developer, like myself, or to potential users of the products. AMT is the right journal to present an analysis of these issues, and prove their consequence on the retrievals. Currently that analysis is missing.

(4) Slow down and present the details. I felt that there was a rush through the “boring” algorithm piece of the paper in order to get to the “exciting” demonstration with the big biomass burning events. There are many details left behind in the rush:

There are many acronyms never properly introduced:

p.2 line 2 should put (SWIR) after shortwave infrared. P2 line 5. ESA and DLR? P2 line 28. Should put (ALH) after aerosol layer height P5 line 5. UVAI is never defined as an acronym, and worse, it is never defined as a product. Suddenly it is being shown in figures and being used as a fundamental part of the analysis. P6 line 25 SAM? P6 line 33. What are total mappers?

The concepts of Level 1 and Level 2 data are not explained (p2 line 5). Exactly what AERONET data are we looking at? Version 2 or 3? Levels 1.5 or 2? There is no explanation that AERONET AOD has a documented uncertainty of 0.02 in the UV, but that the SSA retrieval is a retrieval with much broader error bars. There is no explanation of why or how these 12 stations are selected, nor what the time range we are looking at.

(5) Provide more detail in the demonstration section.

Figure 3 would benefit greatly by adding a swath just to the west of the swath shown. Right now there is a lot of description of fires and smoke in California, the Pacific Northwest and British Columbia, but none of those areas are shown in the figure. Only the areas downwind.

P6. Lines 1 to 6. Is this method here the manifestation of the ACA part of the TropOMI retrieval that is mentioned at the beginning? If so, then please make that clear. If it is a different method, then explain why the referenced ACA method is not used. If not, then is there any demonstration of the ACA TropOMI method? ACA is an important new addition to OMIAERUV, and should be highlighted or discussed if this is going to AMT.

P6 Line 10. The extinction-to-mass conversion is important. The appendix should be referenced here.

P6 lines 13-16. Is there a physical basis for this? This is important, and how the UVAI-AOD relationship relates to height, and especially to height in the stratosphere needs to be explained. Remember that UVAI jumps in suddenly with no introduction. It would be worthwhile to take the time to explain it, and some of the physics behind the whole interrelationship between height, AOD, UVAI and absorption. Maybe in Section 2?

P6 line 25 to P7 line 2. A lot of numbers are given here and these are means with uncertainties surrounding them. The uncertainty is given at the end of $\pm$40%. It would be helpful to explain how the mean is derived (for what density) and what is the interplay between assumptions of density and uncertainty in height.

P7 lines 27-33. This is very interesting, but the figure doesn't really portray this information well. Figure 5 needs to become more informative.

(6) All the captions need to more descriptive. Be sure to give details on specific data, be sure to describe what is shown in each panel, what wavelength is being shown,

what temporal resolution is being plotted (fig. 5), what do each of the colors in the color bars represent. But in general a LOT more information needs to be in the figure captions.

Suggestion: It occurred to me that this manuscript might fit a “letters” journal much better. Right now it is not too long. The authors would need to triage their figures down to 4. Perhaps Figs. 1, 3, 5 (with a bottom panel showing the difference between TropOMI and OMI) and 8. Then the very short description of the algorithm, evaluation and methods would be appropriate, and the purpose of the paper is NOT to describe TropOMAER, but to illustrate these biomass burning events. The point of the paper shifts from an “atmospheric measurement technique” to a better understanding of the Earth’s atmospheric phenomena. GRL would be a possibility, but also ERL.

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