Atmospheric Measurement Techniques Discussions

Atmos. Meas. Tech. Discuss., referee comment RC1
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Comment on amt-2021-32

Anonymous Referee #3

Referee comment on "Ozone profile retrieval from nadir TROPOMI measurements in the UV range" by Nora Mettig et al., Atmos. Meas. Tech. Discuss., https://doi.org/10.5194/amt-2021-32-RC1, 2021

The paper "Ozone Profile Retrieval from nadir TROPOMI measurements in the UV range" by Nora Mettig et al. provides a nice characterisation of the TOPAS algorithm, and its sensitivities to SZA, VZA, different retrieval scenarios, and shows a calibration correction.

Comments:

- Line 42: With the last S5 launched planned in 2038, and an expected lifetime of 7 years, the nadir mission runs into the 2040s if my information is correct. https://www.eumetsat.int/metop-sg

- Line 120+121: Is the ratio between earth- and solar-spectrum actually stable enough to assume that the effects cancel out? Did you check, or is this an assumption?

- Lines 126 and 127: the values '15' and '32' are not insightful to the reader. Do they refer to specific named flags, or is it a confidence scale (and if so, what is the upper limit)?

- Line 166: 24 hours time difference is areally a long time for locations with gradients (i.e. winter hemispheres and the spring season in the Southern Hemisphere). At a surface wind speed of 5m/s = 432km distance in 24 hours. Wind at higher levels is much stronger. This means that you are potentially looking at a retrieval from an air mass that is not comparable to the air at the time of the sonde or lider measurement. The air masses sampled may even be outside of your distance criterium. Can you address this issue?

- Line 215: Why subtract a polynomial? This is not motivated in the text (up to now).

- Line 223: To get an estimate of the potential error you introduce with scaling: is there any indication how this WFDOAS method performs with TROPOMI data in terms of accuracy? Or is this explained in Weber (2018)?

- Line 253: It's not clear to me what the purpose of the third pseudo-absorber is. You mention that it is wavelength independent, but Irrad is not constant across the spectral range. Please clarify.

- Line 256: The subtraction of a polynomial is still not motivated. Why is this needed, and what is its effect? How are the factors of the curve established? (Or did I miss something?)

- Line 277: "Within one iterative step": Maybe you mean: "In the first iterative step..."?

- Line 390: Somewhat more of what? Somewhat more of the shape than just the total ozone content?

- Figure 5: The discontinuity at 300nm is quite profound. Is this due to a different binning factor, or just an inconsistent channel transition? Is the variability for wavelengths > 300nm time dependent degradation, air mass type mis-match? Please discuss.

- Figure 5: Soft corrections based on differences between expected/simulated and measured spectra have been done before, but I think that those corrections were based on more than just one orbit in a day. Have you considered taking a full day of data to get corrections per cross track position? I think it would stabilise the effect of potential mis-matches between the simulated spectrum based on input ozone and the measured spectrum based on the real ozone distribution.

- Figure 5 & Line 420: The calibration error of 20% for small wavelengths could (in part) be caused by a lack of Rayleight scattered light in your RTM. You may need to extend your model to 0.01hPa as Top Of Atmosphere to catch those photons in your simulation. You now seem to stop at 60km / 0.2hPa according to the conversion table in https://www.engineeringtoolbox.com/standard-atmosphere-d_604.html. I don't think that is high enough. Would it be possible to check with one (subset of an) orbit to check whether this helps to reduce the bias? If you choose to do the test and it does not change the bias curve, then just mention that in your response to my comments. If it does change the curve significantly, then act accordingly.

- Line 421: The +40% you mention, is that related to a spectral range where magnesium has absorption lines?

- Line 431: is this a fourth pseudo absorber, on top of the 3 already mentioned earlier? What is the distribution of this scaling factor for retrievals across the Earth? Have you plotted it on a map? Does this show an expected pattern? The point I want to make is this: Using a fitted multiplication factor to explain some bias away is risky when it happens unsupervised.

- Line 455: "The differences vanish in the altitude range where the retrieval is less sensitive". Please be more specific. In some the altitude ranges where the retrieval is less sensitive, like the lower troposphere, the difference increases. Or is something else meant?

- Line 460: About the +40%: With a 1000 km colocation distance and 24h time difference, the difference in observed and reference profile can be large, especially in the UTLS. Can you tighten up those spatial and temporal colocation windows and still have a statistically relevant result, or is the dataset too limited for that?

- Line 465: Would be good to re-iterate that you refer to the raw comparison, not to the AK smoothed comparison (which goes out of the 10% range).

- Figure 9: Why do Lauder and OHP stop near 45km? No data provided above that altitude?

- Line 506: "Low ozone levels are related to cloud coverage": Are these for strictly completely cloud covered pixels? For partially covered pixels is there info below the cloud top?

- Line 518: Please explain what you mean with the words 'and without jumps'.

- Line 554: "From this... regions". How can you derive the benefit value of the TOPAS retrieval from a comparison of an a-priori (climatology) with MLS and OMPS? Whether or not you use the same a-priori climatology for TOPAS or not, if no TOPAS retrievals are used in plots I and J, then I do not see the basis for the statement in the paper. Please explain. Maybe I am missing a step in the logic.

Textual comments:
- Line 26: remove comma after '1980s'
- Line 35: remove space before 'launched'
- Line 74: "The main objective of this study..." I think this could be the start of a new paragraph.
- Line 88: LIDAR is an acronym. There are more occurances in the paper.
- Line 220: double periods after a-priori
- Line 220: mention of P and T profile without mentioning its source, and then at line 225 a repeat with origin from ERA-5. Please mention only once.
- Line 225: period after '(Hersbach et al., 2020)'
- Line 257: has been proved --> has been proven? (To prove, has proved, has been proven? Please check with a native English speaker).
- Line 293: 'is run then' --> 'is then run', or 'then runs'
- Line 383: 'an degrading' --> a degrading
- Line 428: remove 'the': with time
- Line 489: positiv needs an extra e

Purely for consideration:
- Line 21: the use of the word 'toxic'. Maybe it is a philosophical question: is ozone toxic or just very harmful when it comes into contact with other material? Personally, I would use the word toxic for substance that can cause death by ingestion into the cell material. Ozone primarily affects the surface of the skin / lungs as a reactive molecule. Since we are often not talking about internal effects of ozone inside the human cell (or plant cells), I would consider the use of the word harmful instead. I see that you have native English speakers as authors that can speak out about this. It may be that my knowledge of English nuances is too limited. I leave it up to the authors to decide.