

Atmos. Meas. Tech. Discuss., referee comment RC2  
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## **Comment on amt-2021-144**

Anonymous Referee #2

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Referee comment on "Total water vapour columns derived from Sentinel 5P using the AMC-DOAS method" by Tobias K uchler et al., Atmos. Meas. Tech. Discuss.,  
<https://doi.org/10.5194/amt-2021-144-RC2>, 2021

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Total water vapour columns derived from Sentinel 5p using the AMC-DOAS method, by Tobias Kuechler et al.

The paper describes the application of the existing AMC-DOAS algorithm, for the retrieval of total column water vapour, to measurements of the TROPOMI instrument on Sentinel-5P. Furthermore, an inter-comparison is carried out with other data sets. These other data are partly also based on TROPOMI data, and partly independent. As the AMC-DOAS algorithm has previously been applied to other satellite instruments, there are no big surprises here. However, some algorithm improvements were made, notably albedo correction and cloud filtering, that clearly improve the results. The paper serves as a useful description of the improved algorithm and the AMC-DOAS data product from S5P.

In general, the paper is well written but there are several issues that need clarification and/or more discussion.

See my detailed comments below.

The paper is appropriate for publication in AMT once these issues have been addressed.

Detailed comments

in general: the use of the word "ground pixel" is confusing. A ground pixel may be anywhere on the Earth surface.

It seems you mean across-track [ground] pixel or viewing angle. Please replace or clarify in the text.

p.8 Equation(1) indicates that no additional cross-sections are used for Ring, spectral surface albedo (vegetation, chlorophyll) and liquid water. Please give a rationale.

p.9 line 4. Is the viewing zenith angle (across-track pixel number) not used in the radiative transfer ?

p.9 line 19. The "default" albedo of 0.02 is very low for land surfaces. Mention (later in the text) that this often may result in a large albedo correction. Please discuss if the limit of 0.8 on the airmass correction factor (p.9 line 28) or the limit of  $>0.6 < 1.2$  on the albedo/cloud correction factor (p.13 line 13) can cause a land/sea bias in partially clouded scenes, because over land you "always" have an offset to the correction factor compared to sea.

p.9 line 31: Why would higher spatial resolution result in a higher AMF ?

p.12 line 3: Is there no dependence on viewing angle ?

p.12, 13. It is unclear from the text how the albedo correction is done in relation to Eq.(1)

Is it a correction that is only applied to the factor "a" (AMF correction factor) and not to the saturation  $b_{\lambda}$  ?

Equations (3) and (4) seem to suggest so. But isn't it that not-so-low clouds may have significantly less saturation ?

Or is Eq.1 with modified "a" used for the "clear" part of the scene and is there an additional similar equation for the cloudy part with a LUT of a,  $\tau_{O_2}$ ,  $c_{\lambda}$ ,  $b_{\lambda}$  as function of CF, CTH, albedo, SZA, VZA ?

Is it then not a correction at all but rather a calculation method similar to Eq.(1) ?

If it is a correction to Eq. 1, is there any difference to the LUT with fixed surface albedo e.g. in resolution of the parameter grid ?

Please clarify and rework the text.

p.13 line 11. Please discuss uncertainties in the correction for cloud shielding that are related to the assumption of a fixed H<sub>2</sub>O profile.

p.13 line 20. There is a very large contrast between ocean albedo and cloud albedo. I would expect that small errors in the FRESCO

cloud fraction can induce large errors in surface albedo. Please discuss.

p.14 line 1. Please do not use "instrumental striping" or "striping" for your TCWV dependence on viewing angle.  
In the OMI / TROPOMI context the word "striping" is generally used to describe a systematic across-track pixel-to-pixel effect caused by systematic differences of the solar calibration spectrum across the CCD.  
OMI/TROPOMI striping is visible over ocean and over land.  
Your effect is only over ocean (probably NOT instrumental) and has a very smooth viewing angle dependence.  
Its origin is definitively different than that what is commonly named "striping".

p.14 line 2. What do you mean by "our simulation". The (simplified) clear sky model?

p.14 line 2. "Consequently, we assume that they are related to instrumental features"  
This conclusion seems very premature to me. Are there no effects that you have neglected in the simulation ??

First of all, why would an instrument effect be visible over ocean but not over land ?

Second, if I look at Fig.2b, it almost looks as if near the sub-satellite point the negative deviation is largest (glitter ?).

In Fig.2d it seems that the albedo correction removes most of that north-south difference over ocean.

Nevertheless, can glitter not be a reason why your simulation doesn't match the observation ?

Same for maritime aerosol which is always present over areas with whitecaps / large wind speeds.

In general, aerosol scattering introduces an azimuth dependence that is not covered by your retrieval model.

Please note that Grossi et al.2015 (Total column water vapour measurements from GOME-2 [...] AMT 8, 1111) also found in their H<sub>2</sub>O retrievals a strong scan angle dependency over ocean, which they attribute to Cox-Munk like BSDF effects.

Your albedo correction may take some of these effects away, but to conclude that a similar effect seen by another retrieval algorithm in another instrument must be instrumental because it doesn't match your theory is a long shot.

There is nothing wrong with an empirical correction if you have a simplified theory that doesn't match observations, but don't

blame it on the instrument unless you have a good reason to do so. (also review p.29 line 13)

p.14 line 4. "There is no dependence on season or position of the instrument." I think this needs a bit more discussion how you reach that conclusion.

Also Grossi et al.2015 fit a polynomial as function of viewing angle, as you do. However, their correction values are

not only a function of scattering angle, but also vary with latitude, and they vary from month to month.

It may well be that your albedo correction implicitly takes care of these dependencies.

That would be interesting to know.

And what did you look at to conclude that position of the instrument doesn't play a role?

For example, do you get in Fig.4

exactly the same distributions if you split the dataset in northern and southern hemisphere?

How is this for December or June when the subsolar point is at maximum/minimum latitude?  
(or for better statistics: how compare Dec+Jan north/south to Jun+Jul)?  
Of course you may still use a single curve for correction if that's your preferred algorithm.  
But it would be good to know limitations of that approach.

p.17 line 19. "The averages are calculated with weighting according to the latitude."  
Please be a bit more specific how the weighting is done.

Section 4.1.3 and 4.1.4 Do MPIC and SRON use the same surface height as AMC-S5P ?

p.22 line 9. What is the difference between FRESCO used by AMC-S5P and FRESCO-S used by MPIC-S5P ?

p.24 Fig.8 Can you show the average cloud cover in this month (as proxy the number of filtered/valid observations) ?  
Is there a relation to negative values (e.g. in the tropical rainforest)?

p.28 line 17. The "tongues" in southern parts of the Pacific have also turned up in various other comparisons between optical sensors and SSMI / SSMIS. It may be associated to cloud cover, therefore see also previous question.

p.29 line 22. For clarity, add "global average" or say this is the systematic global bias.

p.29 line 30. "The standard deviation of the differences..." Which differences ? Global monthly means ? Monthly means per grid-cell ? Daily means per grid cell (based on 2 months) ?  
You need to be more specific here.  
This comment also applies to similar statements in the Abstract.

p.30 line 4. The natural variability should be smaller than this, otherwise the measurements / retrievals would be error-free which they are not. Apart from the fact that "natural variability" is a dangerous term because it varies with spatial / temporal differences.  
Please be more precise and reword.