

Interactive comment on “The operational cloud retrieval algorithms from TROPOMI on board Sentinel-5 Precursor” by Diego G. Loyola et al.

Anonymous Referee #1

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This paper accomplishes two tasks: first, it provides a scientific update of a cloud retrieval algorithm and, second, it summarizes the suite of cloud products that will be available upon the launch of the Sentinel-5 platform with the TROPOMI payload.

As such, the readership can be wide and mainly made of two groups of individuals: experts in the field of cloud remote sensing and users of future TROPOMI data. This not only sets higher-than-usual requirements on the amount and quality of information to be conveyed in such a paper, but also demands a mixture of technical and scientific writing style.

In fact, while many concepts can be understood by the expert, users might not have the expertise and the required knowledge to understand the paper, especially when it goes down to error budgets and physical reasoning that support the conclusions of the

presented work.

Based on the above reasons, I think that this potentially important paper can be greatly improved with respect to readability and scientific information and publication should be warranted upon **major revisions**.

Specific comments

Abstract

The sentence provided on the higher accuracy of cloud properties derived from the NIR as compared to the TIR is indeed correct, but it is misleading for the reader, because she/he can think that a NIR-TIR comparison is one of the topic of the paper, which is not. So, this statement suits best as part of the introduction or the outlook, but not in the abstract, where, in my opinion, only an objective summary of the main matter of the paper should be given.

As an interesting topic on its own (the NIR-TIR comparison), I flag to the authors that within Cloud_cci (Stengel et al., 2017), TIR retrievals from (A)ATSR are compared with retrievals derived from a combination of TIR and the NIR oxygen A-band channels by MERIS (Fig. 8, p. 21, third panel from above, CTP [hpa]). As outcome, one can appreciate that the addition of the oxygen A-band from MERIS corrects for photon penetration depth issues of the TIR channels and the found average bias amounts to approx 60 hpa, which translates to approx 0.8 km.

Consistently, one recent study (Lelli et al. 2016) compared cloud properties derived from the oxygen A-band with the TIR-derived cloud heights of AATSR. It can be seen that TIR cloud retrievals are indeed placed lower (as the ROCINN_CRB) than the ones derived from the NIR with a scattering cloud layer model, as the ROCINN_CAL, by an average amount, again, in range 0.6 - 1.0 km.

Once the accuracy of ROCINN_CAL and ROCINN_CRB will be assessed, it becomes reasonable to state that the oxygen A-band delivers more accurate cloud heights than

the ones from TIR channels (albeit uncorrected).

Section 1, Introduction, p 2, l 8-12

Keeping in mind, as research outlook, the impact that a change of the used cloud model in cloud retrieval algorithms can have on the accuracy of retrieved trace gas columns, I appreciate a more detailed presentation of past work (facts and figures) in the field. In fact, the sentence “These studies have shown that cloud fraction . . .” is too general knowledge and does not properly convey the importance of the issue to be tackled.

It could be also somehow inaccurate, because when looking at du Piesanie et al (2013), the authors assessed the accuracy of SCIAMACHY water vapor columns as function of changing cloud fraction, optical thickness and cloud top height. They found that, using a scattering cloud model and the OCRA cloud fraction (making their results even more appropriate for this paper), CTH is the most critical parameter for water vapor, while cloud fraction and optical thickness are somewhat less relevant.

So, please, expand this paragraph, briefly reporting past results about trace gas accuracy and information on the cloud model assumption that has been respectively used to derive them (wherever available and appropriate).

Introduction, p2, l13

Is the spatial resolution the same for all TROPOMI bands? If not, please, report the correct information and briefly discuss how different footprint sizes can influence a joint exploitation (e.g., UV-Vis-NIR and SWIR).

Introduction, p2, l17

Overpass time of the mentioned sensors? This is important for the extension of the data record, as different sensing times will record different atmospheres.

Section 2, p3, l5

What is OCRA CF needed for as “baseline input”?

p3, l10

It is said the the ROCINN_CAL is here presented for the first time. Then one might wonder where was the ROCINN_CRB model presented? Please, provide reference.

p3, I10-17

This paragraph needs additional details on the errors as function of CRB/CAL, on the same line of thoughts of the impact of the cloud model on the accuracy of trace gases.

Section 2.1, p3, I24

References for ROCINN algorithm?

p3, I27-28

Two aspects are not clear here. (1) why the IPA allows 1-D plane parallel RT of cloud-contaminated scenes and (2) whether the previous statement also holds for future TROPOMI measurements due to 3-D effects. Please, discuss this aspect.

p4, I6

PMD-derived cloud fraction benefits not only of the spectral coverage but also of a spatial resolution finer than the science channels. So, please, mention this.

p4, I10

The heritage OMI cloud fraction algorithms need a bit more details to make the reader understand how the cloud detection works. I might understand it, but it is not something all readers can follow.

Section 3, p4, I22

Figure 1 contains a block which is not properly described in none of the following sub-sections, the “internal store”. The authors need to address (and amend the manuscript where appropriate) the following questions: (1) Why the need of a-priori selection if the brightness criterion should already deliver a minimum reflectance?

(2) What is the climatology used for?

(3) What climatology? Source, time-space aggregation? Quality of the values? Is a climatology appropriate and does it have shortcomings for the task?

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Section 3.1, p4, I28

It's the first time I read the terminology "ground-cover projection". What is this?

Section 3.2, p5, I8

It is said that reflectances are independent of atmosphere and line-of-sight. What do aerosol, Rayleigh and the surface do? Especially for the latter, does surface reflectivity change over the time needed to build the composite? This is crucial, especially when thinking at a small footprint. Please, add information on the impact of these three components on the determination of cloud fraction and the construction of the composite.

Equation 2

Is the comma correct here?

p5, I14

It is difficult to understand the correct domain of the gb-chromaticity diagram. What is exactly the $(1/2, 1/2)$ point referring to?

Section 3.3, p5, I26

I don't understand why the functions max and min must ensure that cloud fraction is confined in the interval $[0,1]$. Aren't already the cloud free reflectances ρ_{cf} the minimum available for the scene and aren't the β already compensating for radiative affects? What are the physical units of the coefficients α and β ? Are they unit-less?

Section 3.3.1

Recalling that specular reflection occurs when the viewing zenith angle equals the angle of illumination, given zero azimuth, could the authors briefly add an explanation of the need of a reflectance ratio criterium, instead of only geometrical consideration?

Section 4, p6, I19-20

It is said that the limitations of the CRB model are already noticeable with GOME-2. Where to find information on this? What limitations? Please, explain.

p6, I22[Printer-friendly version](#)[Discussion paper](#)

When the authors write that the layers are optically uniform, what properties are they addressing? LWP, droplet phase function, number concentration or? Please, add information on what optical properties are kept uniform.

Section 4

While the technique of wavelength recalibration is often omitted in modern papers about cloud remote sensing, it is relevant on its own. The authors might want to provide here more technical information so that the reader can independently implement it. Among the details to be provided, the following turn out to be useful: spectral sampling of the reference solar irradiance and source; fitting procedure, description of polynomials used in the spectral bins to find the optimal grid and iterations; value of calibration accuracy that can be achieved; references to past literature and technical documents, whenever appropriate (e.g., van Geffen and van Oss, 2003).

Section 4.2, p7, I11

How many scattering layers are clouds made of? Please, provide this information

Section 4.4, p8, I12

I am puzzled by the statement that the “desired total intensity I will incorporate the effects of polarization”. Since we are placed in the NIR region and that the authors state that the thermodynamic phase of water is not relevant for the task under consideration (implying that the retrieval algorithm will not discriminate between water and ice, the latter best seen looking at Stokes Q), I do not see the strict need to simulate all components of the Stokes vector. Could you please clarify in the text how and why you do run VLIDORT? If you have pre-calculated all Stokes components, but you interpolate to find the match between measurement and forward intensity only for Stokes I? Is this a requirement for future applications at trace gas retrieval?

p8, I15

Please, provide the spectral resolution in nm instead of wavenumbers.

p8, I21-22

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Please, state here whether your algorithm will be sensitive to the ice phase.

p9, I9-13

As far as I know, the accuracy of a neural network (NN) approach depends on the training set. Do I correctly understand that here the training set is purely synthetic and is made of NIR radiances, without external real datasets as, for instance, from measurements in the thermal infrared?

Moreover, I find confusing the role of the NN within the ROCINN framework for TROPOMI. In an earlier version of the ROCINN algorithm (Loyola et al., 2007), as applied to GOME measurements, the NN was used to solve the inverse problem, whereas the NN of this TROPOMI-ROCINN version solves the forward problem and the inversion is left to Tikhonov-Phillips.

If this is true, this information should be clearly stated in the paper to avoid confusion and justified from the perspective of the training sets. So, please, help the reader fully understand what development has been undertaken from the old ROCINN to this new version.

Section 4.7

This section has several shortcomings and seems to be written in haste. Basically, explanation of the results presented in all three figures and geophysical settings of this exercise are missing. I list my remarks in the following bullets.

1. The space of sampled geometries and cloud properties is not given. Thus, the reader does not know if the biases of the CRB retrieval (Figure 5) are coming from low-, mid- or high-level clouds.
2. Figure 4 is clearly not informative. Not only are the curves not color-coded, but one cannot understand what spectra are overlapping and why. I suggest to re-

move it, also because the shape of the oxygen A-band as function of changes of the main atmospheric properties under consideration is already well-known.

3. It is well-known that COT accuracy is strongly dependent on the viewing geometry. So, Figure 6 (left) should also address this information and provide the reader with more confidence that deviations from the 0-bias median are due to viewing-geometries (or are there other reasons?). Either increase the size bin of the x-axis, or color-code as function of VZA/SZA.
4. As long as the range of retrieved COT is not given, recalling that COT spans three orders of magnitude and that COT errors are usually non linear, the left plot of Figure 6 is little informative. So, please, provide more explanation on this aspect.
5. Figure 6 is not consistent, because COT bias is juxtaposed for one model (CAL) with the cloud albedo (CA) bias for the other model (CRB). And because no information is given on the correspondence between COT and CA, one cannot judge the performance of the two models within this task. So, either add also a CA bias plot for the CAL model and a COT bias plot for the CRB model or provide a clear description on why the two plots can be regarded as the manifestation of the same process/effect.
6. Please, define in text (and in the figures/captions) how are differences calculated. Are these relative or absolute errors?
7. Please, provide in the text a physical explanation why the cloud albedo difference is not symmetric about the 0-bias line, while the COT bias is, and why should CA be likely underestimated with the CRB model, as the red PDF is slightly skewed into the negative domain.

Section 4.5, p10, I3

What are the other options the inverse framework allows? If the narrative of the paper

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requires this information, then provide it. Otherwise the sentence sounds odd and disconnected from the general flow.

Section 5, p11, I20-21

Could you provide exact figures on the error in COT due to uncertainties in surface albedo and size distribution parameters, in the same fashion you do for the influence of cloud geometrical fraction? The sentence is too general.

p12, I 1-4

Do you have a reference for the TROPOMI calibration exercise?

Section 5.1, p12, I 9

Where can the TROPOMI mapping tables be found? Are they publicly available? If yes, why not mention the source?

Section 6

It is clearly a matter of style, so, as suggestion, I would opt for compactness and avoid undue subsectioning, so that the flow of the paper isn't broken too much. I think it would suffice to rename the title of Section 6 and regroup the comparisons as follows

Section 6 "Application to OMI and GOME-2 and comparison with independent retrievals"

Section 6.1 "Comparison of OCRA with OMI and MODIS cloud fraction"

Section 6.2 "Comparison of ROCINN with GOME-2 cloud top height and thickness"

Section 6.1, p13 I9

I think the authors should check the sequence of figures, because the OCRA cloud-free background has numbering 2, while belonging to a later section.

Section 6.1.1, p13, I23

What kind of MODIS platform and product is? No reference is given here and the naming OMMYDCLD suggest that the authors use Aqua and not Terra. With this

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respect, the different radiometric performance between Aqua and Terra could also impact the zonal comparison of Figure 8. But in absence of a clear reference, no judgment can be given.

p13, I26-27

Are the overpass times of OMI and MODIS comparable? Could you please add this information, if relevant for the differences found in the zonal plot?

p13, I27

Can the author substantiate with references or with a physical reasoning the statement “The UV sensors are not sensitive to optically thick clouds”?

p14, I1-3

While it is clear that fixing the albedo of a cloud at 0.8 (a too large value and to substantiate this statement you can cite Lelli et al, AMT 2012 - and report the mean global cloud albedo value of 0.63 and 0.55 from ROCINN) leads to a lower cloud fraction because the radiative balance within a pixel must be conserved (even if, strictly speaking, this general statement should be first checked against the RT assumptions of the respective cloud fraction algorithms), it is not clear why OMI-derived cloud fractions are still different from MODIS, even without assuming a fixed cloud albedo.

In absence of a quantitative and third cloud fraction source, it is not sound to say that OCRA and OMAERUV are underestimating (MODIS could overestimate as well), but still a physical explanation for this discrepancy should be given. Is this a geometrical, radiative or sampling effect? For the latter, I mention that if the L2 colocation procedure is avoided and the authors deploy a resampling of downstream daily gridded L3 to match OMI spatial resolution, then biases can occur. One should consider the number of available measurements with respect to the gradient of the cloud property within the spatial box to be gridded (cfr. Levy et al. 2009).

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Figure 8 would be more informative if the zonal plots would be split for values above land and water masses.

References

Lelli L, Weber M and Burrows JP (2016) Evaluation of SCIAMACHY ESA/DLR Cloud Parameters Version 5.02 by Comparisons to Ground-Based and Other Satellite Data. *Front. Environ. Sci.* 4:43. doi: 10.3389/fenvs.2016.00043

Stengel, M., Stapelberg, S., Sus, O., Schlundt, C., Poulsen, C., Thomas, G., Christensen, M., Carbajal Henken, C., Preusker, R., Fischer, J., Devasthale, A., Willén, U., Karlsson, K.-G., McGarragh, G. R., Proud, S., Povey, A. C., Grainger, D. G., Meirink, J. F., Feofilov, A., Bennartz, R., Bojanowski, J., and Hollmann, R.: Cloud property datasets retrieved from AVHRR, MODIS, AATSR and MERIS in the framework of the Cloud_cci project, *Earth Syst. Sci. Data Discuss.*, <https://doi.org/10.5194/essd-2017-48>, in review, 2017.

du Piesanie, A., Piters, A. J. M., Aben, I., Schrijver, H., Wang, P., and Noël, S.: Validation of two independent retrievals of SCIAMACHY water vapour columns using radiosonde data, *Atmos. Meas. Tech.*, 6, 2925-2940, doi:10.5194/amt-6-2925-2013, 2013.

J. van Geffen and R. van Oss, Wavelength calibration of spectra measured by the Global Ozone Monitoring Experiment by use of a high-resolution reference spectrum, *Appl. Opt.* 42, 2739-2753 (2003).

Lelli, L., Kokhanovsky, A. A., Rozanov, V. V., Vountas, M., Sayer, A. M., and Burrows, J. P.: Seven years of global retrieval of cloud properties using space-borne data of GOME, *Atmos. Meas. Tech.*, 5, 1551-1570, doi:10.5194/amt-5-1551-2012, 2012.

R. C. Levy, G. G. Leptoukh, R. Kahn, V. Zubko, A. Gopalan and L. A. Remer, A Critical Look at Deriving Monthly Aerosol Optical Depth From Satellite Data, in *IEEE Transac-*

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tions on Geoscience and Remote Sensing, vol. 47, no. 8, pp. 2942-2956, Aug. 2009.
doi: 10.1109/TGRS.2009.2013842

Interactive comment on Atmos. Meas. Tech. Discuss., doi:10.5194/amt-2017-128, 2017.

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