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Atmospheric Measurement Techniques Discussions

Interactive comment on "OCRA radiometric cloud fractions for GOME-2 on MetOp-A/B" *by* R. Lutz et al.

Anonymous Referee #2

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ïż£This paper details the improvements of the OCRA cloud processor and shows some results applying it in PMD data from the two GOME-2 instruments in orbit. The paper is recommended for publication in AMT after the following comments have been addressed.

**********General comments**********

The manuscript structure should be reviewed to improve readability. Introduction, methods, results, and discussion sections need to be more distinguishable. E.g., the introduction contains too many technical details (p. 13473, l. 19 through p. 13474 l. 3) that should be moved into a new section 2 summarizing all data sources and its pre-processing (e.g. also sun glint detection). This section should also include the description of AVHRR and PMAp data. Furthermore, the introduction lacks a more de-

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tailed review of existing cloud detection algorithms and their respective technological differences.

The proposed algorithm is intended to be applicable operationally. The improvements of LOS-dependency treatment are supposed to make the algorithm applicable to future TROPOMI/S5P measurements, for which it serves as the prototype algorithm. However, on p. 13483 the paper states "Once the mission lifetime of GOME-2B will be above four to five years, we will create cloud-free composites based on the GOME-2B data themselves to derive the GOME-2B OCRA cloud fractions" meaning that a substitute background map from other sensors needs to be applied during the first 4 to 5 years. The effect of using a background-map of a different sensor should be investigated. E.g., one could use OCRA to derive a background-map from SCIA and apply it on GOME measurements.

Furthermore, the degradation correction requires access to the entire data-set, which is not possible for an operational processor. Each instrument degrades differently, which further complicates the issue of using the background-map compiled from a different sensor. Please discuss this issue.

The abstract proposes a "straightforward transferability" of OCRA for OMI, but OMI features a much smaller bandwidth than GOME-2 which makes the three color approach more problematic. I am missing a discussion on this issue. Also, what is the influence of TROPOMIs much larger scan-angle compared to GOME-2? Please discuss.

The paper proposes an improved approach to tackle with the scan angle dependency. However, the paper does not convince me, whether this goal has been met or not. Figure 17 is by far not illustrative enough for letting the reader judge by his or her own. Furthermore, some of the OCRA results for GOME-2 already appeared in the Verification Report for TROPOMI/S5P revealing that residual scan angle dependency of OCRA cloud fractions exist, e.g. where neighbouring orbits start to overlap. These residual cloud fractions can be substantial (>10%), but appear to depend on season,

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latitude and surface type. Please include a discussion (and figures) of this behaviour into the manuscript to allow for a more comprehensive assessment of the new OCRA version. Again, Figure 17 somehow obfuscates this problem because data from the swath edges is overlaid by data from neighbouring orbits of the two different sensors. Please provide a more detailed quality assessment.

The GOME-2 fact sheet and previous technical publications state that there is spatial aliasing between different PMD channels. Hence, each PMD channel has a different footprint. How does spatial aliasing influence the OCRA results and the comparison with AVHRR/PMAp, which apparently ignores this particular feature of the GOME/GOME-2 instruments?

Section 2.3 describes the correction for the scan angle dependency. This correction is performed on mean reflectances. I am wondering whether this approach is actually sufficient because latitudinal mean reflectances are probably affected by climatological variations. This issue should be discussed at some point. Furthermore, it should be discussed whether mean reflectances are representative for the minimum and maximum values, which are the key parameters in the presented cloud fraction retrieval.

I am missing a statement that the accuracy of trace gas retrievals using OCRA CF as input are actually much depending on the accuracy at small cloud fractions. Furthermore, I suggest to investigate/discuss this issue in particular.

**********Minor comments**********

The first sentence of the conclusions (p. 13493) states that version 3.0 of OCRA has been presented. I think that this is very important information for users/readers and it should therefore at least appear in the abstract and, preferably, also in the title.

Please rename all occurrences of "PMAP" to "PMAp" to comply with EUMETSAT nomenclature.

Add labels (a, b, c etc.) to denote subplots in Figs. 1, 2, 3, 4, 5, 6, 7, 8, 9, 10, 11, 12,

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13, 16, 18, 19, 20, 21, 22, 23, and 25 to improve referencing in the text.

There seems to be something wrong with the gridding in Figures 20, 22, 25b. The western swath edge features much larger pixel sizes than the rest. Furthermore, Figure 25b features significant distortion over Antarctica. Please improve gridding/rendering of these plots using realistic PMD pixel shapes and correct weighting.

Please ovoid adjectives and superlatives in particular. E.g. discard "extremely" at p. 13472 l. 21. When stressing speed too much, the reader may infer a speed-quality trade-off made.

p. 13473, l. 6: What is meant by "basic cloud parameter"? Please rephrase to clarify.

p. 13473, l. 10: Please provide explanation of OCRA abbreviation.

p. 13473, l. 21: "relative high" -> "a"; "the instrument" -> "GOME-2"

p. 13473, bottom paragraph: Please rephrase whole paragraph before moving it to the new section (see comments above). Particularly, I miss a statement that there are 256 PMD measurements within one scan and that one fourth of them are discarded. Furthermore, add reference to (Munro et al., 2015).

p. 13474, l. 11: Add reference to "Sentinel 4 and Sentinel 5 missions".

p. 13474, l. 16-21: Please improve structure. Detailing the subsections here should be avoided.

p. 13474, l. 23: Please also refer to independent pixel approximation (IPA)

p. 13474, l. 24: "The cloud-free background is calculated offline" -> how does this work operationally.

p. 13475, l. 4: "for each of these three colors" -> "for each of these three colors independently"?

p. 13475, l. 7: "all colors contribute with the same amount" Is the RT difference

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between red and blue negligible? If the components are independent, what is the benefit of treating all colors anyway? Wouldn't then one color/channel be sufficient?

p. 13475, l. 26: Please also mention the tandem mode operation of both GOME-2.

p. 13475, l. 27: "occuring" -> occurring

p. 13476, l. 1: "It is particularly important to avoid Solar eclipses for the construction of the cloud-free composites" -> Answer why? Then start new sentence with "Therefore we..."

p. 13476, l. 3: "all orbits" -> Is really the whole orbit affected by the eclipse? I guess that the effect may be more constrained as suggested by Tilstra et al., 2014b.

p. 13476, l. 15: Please change "I_0" to "E_0" to comply with standard nomenclature and to denote that both have different units.

p. 13476, l. 18: Erase indent after "(SZA)." and before "The wavelength".

Table 1: I think, that this table is obsolete because it is already published in AMTD by Munro et al., (2015). I suggest to replace it with a sensitivity vs. wavelength plot, which details the sensitivity of each PMD channel used and denote the respective binning for OCRA RGB values.

Caption Table 1: "setings" -> "settings"

Table 2: OCRA color B is actually UV

p. 13477, l. 13: What are "statistical soft correction factors"? Please be more specific?

p. 13478, l. 1: omit "of the full 1920 km swath"

p. 13478, l. 1: 192 viewing directions are mapped on 110 bins. I presume this introduces aliasing artefacts into measurement statistics because the number of measurements will alternate by a factor of 2. Please comment on this issue.

p. 13478, l. 9: Fig. 3 should not be referenced before Fig. 2. Please reorder figure

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includes.

Figs. 2 and 4: Please adjust colorbars according to the actual value ranges. Please comment on negative degradation in the discussion.

Caption of Fig. 2: "leads to the slight discontinuity at the transition zones" -> please either justify, why this particular degradation model with steps is appropriate here, or investigate the influences of this discontinuity in the text, or chose another degradation model without steps.

Figure 3: Is this degradation model justified? I mean the polynomial is fitted to a function with an alternating behaviour and the 2nd cycle is not even complete and interferences between alternating an polynomial terms must be assumed. Please improve description of degradation fit in corresponding text (p. 13478)

Figure 5: Please use lighter blue to improve readability.

Please add legend to Figs. 5, 6, 7

Figs. 8, 9. Some curves seem to be influenced by sun glint, e.g. s20s30. Would this interfere with the scan angle correction method? Please comment.

p. 13480, l. 4: "mean reflectance" -> Are the mean reflectance curves also representative for the scan angle dependency of the min and max values? Please comment.

p. 13480, l. 4: What is mean by a statistical "soft" correction? Please be more specific?

p. 13480, l. 5: The scan angle correction is apparently performed for each PMD pixel (in forward direction) independently while the degradation correction is performed based on scan angle rather than pixel number. Intuitively, degradation should happen on a per pixel basis while scan angel dependency depends more on the viewing direction. The applied binning scheme is just opposite. Please provide explanation why the binning scheme changes.

p. 13480, l. 15: It is unclear for me, what a "linear spline interpolation" looks like. Is

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this a cubic spline or just linear interpolation? Please clarify.

p. 13481, l. 1: "feature, for all three colors the monthly mean reflectances are larger at the swath edges than at the nadir position or" -> "feature, the monthly mean reflectances are larger at the swath edges than at the nadir position for all three colors or"

p. 13481, l. 5: "seems" -> this is a very weak description, please be more concrete about your observations

p. 13481, l. 6: "flatter" -> "weaker"

p. 13481, l. 24: "depending on the geolocation" -> "depending on geolocation", Furthermore, please specify how it depends on geolocation.

p. 13481, footnote: Footnotes should be avoided in general. Is this important information? If this different grid was tested, then comment on your experience with it in the text. If not, this information may as well be omitted.

Fig 11: The cloud free reflectance over Antarctica for PR in February appears to be below .5 at some latitude band (top right). I don't think this is realistic because it is too low and the spatial signature is also strange. Please comment on this issue in the text.

Fig. 12: What is intention behind including these rg-color-diagrams? Is there an intuitive explanation which may be added to the results/discussion section?

p. 13482, Eqs. 4abc: Please provide small intuitive description of what normalized colors are and/or provide reference.

p. 13483, l. 8: "the cloud-free background" -> "the cromaticity of the cloud-free background" or " the normalized color of the cloud-free background"

p. 13484, l. 9: Please explain \lambda_i

p. 13484, Eq. (6): What is the difference between background \rho_CF and offset

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\beta? Please explain. Furthermore, the cloud fraction is calculated for all three RGB-channels separately and then averaged. What is the improvement of this approach compared to using just one PMD channel/wavelength?

p. 13485, l.6: Please include information, how the 29 test days are selected. Is this data-basis sufficient? Why are not more data used?

p. 13485, l. 22: "Under certain geometrical conditions it may happen that sunlight reflected by the ocean surface directly reaches the satellite sensor, enhancing" -> "Under certain geometrical conditions, sunlight reflected by the ocean surface MAY directly reach the satellite sensor enhancing"

Figure 14: Please provide geolocation of grid cell in caption. Please also discuss following issues in text: - Cloudy pixels appear more red than the white point. What does this imply? I assumed, all cloud pixels are more white that non-cloudy pixels, i.e. are stretched towards the a-priori white point. But this does not seems to be the case here. - Few grey lines point towards negative Pr and positive Pg (those few dots above left of the main bundle). What is the physics behind this behaviour? What makes a pixel less red and more green in the same time?

Caption of Figure 6: Please explain abbreviations for PSG, Stokes12 and PRPB so that figure may be understood without the text. In return, the discussion contained in the caption should be omitted and put into the main text body to avoid clutter.

Figure 17: What is the reason for the data gap south of Iceland? See also general comments on this figure.

p. 13485, l. 24: "More details on this effect may be found in Kay et al. (2009, 2013)" -> I guess there are many more. Please be more generous.

p. 13485, l. 27: "The flagging of measurements over water which may possibly be affected by sun glint is" -> "The flagging of measurements over water, which may possibly be affected by sun glint, is"

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p. 13485, l. 28: "Due to the MetOp-A/B" -> ""Due to the geometry of the MetOp-A/B""

p. 13486, Eq. (9): What is the advantage of this formula compared to the calculation of the reflection angle and a threshold value like less than 36 degree? Please discuss.

p. 13486, l. 14: "Based on Loyola et al. (2011), in" -> "Based on Loyola et al. (2011) and in"

p. 13486, l. 18: "For measurements which" -> "For measurements, which"

p. 13486, l. 20: "for above" -> "above"

p. 13486, l. 27: "PRPB. The first indicator, PSG, helps to seperate cloudy" -> "PRPB, respectively. PSG separates cloudy"

p. 13487, l. 1: delete "help to"

p. 13487, l. 2: "a certain" -> Please be more specific.

p. 13487, l. 7: "a certain" -> Please be more specific.

p. 13487, l.8: "because sun glint would result in a signal well above this threshold" is redundant, please delete

p. 13487, l. 16-18: Suggestion put parameters for both instruments in a separate table to improve readability.

p. 13487, l. 22: "Beierle" -> "Beirle"

p. 13487, l. 28: "and hence a" -> "and, hence, a"

p. 13488, l. 4: The description starting with "The green solid line" should be rewritten. It is not unambiguously clear what is meant, e.g., by "homogenized" and "shift". Please clarify.

Figure 19: The choice of the colorbar is a bit unclear. Right now, most os the dynamic range is provided at quite small appearances (<10ˆ2.4), anything between 10000 and

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100000 uses the same color. I suggest to invert the colorbar and to adjust the dynamic features to high numbers in order to stress their importance.

p. 13488, l. 7: In my opinion, Figure 19 contains much more information, which should be noted here. E.g. that there measurements of approx. 0.8 from GOME-2A while below 0.1 from GOME-2B. Also the scatter around 0.0 in both directions would be worth mentioning as it allows the informed reader an error estimation for particularly small cloud fractions.

p. 13488: Please move description of the AVHRR data to the method section (see general comments)

p. 13488: Please avoid footnotes. Either Mr. Langs contribution is significant, then add him as co-author, or not, then mentioning him in the acknowledgements should be sufficient to credit his contribution.

Figure 20d: corr=R or corr=Rˆ2?

p. 13489, l. 5: "systemetac" -> "systematic"

p. 13489, l. 7: "not" -> "less"

p. 13489, l. 8: "clouds, whereas the IR or thermal infrared radiances from AVHRR are." -> "clouds, compared to NIR or thermal infrared radiances from AVHRR."

p. 13490, l. 1: "additionally to the cloud fraction also provides the cloud optical depth (COD). Further details can be found in the PMAP Factsheet EUMETSAT (2015)." -> "provides the cloud optical depth (COD) in additional to the cloud fraction (EUMETSAT, 2015)."

p. 13490, l. 5: "Both dataset are" -> "Both datasets are"

Figure 24: Does this figure show similar data as Figure 18a? If yes, please explain why are OCRA results for MetOp-A and MetOp-B more different than in January 2013 and homogenize the appearance of both Figures. If not, please clarify the differences.

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p. 13490, l. 11: "scenes, where" -> "scenes where"

p. 13490, l. 20: I think that the term "more or less constant" is not adequate. Either it is constant or not. I certainly believe that an additional plot showing this difference may help the reader improve the discussion.

p. 13490, l. 23: "Larger discrepancies between the two polarization states may appear for instrumental degradation and scan angle dependencies." Is it known that "discrepancies may appear"? What could be the reason for this behaviour? Please specify.

p. 13490, l. 28: "The difference is in the very low percentage region." is a very qualitative statement. Please be more exact or, preferably, add another figure.

p. 13491, Section 4: Is the cloud fraction over snow/ice an important feature for OCRA? Is it implemented in the current version? The formulation "An alternative [..] would be to do a histogram analysis" (p. 13492, l. 5) gives the impression that this section provides some outlook for future improvements rather than already implemented features. Please clarify. If this is a not implemented, I would move this section to a forthcoming paper because it distracts the reader of this paper from the description of the new OCRA version. In following some typos nevertheless...

p. 13491, l. 4: "incorporated and the affected scenes are flagged and given" -> "incorporated, the affected scenes are flagged, given"

p. 13491, l. 18: "this there" -> "this, there"

p. 13491, l. 19: "interpolation is the best tradeoff" -> "interpolation was found to be a reasonable tradeoff"

p. 13492, l. 1: "case it might be worthwile to consider having separate scaling factors for the different surface types (e.g. permanent ice, sea ice, snow, desert, water, land). Surface dependent scaling factors will be included" -> "case separate scaling factors for the different surface types (e.g. permanent ice, sea ice, snow, desert, water, land) are considered to be included"

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p. 13492, l. 21: Please specify "much smaller".

bottom of p. 13492: If this section stays in the manuscript, which is not advised, an illustrating proof-of-concept image would improve the discussion. Still the intention for including this section remains unclear.

Figure 25b: Please include larger image.

p. 13493, l. 1: "In this paper we" -> "We"

p. 13493, l. 5: "scan angle dependencies and" -> "scan angle and"

p. 13493, l. 16: "This is especially relevant for providing products in near real time." As stated above, the presented algorithm seems to work best on data-set of four or more years. This is not feasible for NRT applications. Please comment.

p. 13493, l. 22: "e.g. OMI" OMI features no R-channel. Does the presented algorithm also work on two channels? Please comment.

p. 13493, l. 24: "enough" How much is enough? Please specify.

p. 13493, Acknowledgements: Is this work part of the TROPOMI/S5P project? If yes, it should be included here.

p. 13494, l. 4: "Beierle," -> "Beirle,"

Interactive comment on Atmos. Meas. Tech. Discuss., 8, 13471, 2015.