**Atmos. Meas. Tech. Discuss., 8, C4664–C4667, 2016**
**“OCRA radiometric cloud fractions for GOME-2 on MetOp-A/B” by R. Lutz et al.**

http://www.atmos-meas-tech-discuss.net/amt-2015-363/

This document provides the author's responses to the comments of anonymous referee #2 regarding the manuscript quoted above.

Comments from Anonymous Referee #2:
RC C5185: 'Review of amt-2015-363', Anonymous Referee #2, 26 Jan 2016

Throughout this document, the following scheme is applied to discriminate between referee comments, author's responses and author's changes in the manuscript:

*italic font:* *comments from referees*
normal font: author's response
red font: author's changes in manuscript

**NOTE:** Due to the suggested re-structuring of the manuscript, some Figures now have different numbers in the updated manuscript: The affected Figure numbers (old number → new number) are:
old 20 → new 21,
old 21 → new 22,
old 22 → new 23,
old 23 → new 24,
old 24 → new 25,
old 25 → new 20.
Figure numbers 26, 27 and 28 are new in the updated manuscript.

**NOTE:** In the marked-up pdf manuscript version with tracked changes created with latexdiff in LaTeX, somehow the section label 4.1 is counted twice. This mis-labeling however does only appear for the tracked version with latexdiff but not for the normal version created with LaTeX.

## ***Anonymous referee #2 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 detailed review of existing cloud detection algorithms and their respective technological differences.*

As suggested by the reviewer, the manuscript structure has been reviewed. The content is now structured into the major sections labeled “Introduction”, “Data selection and pre-processing”, “Methods”, “Results”, “Discussion”, and “Conclusions”. In accordance to the reviewer's suggestion, the technical details of p. 13473, l. 19 through p. 13474, l.3 have been moved to the “Data selection and pre-processing” section. The short description of the AVHRR and PMAp data has been kept in its initial place. Also, the Sun glint detection has not been moved to the “Data selection and pre-processing”, but to the new “Methods” section. Finally, a very brief review of the existing cloud detection algorithms and their respective differences has been added to the introduction.

The overall manuscript structure has been improved based on the suggestions given above.
NOTE: Due to the suggested re-structuring, some Figures now have different numbers in the updated manuscript: The affected Figure numbers (old number → new number) are: (20 → 21), (21 → 22), (22 → 23), (23→ 24), (24 → 25), and (25→ 20). Figure numbers 26, 27 and 28 are new in the updated manuscript.

*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.*

It is correct that at the beginning of a mission, a substitute background map from another sensor needs to be used. In order to generate a reliable cloud-free background map, each grid cell needs to accumulate “enough” measurements to guarantee that the given ensemble of measurements contains at least one cloud-free situation. The exact time, when this condition is fulfilled for each single grid cell depends not only on spatial resolution but also on the cloud probabilities for the pixel geolocation. Regarding spatial resolution, for a small 3.5km x 7km grid cell associated to the TROPOMI resolution it will take less time to contain a cloud-free measurement than for a larger 10km x 40km GOME-2 PMD grid cell. Regarding geolocation, for a grid cell in a desert a cloud-free situation may already be reached after two or three measurements whereas it may take 20 (or 30 or more) measurements for a grid cell in the Amazonas region to provide one cloud-free situation. Generally, the smaller the spatial resolution, the less time it will take to generate a reliable cloud-free background map without any residual cloud contamination. For TROPOMI, initial cloud-free maps based on OMI will be used. These will be replaced by TROPOMI based cloud-free maps as soon as the residual cloud contamination can be considered as not significant. Regionally this may be the case within weeks or months, globally we anticipate this to be the case after 1-2 years.

Concerning the last point, we use the GOME-2 based cloud-free reflectance maps for retrieving GOME/ERS-2 OCRA cloud fractions and the results are very similar as using GOME/ERS-2 cloud-free maps.

*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 cloud-free map is computed using degradation corrected reflectances and therefore it doesn't make a difference if the map corresponds to the same or another sensor. It is correct that OCRA as well as other algorithms based on absolute radiances (e.g. ozone profile, aerosol index, etc.) requires degradation correction of the affected radiances, but this can be done in an operational environment. Note that the degradation of an instrument newly put into space is less significant during the first couple years of operation (see Figures 2 and 4 for GOME-2A/B). For OMI, the radiometric degradation is still very small even after several years in orbit (smaller than 0.5% above 310 nm, see Dobber et al. 2008, "Validation of Ozone Monitoring Instrument level 1b data products", J. Geophys. Res., 113, D15S06).

*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.*

In the framework of the S5P L2 project we already showed that OCRA can be successfully applied to OMI using two colors. A paper showing the OMI results is under preparation.

*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, 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 scan angle dependency is significantly reduced using the improved approach. A Figure is added to the conclusions in order to illustrate this improvement. Since the latitudinal part of the corrections is based on latitude bands of ten degrees width, as indicated in Figures 8 and 9, it might happen that relative small dependencies may still remain. Concerning overlapping orbits, it needs to be kept in mind that these have a temporal difference of roughly 90 minutes and that clouds may have changed significantly from one overpass to the next. Nevertheless, the transition in the overlap regions appears to be quite smooth in Figure 28.

Figure 28 has been added to the manuscript to demonstrate the improvement due to the correction of scan angle dependencies.

*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?*

The integration time for a PMD pixel is 23.4375ms and the read-out time for the PDM detector is 11.72ms. The PMD channels are read out in "sequence down", i.e. the reddest PMD (PMD14 if the 15 PMDs are couted 0-based) at 0ms and the bluest PMD0 at 11.72ms. The full 23.4375ms integration time of a PMD pixel corresponds to 10km across track distance. If we assume the geolocation shifts to be linearly in time, then the maximum spatial aliasing between PMD14 and PMD0 will be 11.72ms, or 5km. Taking the mean of the read-out timeshift for PMDs 11-14 (i.e. the OCRA definition for the color red) gives 1.256ms or 0.54km. The same for PMDs 7-10 (OCRA color green) gives 4.604ms or 1.96km and for PMDs 2-6 (OCRA color blue) 8.371ms or 3.57km. Hence, the spatial mismatch in the across track direction between the footprints associated to the OCRA colors R and G may be estimated as 1.42km and between OCRA colors G and B as 1.61km. Since this is only a small fraction of the PMD footprint width (10km in across track direction), we do not consider this as a significant influence on the OCRA results.

*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.*

It is true that latitudinal mean reflectances are affected by climatological variations, which is why the presented corrections of scan angle dependencies are computed for each month of the year. Hence, the corrections are different in different seasons. Applying twelve monthly corrections for the scan angle dependency sufficiently covers all seasonal variations down to semi-annual periodicities.

*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.*

As shown in Van Roozendael et al., 2006*, possible errors on the OCRA CF are compensated in the ROCINN cloud albedo retrieval resulting in a neglectable net effect on the trace gas retrieval.

*Van Roozendael, M. et al. (2006), Ten years of GOME/ERS-2 total ozone data – The new GOME data processor (GDP) version 4: 1. Algorithm description, J. Geophys. Res., 111, D14311.

The above paragraph has been added to the manuscript.

## ***Anonymous referee#2 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.*

This information is already given in the abstract, see p. 13472, l. 12.

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

All occurrences of “PMAP” have been renamed to “PMAp”.

*Add labels (a, b, c etc.) to denote subplots in Figs. 1, 2, 3, 4, 5, 6, 7, 8, 9, 10, 11, 12, 13, 16, 18, 19, 20, 21, 22, 23, and 25 to improve referencing in the text.*

Labels a), b), c), d) etc. have been added to the subplots of the Figures quoted above.

*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.*

In the quoted Figures, the 14-15 orbits per day are just plotted sequently on a world map. This means that in regions where subsequent orbits overlap, the later orbit simply “overplots” the previous orbit. No re-gridding or weighting of overlapping regions has been applied in Figures 20, 22, 25b. The apparently larger pixel size at the western swath edge as well as the distortions over Antarctica is a plotting issue, because the central lat/lon coordinates are used in these figures. The correct PMD pixel shapes with the four corner-coordinates taken from the L1 data are shown e.g. in Figure 27.

Figures 20 and 25b have been replotted in order to avoid pixels at the western swath to appear too large.

*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.*

The quoted superlative has been replaced. Nevertheless, especially because it is part of an operational environment, we believe that it is justified to emphasize that OCRA is indeed very fast. Timeliness will remain to be an important point also for future missions. e.g. Sentinel-4. In our opinion, OCRA's independence of online RT calculations (see also comments in the conclusions) and the resulting fast computational performance is a valuable advantage.

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

A clarification has been added to the manuscript.

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

The OCRA abbreviation is already given on p. 13472, l. 5.

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

This has been corrected in the manuscript.

*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).*

A reference to Munro et al. (2015) is already given on p. 13473, l. 19 (i.e. at the beginning of the paragraph). A statement on the discarded backscans has been added.

A statement on the discarded backscans has been added to the manuscript.

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

References to Sentinel-4 and -5 have been added.

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

Detailing the subsections has been avoided in the manuscript.

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

Two sentences referring to IPA have been added to the manuscript.

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

In the operational processing, the cloud-free background is a static input which has been computed offline in advance.

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

No, not independently, because the distance from the white point in the normalized color diagram depends on all three colors RGB.

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

Only in the case of a cloudy scene, OCRA assumes that all colors contribute with 1/3 in the normalized color diagram. This does not mean that the components are independent. See also reply to previous comment. In the determination of the cloud-free background, the colors are not treated independently and hence, only one color/channel is not sufficient for the OCRA color space approach.

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

Information on tandem mode operation is added to manuscript.

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

Is corrected in manuscript.

*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..."*

A clarification is added to the manuscript.

*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.*

No, not the whole orbit is affected by a Solar eclipse. However, it is likely to cover several scan-lines depending on the ground shadow track of the eclipse. For simplicity reasons however, the whole orbit has been rejected in the generation of the cloud-free background maps. Considering that the cloud-free background maps are based on more than 30000 orbits and that the number of orbits affected by Solar eclipses is of the order 10-20, we do not think that the data loss due to rejection of a full orbit instead of only rejecting the affected scan-lines is significant.

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

To comply with standard nomenclature, I_0 has been changed to E_0 in the manuscript.

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

Indent has been erased.

*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.*

We would prefer to keep this table, because we believe that this is relevant information, which should be available to the reader immediately without having to search for it in the given reference and also because the numbers given in Table 2 refer to Table 1.

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

Typo has been corrected in manuscript.

*Table 2: OCRA color B is actually UV*

This is true. The OCRA nomenclature is however oriented on the RGB terminology indicating that there are three input colors. Hence, instead of saying R/G/UV, we prefer RGB since it sounds more natural or more familiar.

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

The following explanation has been added to the manuscript: “The correction factors are based on statistically representative measurements which are assumed to describe a certain process well enough, e.g. global daily mean reflectances for degradation or monthly zonal mean reflectances for seasonal scan angle dependencies.”

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

This part has been omitted in the manuscript.

*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.*

The number of 110 VZA bins results from choosing a step size of one degree and going from -55 degrees VZA to +55 degrees VZA. For the scan angle dependency, as can be seen in e.g. Figures 8 or 9, choosing 110 VZA bins instead of 192 PMD pixels does not influence the measurement statistics to a noticeable extent. The data appear to be smooth and neighboring VZA bins do not show an alternating behaviour. The same is true for the degradation, e.g. Figures 2 and 4 do not show any alternating patterns along the VZA bin axis.

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

The Figure references have been re-ordered in the manuscript such that Fig. 3 is not referenced before Fig. 2 in the text.

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

We would prefer to keep the same color bar range for each subplot in order to make the direct comparison between the three different colors easier.

A discussion on negative degradation has been added to the section on instrumental degradation.

*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.*

For the VZA bins 0-20 and 89-109, GOME-2A data are only available if the instrument operates in the 1920km swath mode. Hence, for these VZAs, data are only available until the beginning of the tandem operation with GOME-2B in July 2013. For each VZA bin, we chose to use a degradation model based on the longest possible temporal coverage. For VZA bins 0-20 and 89-109 this is Feb 2007 until July 2013 and for VZA bins 21-88 this is Feb 2007 until Sep 2014.

*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)*

As indicated in the caption of Figure 3 and also on p. 13478, l. 15, in the case of GOME-2B, a linear degradation model is assumed, not a polynomial of 3rd degree as in the case for GOME-2A. The linear degradation model for GOME-2B (currently only 21 months) will be replaced by a more appropriate one based on a longer temporal baseline (as of January 2016, the maximum possible baseline would be 36 months).

A clarification concerning the linear degradation model for GOME-2B has been added to the manuscript.

*Figure 5: Please use lighter blue to improve readability. Please add legend to Figs. 5, 6, 7*

In the updated manuscript, a lighter blue has been used in Figure 5 and legends have been added 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.*

We found that a fourth order polynomial as applied in equation (2) is capable of successfully capturing these features addressed above.

*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.*

The cloud-free reflectance maps are derived after applying the corrections for scan angle dependencies and are therefore no longer scan angle dependent.

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

This relates to referee comment "p. 13477, l. 13": The correction factors are based on statistically representative measurements which are assumed to describe a certain process well enough, e.g. global daily mean reflectances for degradation or monthly zonal mean reflectances for seasonal scan angle dependencies.

*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.*

As indicated in p.13480, l. 17-20, the scan angle correction is based on the VZA. This is also why the x-axes in Figures 8 and 9 are in VZA instead of PMD pixel. The instrumental degradation is calculated on a per pixel basis and subsequently mapped to a VZA, see p.13478, l. 1-3. Choosing VZA-based correction factors has the advantage, that in the operational processing it does not matter whether the input orbit mode is nominal (1920km swath) or special (narrow mode, nadir mode,...). The same PMD pixel number has different VZAs in different orbit modes. Using VZA-based correction factors does therefore eliminate this ambiguity.

*p. 13480, l. 15: It is unclear for me, what a "linear spline interpolation" looks like. Is this a cubic spline or just linear interpolation? Please clarify.*

A linear interpolation is meant.

*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"*

In the manuscript, this sentence has been re-ordered as suggested.

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

"Seems to be stronger" has been changed to "It is stronger" in the manuscript.

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

Has been changed in the manuscript.

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

It depends on geolocation in the sense that grid cells closer to the poles have a shorter revisit timescale. In other words: given a certain timebase, a grid cell close to the poles is likely to contain more measurements than a grid cell at the equator.

A specification has been added in the manuscript.

*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.*

This different grid was tested in the context of representation of coastal lines in the cloud-free background map. Since we do not further discuss coastal lines in the manuscript, we will remove this footnote, as suggested by the reviewer.

Footnote has been removed in the manuscript.

*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.*

We agree that the cloud free reflectance over some regions in Antarctica appear to be below 0.5 for PR in February. A study by Casacchia et al. (2002)* however states, that the snow/ice reflectance in the analyzed spectral range from 350-2500nm significantly depends on various characteristics like e.g. grain size, impurities due to soot or dust, dry snow, wet snow, surface roughness, age of the snow, etc. It is further stated that the main reasons for a decreasing reflectance in the visible range are large grain sizes, high water content and soot/dust impurities. Figures 2-7 in Casacchia et al. (2002) show reflectance curves for several Antarctic test sites covering a variety of surface types like drifted snow, snow and ice, shelf ice, pack and lake ice. For some of these cases, the reflectance in the 600-800nm range, which roughly corresponds to the OCRA color PR, can be well below 0.5. Hence we believe that reflectances below 0.5 can be realistic for OCRA color PR.

*Casacchia et al. (2002), Field reflectance of snow/ice covers at Terra Nova Bay, Antarctica, International Journal of Remote Sensing, 23:21, 4653-4667

A paragraph along these lines has been added to the manuscript.

*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?*

The intention behind these rg-color-diagrams is to show a) that the location of the cloud-free situation can be very different for different surface types, e.g. desert with largely enhanced Pr and ocean with largely reduced Pr, and b) that some surface types do not show monthly variations, e.g. desert with all monthly values in one data cluster, while other locations like the Alpes show large monthly variations due to the seasonal changes of fresh snow coverage in autumn/winter, melting in spring and snow-free in summer. These explanations are already summarized on p. 13483, l. 6-14 and we therefore do not see the need to put duplicate information to the results/discussion section.

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

In contrast to the RGB colors, the normalized colors add up to unity, i.e. b+g+r=1

A description has been added to the manuscript.

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

This sentence has been changed as suggested.

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

Lambda_i in the OCRA context is not a single wavelength, but represents the wavelength range for which the OCRA RGB colors are defined.

A clarification has been added to the manuscript.

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

The background rho_CF represents a grid cell's reflectance in the absense of clouds while the offset beta is calculated as the mode of the (rho - rho_CF) histogram based on a global dataset covering all possible scene types and cloud coverages. Regarding the latter comment, as indicated in the author's reply to the reviewer's comment p. 13475, l. 7, the colors are not treated independently in the generation of the rho_CF. The OCRA color space approach cannot be applied to just one PMD channel/wavelength. The generation of the cloud-free maps requires at least two channels.

*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?*

The 29 test days are selected in a way that the whole temporal baseline of the data set is covered. Since, as stated in p. 13485, l. 10-12, we do not see any significant variations, we did not see any need to increase the number of test days.

*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"*

Sentence has been re-phrased as suggested.

*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?*

The theoretical white point at (1/3, 1/3) is defined for ideal conditions. The "measured" white point may deviate slightly, as suggested by Figure 14. See also the replies to the first general and specific comments from reviewer #1. Concerning the few dots above left of the main bundle, we do not understand how to interpret the reviewer's comment "... point towards negative Pr" since the normalized colors cannot be negative. As a general feature, we found that a pixel with less red and more green contribution in combination with a larger blue contribution is indicative for cloud-free situations connected to water/ocean surface conditions (e.g. South Atlantic, see panel f) in Figure 12).

Geolocation of the grid cell has been provided in caption.

*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.*

We assume, the reviewer means Caption of Figure "16" instead of Caption of Figure "6".

In the Caption of Figure 16, the abbreviations have been explained in the caption text and part of the caption text has been moved to the main text body.

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

Since this small data gap south of Iceland also appears in the operational level 2 products, we assume that it may be a level 1 issue.

*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.*

Additional references concerning sun glint have been added to the manuscript.

*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"*

Has been corrected.

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

Has been corrected.

*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.*

After a visual inspection of RGB imagery of sunglint events, we find the applied formula to be more realistic as compared to the "high risk" and "low risk" sunglint flags provided in the L1b data.

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

Has been corrected.

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

Has been corrected.

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

Has been corrected.

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

Has been corrected.

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

Has been deleted.

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

Has been re-phrased in the manuscript.

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

Has been re-phrased in the manuscript.

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

Has been deleted.

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

In order to improve readability, the parameters have been putted as a list.

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

Has been corrected.

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

Has been corrected.

*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.*

A clarification has been added to the manuscript.

*Figure 19: The choice of the colorbar is a bit unclear. Right now, most os the dynamic range is provided at quite small appearances (<10^2.4), anything between 10000 and 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.*

The colorbar has been updated in the manuscript with an emphasis of adjusting the dynamic features to higher numbers.

*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.*

It should also be noted here that a direct PMD pixel to PMD pixel comparison for the full swath between GOME-2A and GOME-2B is not possible since the ground tracks are not the same and the temporal coverage is not the same (48 minutes time offset). The former forces the necessity to re-grid the data on a common grid before comparison and the latter is a non-avoidable error source since clouds may have moved during that time. Both effects together may pose a significant error source. As indicated in the previous reply, the colorbar has been adjusted.

The above statement has been added to the manuscript.

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

We would prefer to keep the description of the AVHRR data in the section labeled “Comparison with AVHRR data”.

*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.*

The footnote has been removed and Mr. Langs contribution is mentioned in the acknowledgements.

*Figure 20d: corr=R or corr=Rˆ2?*

The value given in the plot is corr=R.

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

Has been corrected.

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

Has been corrected.

*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."*

Has been rephrased.

*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)."*

Has been corrected.

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

Has been corrected.

*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.*

Figures 24 and 18a do not show similar data. Figure 18a is based on monthly cloud fraction data and Figure 24 is based on daily cloud fraction data. The latter therefore shows a larger scatter.

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

Has been corrected.

*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.*

We agree that the term "more or less constant" is not adequate. The authors intention was to emphasize that the offset has a constant sign over the whole latitude range, i.e. the zonal mean AVHRR geometric cloud fraction is larger than the zonal mean OCRA radiometric cloud fraction for all latitude bands considered.

A clarification has been added to the manuscript.

*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.*

"May appear" has been changed to "do appear" in the manuscript.

*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.*

A clarification and further explanation has been added in the manuscript.

*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...*

An improved cloud fraction retrieval over snow/ice is not implemented in the OCRA version presented in this paper. Indeed, Section 4 provides some outlook for future improvements. Instead of moving this discussion to a forthcoming paper, we believe it is of interest for the reader at this point since it allows the reader to better judge the current OCRA performance over snow/ice and to better understand why there are shortcomings in the current version and how these shortcomings will be tried to solve in a future version.

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

Has been rephrased as suggested.

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

Has been corrected.

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

Has been rephrased as suggested.

*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"*

Has been rephrased as suggested.

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

The section containing this part has been removed (see next comment).

*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.*

The section with the HSI approach has been removed from the manuscript.

*Figure 25b: Please include larger image.*

A larger image has been included in the manuscript.

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

Has been corrected.

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

Has been corrected.

*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.*

Cloud-free maps based on a data-set of several years from heritage instruments are available and therefore OCRA can be used in NRT applications. Furthermore, OCRA does not need any online RT calculations and is therefore computationally extremely fast. As stated in section 5, the computation of cloud fractions for a typical GOME-2 orbit with 120000 PMD measurements is about 20s, which is well suited for NRT processing.

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

Yes, OCRA has also been successfully tested with OMI and SEVIRI data using only two color information.

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

A detailed answer to this question has been given in the author's response to the second general comment of anonymous referee #2. We repeat the response here:
It is correct that at the beginning of a mission, a substitute background map from another sensor needs to be used. In order to generate a reliable cloud-free background map, each grid cell needs to accumulate “enough” measurements to guarantee that the given ensemble of measurements contains at least one cloud-free situation. The exact time, when this condition is fulfilled for each single grid cell depends not only on spatial resolution but also on the cloud probabilities for the pixel geolocation. Regarding spatial resolution, for a small 3.5km x 7km grid cell associated to the TROPOMI resolution it will take less time to contain a cloud-free measurement than for a larger 10km x 40km GOME-2

PMD grid cell. Regarding geolocation, for a grid cell in a desert a cloud-free situation may already be reached after two or three measurements whereas it may take 20 (or 30 or more) measurements for a grid cell in the Amazonas region to provide one cloud-free situation. Generally, the smaller the spatial resolution, the less time it will take to generate a reliable cloud-free background map without any residual cloud contamination.  For TROPOMI, initial cloud-free maps based on OMI will be used. These will be replaced by TROPOMI based cloud-free maps as soon as the residual cloud contamination can be considered as not significant. Regionally this may be the case within weeks or months, globally we anticipate this to be the case after 1-2 years.

An explanation has been added to the manuscript.

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

A reference to the TROPOMI/S5P project has been added to the acknowledgements.

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

Has been corrected.