Authors' response to the comments of Referee #1 on "MICRU background map and effective cloud fraction algorithms designed for UV/vis satellite instruments with large viewing angles" by Holger Sihler *et al.*

We would like to thank Referee #1 for the review of our submission to AMTD and for contributing helpful comments and suggestions to improve the quality and clarity of our manuscript.

For reference, the original Referee comments below are typeset in black, our responses in blue. Modifications of the original manuscript (green) are indicated in red.

Summary:
Section 1 introduces a basic motivation and explains why cloud fraction retrievals are an important ingredient to trace-gas retrievals. It further provides an overview of existing algorithms and their respective heritage. A particular importance is rightfully directed to surface contributions and to the recent developments in the field to address BRDF effects. Instrument characteristics and the cumbersome relations between MSC and PMD read-outs are well explained (section 2.1). All used auxiliary data are briefly introduced and their treatment (spatio-temporal interpolation) is justified. The main part deals with the determination of the lower threshold T_min (section 2.3). The result for the presented approach of a surface fit is shown for two example geolocations (Australia, Atlantic) at two different wavelengths (382 nm and 516.7 nm), respectively. Finally, the data sets where MICRU is compared against are briefly described (section 2.6). Section 3 shows results of MICRU for three example scenes of GOME-2 (Brazil, North America, Indian Ocean) and compares those to different FRESCO versions and OCRA. The various MICRU versions (MSC, PMD, different wavelengths) are also intercompared to highlight their differences. The comparison to FRESCO and OCRA is also extended to monthly statistics and the temporal evolution is investigated. An interpretation of the comparisons and findings for several selected individual cases and larger statistics is presented in section 4, both for the various MICRU applications and also for the comparison algorithms. Section 5 reminds that the main topic of investigation are small cloud fraction regions and recalls the novelty approach of an empirical BRDF surface model. It concludes a transferability of MICRU to other spectroscopic satellite missions and imager data. It is finally recommended to prefer the UV/blue spectral region over the red spectral region to reduce surface effects. Finally, Appendices A to E provide further valuable information for the reader.

As an overall conclusion, the manuscript is well written and structured, provides a relevant and very interesting contribution to the scientific topic addressed and therefore I recommend its publication after the general comments are addressed.

general comments:

GC1): While a lot of effort is put into the investigation and determination of the lower threshold, the description and assumptions of the upper threshold are quite brief. The authors may consider to expand a bit on the justification of the chosen simplified approach for the upper threshold and for which types of clouds it may be justified and for which not.

This issue is also addressed by GC3 and in the specific comments and technical corrections. Please see our combined responses below.

GC2): Since many cloud retrieval algorithms struggle particularly over very bright surfaces, has the MICRU performance also been tested e.g. over snow/ice conditions?

We thank the Referee for this question. Actually, snow/ice conditions were not tested because these are not discriminated by the algorithm. Hence, snow/ice conditions are potentially biased high and frequently not reliable. Certainly, this issue is worth to be improved in a revised version of the MICRU algorithm. For now, however, we included a flag for snow and ice coverage from auxiliary sensors, respectively (cf. Table 7).

We interpret the Referees comment such that we should emphasise more the capabilities of MICRU over bright surfaces such as deserts. However, we refrained from including another specific study/figure to the paper as it is already quite long. From the figures in the paper we may already follow that

- MICRU performs reliable over bright surfaces, especially at smaller wavelengths due to the smaller surface albedo.
- FRESCO and OCRA both apply data from red spectral region significantly affecting CF retrievals.
- Figure 13 partially includes measurements over East India and North America, which are significantly brighter than the measurements over Brazil. This allows for a relevant comparison between MICRU, FRESCO, and OCRA.
- The averaged biases over deserts may now be estimated from the revised global average maps in Appendix E (see below).

This issue is now included in Section 3.2 of the revised manuscript based on the new Figure 13:

Figure 13 furthermore compares MICRU results to FRESCO and OCRA. Over land, the CF maps of FRESCO L1b and v7 measurements (Figs. 13(j), (k), (m), and (n)) reveal significant positive biases in the western part of the swath. Cloud fractions larger than 20% are detected even though AVHRR and MICRU both detect no clouds. FRESCO v8 displays a significant improvement over Brazil (Fig. 13(p)), whereas CF over North America in Fig. 13(q) are still significantly biased in the west of the swath. Over East India (orbit #17907), however, all FRESCO versions are significantly biased high (Figs. 13(l), (o), and (r)). Switching to OCRA, Fig. 13(s) reveals significantly smaller positive biases of OCRA over Brazil compared to FRESCO L1b and v7. Over North America (Fig. 13(t)), however, a positive bias and scatter are significant. The biases of OCRA_fixed_albedo at MSC resolution are significantly smaller, especially over Brazil (Fig. 13(v)). Similarly, Fig. 13(x) shows significantly smaller biases over East India compared to the native OCRA results at PMD resolution in Fig. 13(u).

The influence of bright surfaces is now also discussed in Section 4.1 of the revised manuscript as specified in our answer to our following answer regarding strong aerosol events.

Furthermore, 3 more MICRU MSC maps (see below) are added to Appendix F of the revised manuscript, allowing for a more detailed comparison between different wavelengths and products, especially over the bright desert surfaces in the centre of the maps.

Figure F1: Six year average of MICRU MSC channel 2 cloud fraction measurements recorded between 1 July 2007 and 30 June 2013. Areas without data are plotted in gray.

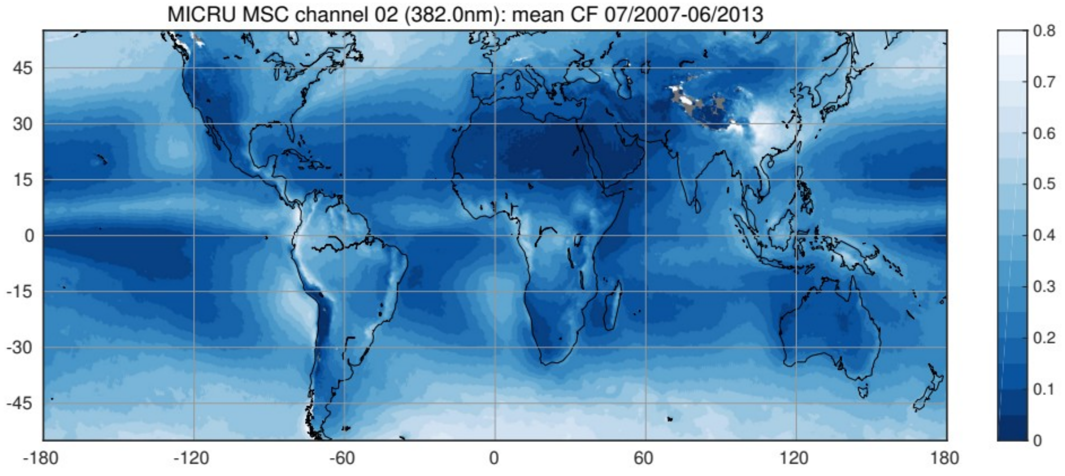


Figure F2: Six year average of MICRU MSC channel 12 cloud fraction measurements recorded between 1 July 2007 and 30 June 2013. Areas without data are plotted in gray.

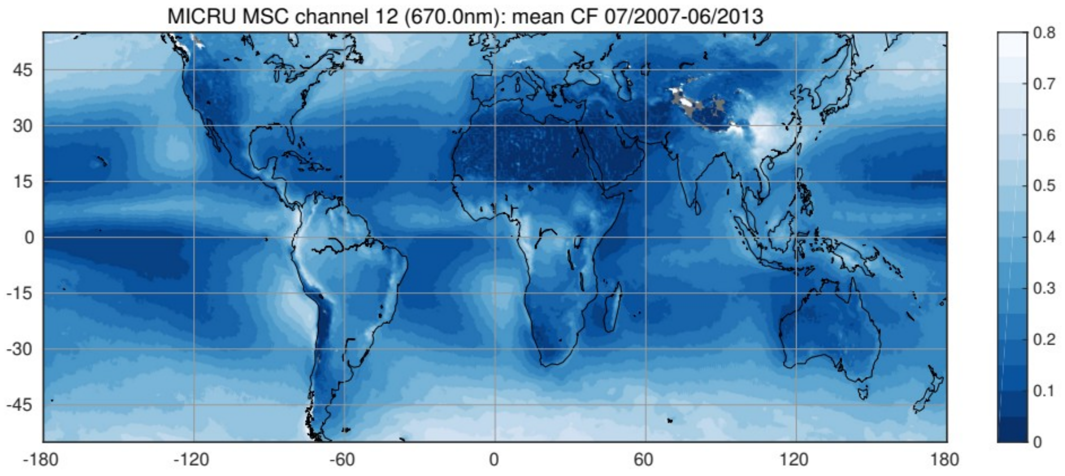
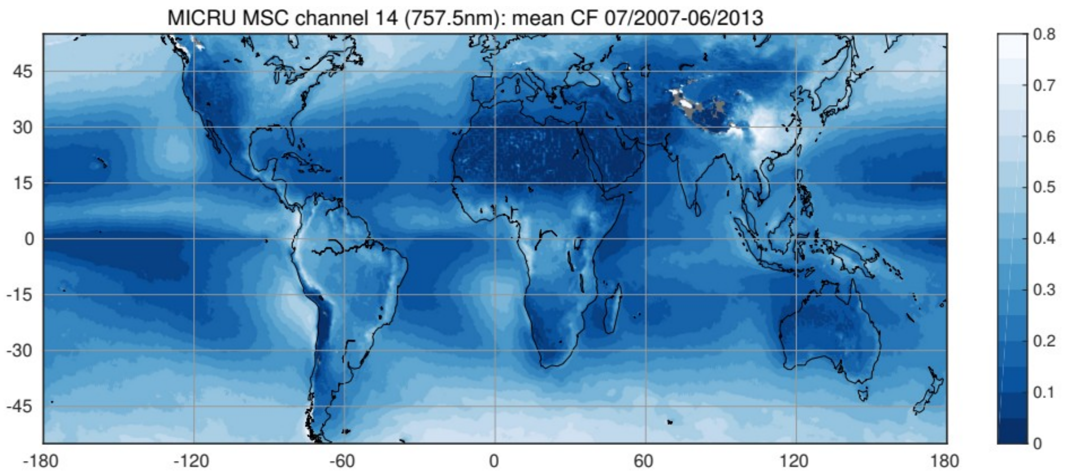


Figure F3: Six year average of MICRU MSC channel 14 cloud fraction measurements recorded between 1 July 2007 and 30 June 2013. Areas without data are plotted in gray.



Also the behavior over different strong aerosol events (desert dust, urban pollution etc.) could be interesting to analyze.

We agree with the Referee and appended the following passage to the fifth section of the discussion of MICRU results in Section 4.1 (p. 38, l.30)

The average CF maps Figs. F1, 14, F2, and F3 of MICRU CF at 382, 440, 670 and 757.5 nm, respectively, reveal significant systematic biases of the bright tropic deserts at 670 and 757.5 nm, notably aver Africa and Australia. At 382 and 440nm, the MICRU results over bright desert areas are significantly smaller than all other results and may, therefore, be assumed more reliable. This assumption is confirmed by Fig. 13, where MICRU CF results over North America and East India, which are significantly brighter compared to Brazil, are not significantly biased. These maps furthermore indicate systematic CF biases from anthropogenic aerosols over East Asia and residual clouds in the tropics of South America and Africa.

Events of desert dust are, however, not considered here.

GC3): The consequences of using a fixed cloud albedo of 0.8 (p2, l30; section 2.4, etc.) should be discussed in more detail in the paper. For example, how should the trace gas retrieval use a MICRU CF>1 (p19, l8)? Are the MICRU CF applicable only to tropospheric trace gas retrievals?

In the following, we would like reply to the Referee's concerns about the upper threshold, which she/he also addressed in GC1 and in the specific comments and technical corrections.

In our view, using a fixed cloud albedo as upper threshold provides several advantages for our algorithm when compared to other options:

- Compared to an empirical upper threshold, or any model with a varying cloud albedo, a fixed cloud albedo represents a more transparent choice, which is independent from instrumental effects, geo-location, observation geometry and cloud properties.
- Compared to a volumetric cloud model, a Lambertian cloud model requires less parameters and is more straight-forward to implement in RT models.
- There is quite number of algorithms for cloud fractions, cloud properties, and trace gases based on the assumption of a cloud with a fixed cloud albedo. Hence, MICRU results are applicable as input to these algorithms right away.
- Additionally, it needs to be noted that the choice of cloud model only has a small effect on the accuracy of small cloud fractions, which is the main domain of MICRU.

In order to make our choice more transparent, we apply the following changes to the manuscript:

Citations to McPeters et al., 1996 and Vasilkov et al., 2017 are added to the introduction (p.2, l.30).

Section 2.4 now reads:
In MICRU, the upper threshold R_{max} is defined as the reflectance of a Lambertian surface with an albedo of 0.8 located at 7 km altitude. This simple cloud model, which was adopted from McPeters et al. (1996) and Koelemeijer et al. (2001), improves applicability to retrievals building on MICRU cloud products because cloud correction algorithms in many trace gas retrievals apply the same model (Vasilkov et al., 2017). Furthermore, the assumptions on cloud RT need to be consistent between cloud and trace gas retrievals for AMF calculation. Volumetric clouds, on the other hand, are more complex to simulate and would require more parameters, which are unknown a-priori. R_{max} is assumed independent of geolocation and time and calculated applying the look-up-tables described in Sect. 2.2 and Table 4. A quantitative discussion of choosing T_{max} = 0.8 as a cloud albedo for an Lambertian cloud model as upper threshold is provided by Koelemeijer et al. (2001),

Ahmad et al. (2004), and Stammes et al. (2008). As a consequence, however, very bright clouds exceeding T_{max} = 0.8 will result in a MICRU CF > 1. Some CF algorithms normalize CF>1 to 1, but MICRU rather provides these exceptionally high values as additional output.
It needs to be noted that instrumental degradation may introduce a systematic bias of the CF, which will be strongest for large CF. Most importantly for MICRU, the influence of the applied cloud model and on the CF accuracy decreases with CF.

GC4): A direct comparison between MICRU CF and FRESCO CF is possible because both algorithms use a fixed cloud albedo of 0.8. OCRA doesn't have this constraint, therefore the comparison between MICRU and OCRA (Section 3.2, 3.4.1., 3.4.3, 4.2, Fig. 11, Fig. 16, Fig. 17, Fig. 21 Appendix D,Appendix D, Fig. D1, Fig. D2, Fig. E5) should be extended by adding an additional 'OCRA CF_fixed_albedo' by converting the OCRA CF to a magnitude similar to the MICRU CF using the following approximation:

OCRA_CF * ROCINN_CA ~ MICRU_CF * 0.8

OCRA_CF_fixed_albedo = OCRA_CF * ROCINN_CA/0.8

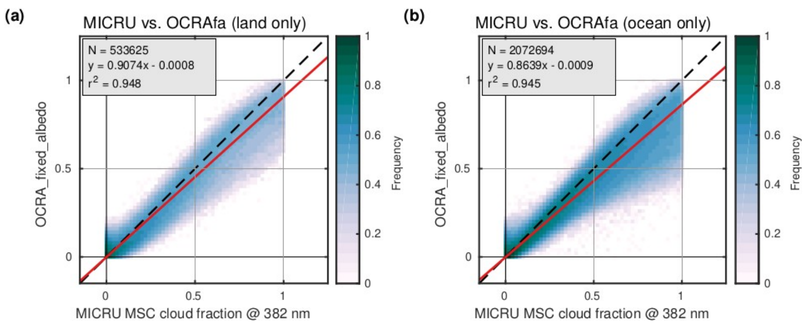
where ROCINN_CA is the cloud albedo retrieved with ROCINN. This adaptation can only be done at the MSC level because the ROCINN parameters are only provided for the MSC footprints and not at PMD level, hence for the OCRA PMD cloud fraction, this modification cannot be applied. However, the manuscript focuses on providing accurate cloud information for the retrieval of trace gases. Since the trace gases are retrieved for the MSC data, I would strongly suggest to add to the comparisons also the OCRA cloud fraction for the MSC data including the adaptation with the ROCINN cloud albedo as outlined above. In summary, I would recommend to

a) add to the comparisons also the "OCRA_CF_fixed_albedo" for the MSC data as outlined above,

We thank the Referee for this comment and added this additional comparison to the paper. We performed our evaluations adding the new term and changed figures accordingly. However, as CF at PMD output is the principal OCRA output, we still focus on the uncorrected OCRA. The changes to the manuscript are only briefly summarized here for the sake of clarity. The latexdiff-document details all modifications to the manuscript.

Changes to the manuscript:

- Description of OCRA input modified and parameter OCRA_fixed_albedo added to Section 2.6 "Comparison data"
- An additional row with OCRA_fixed_albedo is now added to Figure 11 and its caption is respectively updated
- Added OCRA_fixed_albedo figure respective to Fig 16→ new Figure 19



Caption: Comparison between MICRU MSC at 382 nm and OCRA_fixed_albedo based on CF data from April 2010: measurements over (a) land and (b) ocean. This comparison applies cropped data.

- Figures in Appendix D (now E) now also include OCRA_fixed_albedo.
- Added global average map of OCRA_fixed_albedo to Appendix E (now Appendix F)

We appended to the first paragraph of OCRA results (Sect. 3.4.1):

And indeed, the comparisons between MICRU MSC and OCRA_fixed_albedo in Fig. 19 result in more moderate slopes of 0.9 and 0.86 for land and ocean, respectively. Also the linear coefficient of correlation is slightly higher compared to the uncorrected OCRA values in Fig. 18.

and to the second paragraph:

The same but less pronounced behaviour may be observed for OCRA_fixed_albedo in Fig. 19.

Edits to Section 3.4.3, the discussion (Section 4.2) and the Conclusions are complex and compiled in the latexdiff.

The edits according to new Figures 11 and 12 showing VZA dependency are detailed in our answers to Referee #2

and
b) add to the conclusions for the OCRA PMD vs MICRU PMD comparisons a statement that the modification with the cloud albedo as outlined above cannot be done at PMD level and this might be a potential source for discrepancies in the comparison.

The comparison to OCRA and FRESCO is not detailed in our conclusions, yet. We therefore added to the discussion of the comparison between MICRU and OCRA (Section 4.2):

It needs to be noted, however, that comparisons to OCRA_fixed_albedo may only be performed at MSC resolution because the required ROCINN cloud albedo values are not available in PMD resolution. Therefore, all comparisons to OCRA at PMD resolution are affected by a different cloud albedo.

Further specific comments and technical corrections below refer to p(age) and l(ine) of manuscript amt-2020-182.pdf:

p3, l4: There are also new retrieval algorithms that combine the LER models with a geometry-dependent BRDF correction, see for example (Loyola et al., 2020) https://doi.org/10.5194/amt-13-985-2020

The Reference is added to the manuscript. Furthermore, the following subclause is added at the respective location:

, or apply a geometry-dependent LER model (Vasilkov et al., 2018; Loyola et al., 2020)

p3, l32: p5, l1; and p22, l11: The third version of OCRA was not applied to TROPOMI but to OMI. The fourth version of OCRA (Loyola et al., 2018) https://doi.org/10.5194/amt-11-409-2018 is applied to TROPOMI, but this reference is missing in all three paragraphs.

The Reference to (Loyola et al., 2018) is added to the manuscript.

At the first occurrence on page 3
The fourth version of OCRA applied to TROPOMI is described by Loyola et al. (2018).

is now added.

On page 5,
(Lutz et al., 2016)
is changed to
(Loyola et al., 2018)

On page 22,
(Loyola et al., 2018)
is appended to the sentence starting in line 11.

p4, l34: To be more precise, for the TROPOMI/S5-P mission OCRA is part of the S5P L2 CLOUD product and FRESCO is an auxiliary cloud product used for the S5P L2 NO2, ALH, CH4 and O3 profile products.

We agree with the Referee.
The operational cloud fraction algorithms for these missions are OCRA (Lutz et al., 2016) and FRESCO (Wang et al., 2008), respectively.
Is changed to
The operational cloud fraction algorithms for TROPOMI/S5P is OCRA (Loyola et al., 2018) and FRESCO is an auxiliary cloud product used for selected level 2 products (Wang et al., 2008), respectively.
in the revised manuscript.

p6, l23: Could you specify, which data version was acquired from EUMETSAT? Was it the reprocessed AC-SAF data set?

For the spectral data, reprocessed and near-real-time data based on level 0 to 1b processor version 5.3 is applied.

To specify this, we changed
All spectral data is contained in the level 1b (L1b) data provided by EUMETSAT.
to
All spectral data is contained in the level 1b (L1b) data (processor version 5.3) provided by EUMETSAT.
in the revised manuscript.

p10, Figure 4: The last sentence of the figure caption might be confusing and binning #1 might be misunderstood as native resolution. Please rephrase to avoid confusion.

We agree with the Referee that the term “native” may be misleading. The last sentence of the caption is rephrased to (also see comment by Referee #2 on same caption)
The highest resolutions for MSC and PMD lower threshold maps are 0.1°×0.05° and 0.0125°×0.05°, respectively, and denoted binning #1 as in Table 6.

p10, l5: Forward and backward scans were not mentioned before. A short introduction and explanation, why backward scans are discarded could be beneficial for the reader here.

We agree that the backward and forward scans should be introduced. However, we think that this information should not be provided in Section 2.1.2 but rather in Section 2.1.1 “GOME-2 data”.
We replace (p. 6, l. 25)
One nominal swath consists of 24 MSC or 192 PMD pixels, respectively.

by
One swath consists of 32 MSC or 256 PMD pixels, respectively. One swath is divided into a forward and a backward scan. The scanner turns three times faster during the backward scan resulting in three times larger pixels, which are discarded altogether in the following. Hence remaining are 24 MSC or 192 PMD pixels, respectively.

p15, caption of Figure 7: In addition to observation geometry and wind speed, doesn't the sun glitter contribution also depend on geolocation and time? E.g. in summer the sun glitter appears at different latitudes than in winter. Please clarify.

We agree that this formulation may be misleading. We erased observation. The revised caption now reads
the sun glitter contribution depends on geometry and wind speed
which is a more general statement because geometry naturally depends on geolocation, observation geometry and time

p15, l8: Same reference is given twice: ". . .swath edge in Figs. 8(d,e) and 8(d,e)"

The references are updated to
Figs. 8(d,e) and 9(d,e)
in the revised manuscript.

p15, l9-13: Additional tests are described but discarded because of inferior results in a number of case studies. In my opinion it could be elaborated a bit more on the reasoning of selecting these choices in order to give the reader a bit more background information.

We agree with the Referee. In the revised manuscript, the paragraph now reads:

The cosine normalises the parameter improving the fit stability. As tests, we replaced the empirical term either with the precise Li-dense kernels, a reduced cos θ_r term for surface effects, csc θ_s, or $\cos^2 \theta_s$ but all of these test resulted in less accurate surface fits and, hence, increased T_{min} noise compared to our final choice.

p16, table 5: Why is degradation for MSC channels 12, 13, 14 constrained to 0? Please add short explanation.

In the revised manuscript, the table footnote is changed to
$*$: Degradation constrained to 0 for MSC channels 12, 13, and 14 (see text).

In the main body of the manuscript (page 16, line 4), we changed
[…] Clearly, this model is […]
to
[…] The parameters a_t and a_{a1} are constrained to 0 for MSC channels 12, 13, and 14 as preliminary evaluations showed that degradation can be neglected for these channels, which furthermore improved signal-to-noise. Clearly, the above model is […]

p17, Figure 8(b): Is there a mixup, where in the text ". . . measurement set Omega_0 (red) and finally fitted set Omega_I (blue)", the colors red and blue should be swapped? The figure legend says that the fitted data are the red points.

It is true that the colors were swapped. The figure caption is revised to
(b) 3D representation of measurement set Ω_0 (blue) and finally fitted set Ω_I (red)

Figures 8e and 9e: The surface fits for MSC 2 over Australia (8e) and MSC 10 over Atlantic (9e) show very different patterns. The former is strongly pronounced in the western half of the swath and becomes stronger in time while the latter is restricted to the east half of the swath and present for all years 2007-2013. Is there a simple explanation for the driving factor behind these differences (except geolocation and wavelength)? Edit: Ok, the pattern of the latter is later addressed on p24, l30 and assigned to sun glitter but what could be the explanation for the increasing trend in the first example?

We thank the Referee for this particular comment. Both case studies differ not only in geolocation and wavelength, but also surface type and MSC band.

In order to clarify this issue, the first paragraph of Section 4.1 is extended in the revised manuscript: The periodic structures in the west of Fig. 8(e), which are also visible but less pronounced in Fig. 9(e), may be mostly attributed to the anisotropic reflectivity over land. Figure 11 suggests that this anisotropy may be underestimated by the operational CF products. In Fig. 8(e), there seems to be an upward trend and a shift of the apex towards east over time. Both may be attributed to the degradation of this particular MICRU channel (cf. Figs. D1(a) and (d)). Furthermore, also local changes of land use, vegetation type, or precipitation climatology may lead to shifts in the lower threshold, which would be linearised by the applied T_{min}-model. These additional local trends may be reproduced by the MICRU algorithm as Figs. C1(d) and (g) illustrate.

p19, l8: How are MICRU CFs > 1 treated? Are they cropped to 1 or flagged? Please clarify.

In this study, "raw MICRU CFs" with values <0 and >1 are applied unless noted otherwise. The public data contains data normalised to CF∈[0,1]. The files furthermore contain the raw MICRU CFs, which are uncropped. The flags contained in the data are compiled in Table 1 and do not contain an additional flag indicating normalisation to avoid redundancy.

p19, section2.4: The upper threshold is assumed to be fixed with a cloud albedo of 0.8 at an altitude of 7km without a dependency on geolocation and time. While a lot of effort has been put into the various dependencies for the lower threshold, the assumptions on the upper threshold are few. It is argued that MICRU focuses on small CFs, but how valid is e.g. the assumption of cloud albedo = 0.8 in the case of an optically very thin cloud close to the surface or very high in the troposphere? Could it be specified for which type of clouds this simplified assumption on the upper threshold is valid and justified and for which not?

This issue is also addressed by GC1 and GC3. Please see our responses above.

p21, l24: The sentence "probably the most commonly used CF product" is not correct. Most of the operational GOME-2 AC-SAF trace gas products are based on the OCRA/ROCINN algorithm.

We replaced
probably the most commonly used CF product
by
widely-used product
in the revised manuscript.

p22, l14: "and OCRA applies a volumetric (or scattering) cloud model". Technically, the scattering cloud model is only relevant for the ROCINN part of the OCRA/ROCINN algorithm combination, which retrieves cloud top height and cloud optical thickness. For the OCRA cloud fraction using the color space approach, the relevant assumption is a spectral independence of a fully cloudy reflectance across the UV/VIS wavelength range. Therefore, “and OCRA applies a color space approach for the upper threshold (Lutz et al., 2016)” seems more fitting.

We agree with the Referee. The sentence in the revised manuscript now reads:
The selected cloud products define the upper threshold differently. FRESCO applies a Lambertian (or reflecting) cloud model – like MICRU – and OCRA applies a color space approach for the upper threshold (Wang et al., 2008; Lutz et al., 2016).

p23, l1-2: The sentence “These biases propagate into trace gas retrievals if normalized CF data are applied” may apply to FRESCO but is not correct for OCRA/ROCINN. Any possible bias on the OCRA CF will be compensated in the ROCINN cloud albedo and therefore possible bias will be not propagated into trace gas retrievals (Loyola et al., 2007)
http://dx.doi.org/10.1109/TGRS.2007.901043

We thank the Referee for this comment. We revised our citation of Loyola et al. (2007) who state that “ROCINN partly compensates the cloud fraction underestimation from OCRA by overestimating the cloud-top albedo and by underestimating the cloud-top height.” In the present study, however, it is observed that OCRA systematically *over*estimates CFs towards the west. In Figure 11 of the revised manuscript, it may be observed that ROCINN actually compensates some of the overestimation, but certainly not all.

According changes to the manuscript are detailed in our answers to Referee #2 suggesting a detailed study of the VZA dependence, in which we included both OCRA and OCRA_fixed_albedo results.

p31, Figure 16: Title and axis labels say “PMD ch 1” and “PMD-PP cloud fraction @ 382 nm” while the figure caption reads “MICRU MSC at 440 nm”. Please clarify.

The caption now reads MICRU PMD at 382nm

p31, l7: see comment to p22, l14

See our answer above.

p37, Figure 21: Is there a reason that subplot (a) shows all three FRESCO versions L1b , v7 and v8 while in subplot (b) only the FRESCO v8 are shown?

Yes, there is. From Figure 19 it is clear, the older two FRESCO versions are much more affected by the LOS-dependency than FRESCO v8. In order to reduce complexity of Figure 21(b) (Figure 24 in the revised manuscript), of the three different cloud products (OCRA, FRESCO, and MICRU) only the version least affected by the LOS-dependency are compared. Therefore, FRESCO L1b and v7 are omitted.

We appended to the caption of the revised figure (now Fig. 24):
FRESCO L1b and v7 results are omitted in (b) for the sake of clarity.

p37, Figure 21: Are sun glitter scenes included in the 15th percentile cloud fractions shown? The difference for the OCRA East might be due to the fact that sun glitter appears only in the east half of the swath and OCRA sets scenes affected by sun glitter to zero by default. This might contribute to the low bias.

We guess, the Referee refers to the differences between OCRA East and OCRA Nadir in subfigure 21(b) (Figure 24 in the revised manuscript). In that Figure, however, ocean scenes are omitted (denoted: land only) and, therefore we do not believe that the Referees rationale sufficiently explains the observed biases between OCRA viewing angles.

Please note that the revised manuscript applies OCRA_fixed_albedo CFs in Figure 21(b), which reveal a much smaller but still significant gradient between east and west compared to the OCRA PMD values.

p38, l19-20: Please note that the VZA dependence in OCRA is also evaluated empirically with a monthly temporal resolution.

We agree with the Referee. We added to the specified paragraph:

It needs to be noted, however, that also OCRA applies an empirical VZA correction based on global monthly means (Lutz et al., 2016).

p40, l13-15: It is true that the OCRA sun glitter removal at areas with larger theta_r may be positively biased (particularly visible in the left swath of Figure 11(u)). However, it could also be pointed out here that in regions of very strong sun glitter (yellow in Figure 11(c)), OCRA seems to properly account for this effect (visible in the right swath of Figure 11(u)). Furthermore, OCRA includes a sun glint flag.

We agree with the Referee. We added to the revised manuscript

At regions of very strong sun glitter (yellow areas in Fig. 13(c), OCRA seems to properly account for this effect on PMD resolution.

and

and that also OCRA contains a sunglint flag

p45, l6: This is a very interesting detail. Could this terrestrial sun glitter signal over the Amazonas be related to high oriented ice crystals as suggested by Marshak et al. based on EPIC/DSCOVR? (Terrestrial glint seen from deep space: Oriented ice crystals detected from the Lagrangian point https://doi.org/10.1002/2017GL073248)

We thank the Referee for introducing this interesting aspect. However, we do not believe that we found a sign of oriented ice crystals due to two reasons: (a) the signal discussed by Marshak et al. would increase the radiance, and hence, would be filtered by the lower threshold retrieval algorithm, and (b) the spatial structures we observed correspond to the location of water bodies, which we consider spatially only weakly correlated to the location of atmospheric ice platelets due to their significant altitude and the temperature profile in the Amazon Basin.

technical corrections:

p2, l25: change “the time dependent the” to “the time dependency of the”

Done.

p2, l28: change “a-priory” to “a-priori”

Done.

p2, l30: change “albedo of 0.8 Stammes et al. (2008) rendering” to “albedo of 0.8 as in Stammes et al. (2008), rendering”

Done.

p7, l16: change “channels 2,5, 10” to “channels 2, 5, 10” (blank space missing before 5)

Done.

p16, l21: change “a-priory” to “a-priori”

Done.

p20, l1: change “to form a complete parametrisations” to “to form complete parametrisations”

Done.

p20, l17: change “prior the T_min retrieval” to “prior to the T_min retrieval”

Done.

p22, l4: something is missing in “is increased to effects of”. Maybe “is increased to cover effects of”?

The sentence in the revised manuscript now reads
The resolution at the coast and over specific regions is increased to reduce interference from different surface types within one GOME-2 pixel (Wang et al., 2018).

p22, l15: Sentence “Some FRESCO both and OCRA. . .” sounds weird. Is the following meant: “Both FRESCO and OCRA. . .”?

In the revised manuscript, the sentence begins with
Both FRESCO and OCRA [..]

p24, l17: change “in discrete boxed defined” to “in discrete boxes defined”

Done.

p29, l5: change “evaluation” to “evaluations”

Done.

p29, l6: change “and m = 1 519nm” to “and m = 1 at 519nm”

Done.

p31, l6: change “differently” to “different”

Done.

p32, l19: “Figs. 19(d) and (g))”: Shouldn’t this be “Figs. 19(c) and (f)”?

Thanks for pointing out this error. Actually, largest biases are found in the west. Hence, the beginning of the sentence is now changed to

Largest biases are observed towards west for FRESCO L1b and v7 (Figs. 19(a) and (d)), [..]

p37, caption of Figure 21: first line ends with “(b): comparison between selected MICRU”, while it should be “(a): comparison between selected MICRU”.

Done.

p38, l32: change “retried” to “retrieved”

Done.

p40, l5: “(Fig. 21(c))”: Fig. 21 has no subplot (c). Is Fig. 21(b) meant?

Yes, done.

p40, l24: “at coasts an inland”: Is “at coasts and inland” meant?

Yes, done.

p40, l30: “as investigated by Fig. 19).” The closing bracket has no opening bracket.

Done, closing bracket erased.

p48, caption of Figure D2: “circled values in (c)”: The circles are in (a)

Done.

p49, l5: at the end of line change “onky” to “only”

Done.