We thank anonymous referee #3 for reviewing the manuscript and providing corrections and suggestions. Following are our point-by-point replies with the referee comments in italic. Page and line numbers refer to the marked-up revised manuscript, to be provided as a final response.

Specific comments

The paper combines methodologies mainly from Arduini et al. (2005) and Cho et al. (2015) with the results of Mayer et al. (2004).

The main deficiencies of the paper:

1. The authors talk about the cloudbow effect for scattering angles close to 140° (page 2, line 32) and the "collapse" of the phase function at a scattering angle of 132° and cite to this end the results of Cho et al. (2015) about retrieval failures for MODIS. Similarly, they cite this paper for the glory effect that also leads to retrieval failures for MODIS, but they also mention the work by Mayer et al. (2004) that uses the feature of the liquid cloud phase function around the backscatter direction to retrieve optical thickness, effective radius and effective variance of the particle size distribution. I think that in this paper two different effects are mixed together: on one side, the "bumps" in the phase function observed at the bow and glory geometries; on the other side the reduced sensitivity of the observations to effective radius due to the "collapse" of the phase function close to the same observation geometries.
An increased phase function intensity for particular scattering angles has as a consequence "stripes" of higher reflectivity for these scattering angles (as observed by Mayer et al. (2004) in the glory region) that, on their turn, could have as a consequence artificial "stripes" of higher optical thickness when the underlying LUT in the Nakajima-King retrievals does not consider these features (e.g. because of an unsufficient angular sampling of the phase function).
The "collapse" of the phase function instead produces retrieval failures due to the fact that the LUTs are narrower, especially for thin clouds, such that the retrieval is more prone to failure because the retrieval/radiative transfer/calibration uncertainties can "push" the observation out of the LUT more easily (cited more or less literally from Cho et al. (2015)). However, this collapse takes place in this paper (Fig. 5) at 132° and is *the cloudbow effect found in Cho et al. (2015). The authors explain this effect in a nice way, but they assert that "in the cloud bow time slots retrievals are rather normal, with big differences occurring in r_e for smaller scattering angles, namely close to 132°" (page 10, line 15-16). Thus, they mix up these two aspects that shall be separated clearly in the revised version of the manuscript. Correspondingly, it shall also be considered to plot the "collapse" angles instead of the cloudbow angles.*

It is true that the cloud bow and glory, on one hand, and the collapse of the phase functions, on the other, are two different effects, which we did not intend to mix. These effects are examined together in this study because they both cause irregularities in the retrievals. However, we think that mentioning that the effects of "collapse" occur "near" the cloud bow or glory, which we already mention in the first manuscript version (page 3, line 20), helps making their description simpler. In the revised manuscript, we try to clarify this further (page 3, lines 1).

While we have verified that our LUTs adequately consider the increased phase function intensity for these features, at least regarding the angular sampling, we also show that this phase function intensity is highly sensitive to the value of the effective variance (Fig. 6b), which has to be assumed. This sensitivity can explain the corresponding differences reported in cloud optical thickness for time slots around glory conditions, as we show e.g. in Figs. 7 and 8.

The problem caused by the collapse of the phase functions is indeed different, but it occurs *close to* the cloud bow, which is a "bump" in the phase function, as the reviewer describes. Hence, we don't understand why the statement in page 10, lines 15-16 of the initial manuscript is problematic. We think that it clearly separates the cases of cloud bow from those of scattering angles where the "collapse" occurs. Although Cho et al. (2015) describe similar failures as a "cloud bow effect", they clearly show their relation to the scattering angles where the phase functions collapse, and not to the local maximum in the phase function intensity, which defines the cloud bow. Both in the initial and the revised manuscript, we try to be specific regarding this discrimination, and a similar one regarding the cloud glory (see also our next reply), by using regularly the terms "near the cloud bow" and "around the cloud glory". However, starting the analysis with the observed reflectances, (Fig. 4c and e), we opted to plot the cloud bow angles, where reflectances peak locally.

As far as the glory is concerned, I would suggest not to talk about maximum scattering angles and label them as "glory" (page 18, line 6) but to check the behavior of the phase function for angles larger than say 170° and then argument whether a particular effect on the cloud retrieval is expected at all (and in case which effect) or not. In this sense I think that also the information about the 0.6 µm phase function used in the argumentation on page 18, line 5-10 should be shown. Furthermore, the same effect in this scattering angle range (glory) might be expected as for the cloudbow as is explained in Cho et al. (2015). The "collapse" of the phase function mentioned at 132° is the most clear one, but Cho et al. (2015) identify further angles where this reduced sensitivity is also observed: they find 133°, 142° and 177° for their MODIS example. Even if at slightly different angles, this effect is present in this manuscript as well (see Fig. 5c and Fig. 6a) and could be the reason for the irregularities investigated. In particular, the 177° angle is a "glory angle" such that retrieval failures in this scattering angle range might also be traced back to a "phase function collapse".
These aspects shall be addressed in more clarity and the analysis adapted.

It is true that the "maximum scattering angles" do not always coincide with a local maximum in the phase function intensity, which defines glory conditions, and this part was corrected accordingly. Nevertheless, the difference in scattering angles between adjacent 15-min time slots is of the order of 3°, ensuring that glory conditions will occur (perhaps more than once) close to this maximum. This probably explains also the difference in the shape of irregularities around the glory between MSG-1 and MSG-3 (see e.g. Fig. 8). Plotting the maximum scattering angles was rather selected as a common reference to guide the eye to the "center" of the period when glory effects occur. We clarify this point in page 11, lines 10-12 of the revised manuscript.

The referee's suggestion, namely to first check the behavior of the phase function for large angles and then argue whether a particular effect should be expected, is not feasible in our case, since it would imply that we already know the exact shape of the phase function, which depends on the effective variance.

Information about the 0.6 µm phase function is now included in Fig. 6b.

Following the referee's suggestion, in the revised manuscript we repeat the approach followed by Cho et al. (2015) and calculate separation indices for every group of phase functions we examine, in terms of both different r_e values and different v_e values (page 13, lines 20-30). This is a nice way to visualize and quantify the effect of "collapsing" phase functions on the retrieval process and helps in the interpretation of corresponding results. Our results verify the presence of "collapsing" conditions in the abovementioned scattering angles. It should be pointed out, however, that the 177° is not always a "glory angle", since it depends on the wavelength examined.

2. To observe the effects described above I find the approach of using two fixed regions where the results of τ and r_e are averaged (I think at least they are averaged: page 9 line 3-4 is not completely clear in this respect) to be not optimal since it might wash out the effects. In fact, all these "ripples" in the phase function take place in few degrees such that the approach of using regions with 5°×5° or 4°×4° size may complicate the identification and explanation of the related retrieval effects. In this sense, do for instance all pixels at 9 UTC in Fig. 4 have maximum scattering angle? I think that it should be discussed how strong the scattering angle can vary inside a SEVIRI pixel and inside the regions investigated in order to assess whether the expected effects can be identified or to what extent they are weakened by the averaging procedure. Furthermore, for a clearer illustration of the results I think that an additional picture showing one area (i.e. a 2D plot in latitude and longitude) in the cloudbow and/or glory slot would help interpreting the results (similarly to Fig. 10 in Cho et al. (2015)).

The results in Fig. 4 are averaged, as are the reflectances. We clarify this point in page 10, lines 11-13 of the revised manuscript. The range of scattering angle values for each satellite and time slot was also quantified. Typical ranges for the southern Atlantic region are 0.91°±0.11° and 1.01°±0.11° for MSG-1 and MSG-3, respectively. It was also verified that the maximum scattering angle occurs in the same time slot for every pixel in both MSG-1 and MSG-3. While these results ensure no complication due to the size of the areas selected or the 15-min frequency of observations, we also examined averaging in smaller areas, based on the reviewer's concerns. We found that using 2°×2° areas we could still identify the effects under study, without increasing significantly the variability between time slots, which could compromise our results. However, the area covered with liquid clouds was significantly increased (Fig. 3 of the revised manuscript), and typical variations of scattering angle values within a time slot dropped to about 0.4°, providing a better correspondence between averaged retrievals and phase function characteristics. This is highlighted in page 11, lines 11-13 and page 12, lines 1-2 of the revised manuscript. Hence, we decided to repeat the analysis based on this reduced (2°×2°) area size.

However, the study areas are much smaller compared to the area shown in Fig. 10 of Cho et al. (2015) (~20°×20°), and uniformly covered with liquid clouds. Hence, adding a picture showing one of the areas would not provide any additional help in the interpretation of the results.

3. Fig. 6b shows that there are angles of reduced sensitivity to the effective variance while most of the phase function shows a clear dependence on v_e in this angle range. Can you see this "collapse" of the phase function w.r.t. v_e in Fig. 7? Does this dependency of the phase function on v_e extend to other scattering angles as well? Looking at Fig. 2 it would be interesting to shortly explain where an effect due to the dependence of the phase function on v_e can be expected.

A collapse of the phase function with respect to r_e, shown e.g. in Figs. 5c and 6a, leads to an irregularity in the r_e retrieval (e.g. Figs. 8c and 8d near the cloud bow angle), independently of the v_e value, which is selected *a priori*. This happens because phase functions collapse close to the cloud bow angular region no matter the v_e value selected. Correspondingly, we should expect a reduced sensitivity in an attempt to retrieve v_e in angles where phase functions of Fig. 6b collapse. Hence, the effect of the collapses in Fig. 6b cannot be seen in the results of Fig. 7. The irregularities in the r_e retrieval around the maximum scattering angles (Figs. 7b, 8c and 8d), occurring especially for larger v_e values, are rather associated with increased "collapsing" of the phase functions in the backscattering region for larger v_e values, which leads to higher separation index values and hence more points falling outside the LUT.

4. It has never been clearly stated in the manuscript whether the "flagged pixels" (e.g. page 14, line 14) contribute to the plots (e.g. Fig. 4). However, page 10 line 24-25 suggests that the τ - r_e results for the

flagged pixels are used for the statistics (i.e. the diurnal cycles). From my point of view, these flagged pixels correspond to the failures investigated in Cho et al. (2015) and thus should not be used.
Similarly, do you show in Fig. 4c,4e the mean reflectance of the box or the mean cloud reflectance? This uncertainty arises from the phrasing "used as input to the CPP algorithm" (page 9, line 2) which seems to imply that you mean the cloudy pixels alone, since no retrieval is run for cloudfree pixels, I suppose. And what about the scattering angles/optical thickness/effective radius?

"Flagged pixels", i.e. those falling outside the LUT, are indeed used in the diurnal plots. The purpose of providing flags in a data record is of course to inform the user on possible failures and reliability issues, and in that sense we agree with the reviewer that they should be excluded from a study using these data. In the present study, however, our purpose is to highlight the irregularities caused by these failures, and investigate their origin and ways to reduce their effects. Furthermore, exclusion of these pixels would cause gaps in the diurnal variation, or at least not directly comparable adjacent time slots, since flagged pixels cover large parts of the study areas in the specific time slots studied (Fig. 9).

The mean reflectance in Figs. 4c and 4e are mean liquid cloud reflectance, to be directly comparable with the CPP retrievals, which are also averaged over liquid clouds only. Scattering angles are averaged over the entire area. The almost complete coverage of the study areas with liquid clouds ensures that possible discrepancies are minimized. This is especially true for the revised results (see also revised Fig. 3). All these points are clarified in the revised manuscript (page 10, lines 11-13 and page 10, lines 15-16).

5. In Fig. 4, the cloud glory regions (red and black) show different behaviours: the red one (MSG-1) shows a strong irregularity made up of two strong local minima and one local maximum in the optical thickness plot (d), while the black one (MSG-3) shows a weak local mimimum alone. Please explain why there is this difference.

This difference should be attributed to the different scattering angles from the two satellites. The small differences, combined with the high sensitivity of the scattering phase functions in the backscattering directions, led to these different behaviors.

6. I am missing an overall discussion about the plausibility of the retrieved diurnal cycles. This would increase also the plausibility of the investigations shown in the entire paper. For instance:

- *Can one expect that marine Sc has an almost flat re diurnal cycle (Fig. 4) while the optical thickness is decreasing strongly, a hint that the thermodynamic conditions the clouds are developing in are changing during the course of the day?*

  It is true that the flat r_e diurnal cycle, combined with the strongly decreasing τ, is not the average behavior of the marine Sc, where a decrease in r_e would typically be expected (see e.g. Seethala et al., 2018). However, this is an one-day case, hence not necessarily representative of the average. We discuss this point in page 10, lines 14-15 of the revised manuscript.

- *The diurnal cycles in Fig. 4 and Fig. 10 differ: the 3.9 µm retrieval produces a lower τ and a lower r_e, although in an adiabatic environment one would expect higher r_e at the cloud top, where the 3.9 µm retrieval is more sensitive. If you think that "subpixels fractions of open water" (page 17 line 9-10) are the reason for this, you might take a look at the HRV channel, if it is available over these regions, for a first check about this hypothesis. What is the uncertainty of the 3.9 µm retrieval, which should be higher than the one for the 1.6 µm?*

The study area is very homogeneously covered with clouds (and even more so is the adjusted 2°×2° area), and therefore the presence of subpixel fractions of open water is highly unlikely. We believe that potential imperfections in the treatment of the 3.9 µm channel (e.g., calibration, atmospheric absorption) are the most likely explanation for the differences between the 1.6 µm and 3.9 µm retrievals.

The calculated uncertainty in the 3.9 µm retrievals is overall somewhat lower than in the 1.6 µm retrieval. The probable explanation for this is that presently no uncertainty due to atmospheric absorption and the thermal contribution in the 3.9 µm retrievals is included in the calculations.

- *MSG-1 and MSG-3 in Fig. 10 provide different diurnal cycles of r_e. While MSG-1 seems to observe a decrease in re at around 9 UTC, MSG-3 yields an increase and a decrease afterwards.*

The decrease in r_e retrieved from MSG-1 at around 9 UTC, also apparent based on the new results, is due to the glory conditions affecting the retrieval. Examining non-affected time slots, it can be seen that MSG-1 and MSG-3 retrievals are quite similar, with increasing r_e before 9 UTC and slightly decreasing after 10 UTC.

- *In Fig. 12 MSG-1 and MSG-3 also provide very different diurnal cycles: not only the absolute values but also the variations in time are different, both in τ as well as in r_e. How can this be explained? Such strong differences preclude of course the use of simultaneous observations of the two satellites, both for physical/meteorological investigations of cloud properties and for the purpose of the present paper.*

It is true that there are many differences between MSG-1 and MSG-3 retrievals over the continental region, based also on the smaller study area of the revised manuscript. Although we are not certain on the reasons behind these differences, they are probably related to specific cloud types and 3D effects, since no similar issues were found in the marine case. These differences were not investigated further, since they don't actually preclude the separate use of the two sensors for the purpose of the present study: the simultaneous usage of the two satellites, which was based on the assumption that retrievals are the same, was dropped earlier in the study for practical reasons (see page 15, lines 9-10). The differences between MSG-1 and MSG-3 should indeed be of high concern if the previous assumption was required, and we highlight this point on page 15, lines 10-11 of the revised manuscript. In Section 3.4, however, they are only analyzed in parallel, to highlight the same effects based on different illumination and viewing conditions. However, since this point is important, we also included a small discussion acknowledging this issue (page 22, lines 12-16).

7. The continental case is said to be "not directly comparable with the marine Sc case" (page 18, line 5). If this case is shown, and I think the paper benefits from this since it shows a cloud with higher optical thickness and much higher effective radius, it should be done in more detail: see my comment above about the phase function and the explanation of the diurnal cycles. Further aspects that are not completely clear for this case are the fact that τ shows a dependency on v_e while r_e shows none in the glory geometry, and the short temporal displacement between the small local minima in τ for the cloudbow and the local maxima in r_e (Fig. 12).

Section 3.4 was expanded in the revised manuscript based on the aspects raised by the reviewer, as well as on the additional analyses of uncertainties and separation indices. Our arguments regarding differences between τ and r_e under glory conditions were based primarily on the characteristics of the 0.6 μm and 1.6 μm phase functions.

8. Retrieval results still seem to show a relatively small variability w.r.t. v_e. Is this sensitivity to v_e comparable to the retrieval uncertainties or retrieval errors or is it larger?

While it is not clear to which retrieval results the reviewer refers to, it is indeed crucial for the validity of our conclusions to compare the variability w.r.t v_e with retrieval uncertainties. For this reason we used the methodology described in Stengel et al. (2017) to propagate the level 2 retrieval uncertainties to the spatially averaged values used in this study. We describe the process used in page 8, lines 8-14 and page 9, lines 1-2. In the Results section of the revised manuscript, it is also explicitly mentioned if the v_e variability is smaller or larger than the propagated uncertainty.

9. The manuscript demonstrates that particular geometries like the cloudbow and the glory can lead to biased optical properties, but what would you propose in order to reduce this bias, keeping in mind that in 15 minutes (from one slot to the next) also cloud physics can vary (the cloud can thin out, become thicker, its particle size distribution can change...)?

Based on our results, we propose that using more appropriate values of effective variance can help reducing the biases associated with the cloud glory geometry (page 26, lines 13-14). While cloud properties indeed vary within 15 minutes, this variability is not apparent in our spatially averaged analysis. On a pixel basis, this variability is expected to be more pronounced, but correcting the size distribution as we propose would still remove the additional irregularities.

10. The paper should shortly discuss/mention at one place the reasons why the same retrieval from two satellites at the different locations could yield different results, apart because of glory and cloudbow. Here I think of shadow effects, partially cloudy pixels, cloud inhomogeneities, 3D radiation effects, surface BRDF, mixed phase clouds, misidentification of thin cirrus on top... This is the basis for the synergistic use of the two MSG spacecraft. Parts of this discussion are e.g. at page 8 line 8 and page 10 line 11.

This is indeed an important part of the discussion that we included in page 4, lines 11-18.

Further comments:

***Title**: Since the paper presents results for two selected days over two selected regions I recommend to add "A case study" somewhere to the title.*

While the paper indeed examines two selected days, we would hesitate to characterize it as a "case study" for two main reasons: (a) any further averaging of data, either in space or in time, would render the cloud bow and glory phenomena impossible to study, since their effects are restricted in both space and time. Hence, this study cannot be conducted on a "bulk-data" basis. (b) The notion of "case study" usually refers to a phenomenon of particular interest, which manifests as a specific event (e.g. extreme weather event). However, the days studied here were chosen based only on specific criteria that were met. The phenomena studied occur around the globe on a daily basis.

Abstract: *Please mention that you analysed two days of data.*

Done.

Figures:

• *I suggest to merge Fig. 3a with Fig. 4 and Fig. 3b with Fig. 12.*

We opted to keep the figures separated, because we wanted to highlight from the beginning that both study regions met (and were selected based on) the high coverage with liquid clouds criterion. Furthermore, Fig. 4 would become too "busy" with such a merging.

• *Please use the same colors for MSG-1 and MSG-3 in all figures.*

The reviewer probably refers to Fig. 3, where colors were swapped. This is now corrected.

• *Please add the solar zenith angle to Fig. 4 in order to understand when θ_0 is reaching 90°, i.e. sunrise and sunset. This might explain for instance the increasing reflectance at 0.6 μm in Fig. 4c and 4e.*

The solar zenith angle in these plots is always less than 84.3°. In fact, this is the threshold for CPP retrievals used in CLAAS-2 and applied throughout this study. This is mentioned in Table 1 but is further clarified in the revision (page 9, lines 20-21). Hence, sunrise and sunset cannot explain the increasing reflectances in Figs. 4c and 4e, and adding the solar zenith angle to Fig. 4 would not be useful. As explained in a later comment (page 10 of this document), the increasing reflectances are probably due to the high solar zenith angle combined with the higher viewing angle from MSG-1 compared to MSG-3. This difference in reflectance, however, is not repeated in the CPP output, showing that these conditions are accounted for in the retrieval.

• *I suggest to move Fig. 5c to Fig. 6.*

Figure 5c was placed next to the LUT diagrams of Figs. 5a and 5b to highlight the effect of the "collapse" of the phase functions at 132° on the corresponding LUT depicted in Fig. 5a. The phase functions of Fig. 6 focus on the backscattering directions and the relevant discussion refers to cloud glory. For this reason we think that it is better to keep Fig. 5c and Fig. 6 separated.

• *For all figures with glory and cloudbow: it would be helpful for the reader to write directly into the plot which vertical line is glory and which one is cloudbow.*

We have added letters next to each vertical line, denoting either cloud bow (b) or glory (g).

• *For all figures with diurnal cycles: it would be easier for the reader if each panel contained MSG-1 or MSG-3 somewhere to distinguish the satellites at a glance.*

Done.

• *Since only hours are used in the diurnal cycle plots I think that e.g. "05" or "5" would be better than "0500".*

Done.

• Units should be expresses either as e.g. "Θ / degree" or "Θ [degree]" but not "Θ (degree)". Furthermore "Reflectance (0.6 μm)" should read "Reflectance at 0.6 μm" with no unit. Instead of "Hour (UTC)" I suggest "UTC hour".

Done.

• Fig. 5 is too small. Furthermore, it is probably not a "scatter plot" (page 10, line 25-26) but I guess a 2D histogram. In that case the colors should be explained as well.

Indeed, the term "density plot" explains better what Figs. 5a and 5b show (page 13, lines 3). The colors are also explained in the revised figure caption.

• Please add a (dotted) line at height 0 (r_e or τ difference = 0) in Fig. 7.

Done.

Page 4, line 21*: I cannot believe that the MSG-1 satellite is moving so fast and so much (10° latitude in 24 h) around its subsatellite point. Please check this in more detail! This could have an important impact of the observation geometry.*

Sub-satellite point coordinates are available in the original MSG SEVIRI files metadata, while EUMETSAT has also warned users on this issue (see e.g. www.eumetsat.int/website/home/News/ConferencesandEvents/DAT_3647214.html). This deviation, however, should not be seen as an independent movement of the satellite around the nominal sub-satellite point, but rather as the satellite lying in an orbital plane inclined by about 5° compared to the equatorial plane. While the impact on the observation geometry can indeed be important, inclusion of this information in our retrievals actually ensures avoidance of possible misinterpretations.

Page 6, line 21*: Why do you need three values of the surface albedo?*

The explanation for this is somewhat technical. The cloud reflectance can be written as the sum of the reflectance for a dark underlying surface and a term containing cloud transmittance, the hemispherical sky albedo for upwelling isotropic radiation, and the actual surface albedo. From radiative transfer simulations of the cloud reflectance at two particular surface albedos (chosen as 0.5 and 1 for numerical stability), the transmittance (function of zenith angle) and sky albedo can be determined. These are also stored in the look-up table and then allow the direct calculation of cloud reflectance for any value of the surface albedo. This procedure is described in the CM SAF CLAAS-2 ATBD, and a reference to this ATBD has been added in the manuscript (CM SAF, 2016a; page 7, lines 12).

Page 6, line 24-27*: Please give a reference (or a short explanation) for the gas absorption correction and the thermal emission consideration.*

The gas absorption correction method is explained in the CM SAF CLAAS-2 ATBD. The thermal emission calculation is not covered by the CLAAS-2 ATBD because CLAAS-2 does not involve the 3.9 μm channel. However, the CPP algorithm has also been applied to AVHRR for the production of the CLARA-

A2 data record. In that context, the AVHRR 3.7 μm channel is used, and the consideration of thermal emission is covered in the corresponding ATBD, which has been added to the main text (page 7, line 10) and the reference list:

CM SAF: Algorithm Theoretical Basis Document, CM SAF Cloud, Albedo, Radiation data record, AVHRR-based, Edition 2 (CLARA-A2), Cloud Physical Products, EUMETSAT Satellite Application Facility on Climate Monitoring, SAF/CM/SMHI/ATBD/CPP_AHVRR issue 2.0, 19/08/2016, doi: 10.5676/EUM_SAF_CM/CLARA_AVHRR/V002, 2016b.

Page 7, line 3-4*: The size distributions do not depend on wavelength (line 3), but the phase functions do (line 4), so please shift "for the visible wavelength (0.6 µm)" after "phase functions". Please correct also the caption of Fig. 2.*

Corrected.

Page 7, line 4-5*: Please indicate in the text and/or in the figure where the cloudbow and glory features can be observed.*

Cloud bow and glory features manifest as peaks near 140° and in the backscattering direction, respectively. This is added in the revised manuscript (page 7, lines 20-21)

Page 7, Table 1*: Is there such a set of LUTs for every value of the surface albedo mentioned in the text? Please explain this.*

No, as explained earlier, radiative transfer calculations for three values of the surface albedo are used to derive cloud transmittance and hemispherical sky albedo, which are stored in the LUT, and allow the calculation of the cloud reflectance for any albedo of the underlying surface.

Page 8, line 4*: Please indicate in the text here and not only in the caption of Fig. 1 the details of the region coordinates.*

Added (page 9, lines 6-7).

Page 8, line 5*: Please mention which quantity has been used to assess "uniformity" of the cloud deck.*

By "uniformity" we mean high degree of spatial coverage. We have rephrased accordingly (page 9, line 7).

Page 8, line 8-10*: This argument, related to the different viewing conditions, also depends on the cloud field observed. If the Sc has dimensions that are anyway smaller than the spatial resolution of SEVIRI, only small differences might appear here.*

This is indeed the case in the updated results. Differences are larger in the continental case and only minor in the marine region (Fig. 3).

Page 8, line 10*: What does it mean that MSG-1 detected "more ice clouds"? Are there ice clouds during these days? Do they contribute to the cloud cover shown in Fig. 3? Are there ice clouds that MSG-3 does not detect and contribute to the retrieval results (Fig. 4 onward)? Are ice clouds maybe one of the* *reasons for the differences in cloud cover from MSG-1 and MSG-3? Which further factors might explain these differences?*

Examination of the retrieved cloud phase data showed that over the southeastern Atlantic between 12:30 and 17:00 UTC MSG-1 retrieved ice clouds covering 3-4% of the 5° × 5° study region, reaching 17% in 16:30 and 16:45 UTC. For the same time slots ice cloud cover from MSG-3 never exceeded 0.5%. Over the 4° × 4° continental region both satellites detected ice clouds covering between 10% and 20% from 14:00 UTC to 16:00 UTC and about 5% in other time slots. While these findings can explain results shown in Fig. 3, ice clouds did not contribute to later retrieval results, which were constrained to pixels where both satellites retrieved liquid clouds. Furthermore, in the revised manuscript, where both study regions are decreased to 2° × 2°, ice cloud coverage over the southeastern Atlantic for the same time slots from MSG-1 never exceeds 4%, while no ice cloud is detected from MSG-3. Over the 2° × 2° continental region, ice clouds never exceed 2% in either satellite retrieval. Hence, this statement was removed from the revised manuscript.

***Page 8, line 14**: At this point of the manuscript it is not clear yet when the cloudbow and glory geometry occur, so please explain this in the text. Nevertheless, if you merge this figure with Fig. 4 or 12, as suggested above, this remark is superfluous.*

Our purpose here is not to highlight the cloud bow and glory time slots, but rather to mention that they are also included in the time range with high liquid cloud cover. This part was rephrased accordingly (page 9, lines 18).

***Page 8, line 21**: In the MSG-1 curve in Fig. 3a there are discontinuities in the afternoon while the MSG-3 cloud cover is very smooth. Might they be caused by sunglint? Which possible effects might explain these differences otherwise?*

Figure 3 was updated based on the revised analysis and the reduced size of the study regions (2°×2°). Discontinuities in the MSG-1 curve are now much less pronounced. Sunglint conditions are included in CPP as a flag, and it was indeed found that these conditions were fulfilled for some pixels and time slots in late afternoon over the southeast Atlantic region with MSG-1. However, their effect on CPP retrievals would be significant only if clear-sky pixels were misinterpreted as cloudy. The good agreement between MSG-1 and MSG-3 cloud cover ensures that this is not the case here.

***Page 10, line 6-7**: "with values increasing rapidly ..." ! Please explain.*

This increase in reflectances from MSG-1 in late afternoon, which is not present in MSG-3, should probably be attributed to a combination of the sun positioned low above the horizon and the viewing angle of MSG-1, which is larger than that of MSG-3. It should be noted however that, based on the CPP output, where no similar difference is found, these conditions are accounted for in the retrieval LUTs. This is clarified in page 12, lines 7-9 of the revised manuscript.

***Page 10, line 10**: "different illumination conditions" ! please explain.*

Here we mean illumination *and viewing* conditions. We have rephrased accordingly (page 12, line 12).

***Page 10, line 27**: Please mention the observation conditions that are shown here.*

The observation conditions (θ,θ_0 and Θ) are given in the revised Fig. 5 (for MSG-1: $\theta = 43.4°$, $\theta_0 = 55.7°$, $\Theta = 86.1°$; for MSG-3: $\theta = 22.7°$, $\theta_0 = 55.7°$, $\Theta = 134.1°$).

Page 11, line 1-3*: This is an interesting point and also not obvious since the reflectance observed by the satellite is affected by single and by multiple scattering at the same time. Thus it is not trivial to find a signature of the single scattering properties in this quantity. Please consider mentioning this aspect in the text.*

We agree with the reviewer and we have included this aspect in the text. We have also added that this LUT characteristic affects optically thin clouds only, where single scattering prevails (page 13, lines 7-11).

Page 11, line 9*: Please quantify "thin".*

Based on the LUT in Fig. 5a, $\tau < 4$ would be a rough quantification (included in page 13, line 18).

Page 11, line 15*: Is this assertion from Mayer et al. (2004) who used reflectances at 753nm also valid for other wavelengths? I think you can cite your plots as well to explain this.*

This part was rephrased to reflect the referee's suggestion (page 14, lines 1-2).

Page 12, line 9-11*: Could you please explain what you mean with this sentence, in particular with "their differences"?*

In the case of τ, where differences due to different size distributions appear only in cloud glory conditions (Fig. 7a), the sensitivity to v_e is clear. In the case of r_e, however, we have differences in both glory but also large irregularities in cloud bow conditions, and the glory from one satellite almost coincides with the cloud bow from the other. Hence, it is difficult to discern the sensitivity to v_e. This part is rephrased in the revised manuscript (page 15, lines 8-10).

Page 12, line 11*: What is meant with "This is due..."?*

We mean the difficulty to discern the sensitivity to v_e. This sentence has been rephrased (see also our reply to the previous comment).

Page 12, line 12-13*: If you "give up" your synergistic approach of using MSG-1 and MSG-3 you might consider showing results from only one satellite. This would make the next figures "lighter" and you can eventually mention that these results are confirmed (not shown) by the other satellite.*

While the reviewer's suggestion would indeed make the presentation of results "lighter", we opted to continue the analysis with both satellites. The main reason is that including a second satellite adds cases with different scattering angles, which can contribute to the analysis.

Page 13, Fig. 7*: Why is the effect of the glory smaller for MSG-3?*

This difference should be attributed to the different maximum scattering angles: 176.4° for MSG-1 and 172.6° for MSG-3. An inspection of Fig. 6a, and especially the corresponding separation index values (Fig. S3 of the supplement) shows that indeed MSG-1 would be more prone to failures, and thus irregularities, than MSG-3. This is explained in the revised manuscript (page 17, lines 12-15).

Page 14, line 7*: Please specify "significant effect" and put it in relation to the uncertainty of the* r_e *retrieval.*

The term "significant" here was not meant in a statistical sense, and was replaced by "strong" for more clarity. The retrieval uncertainties are analyzed in the revised manuscript as described in page 8, lines 8-14, and page 9, lines 1-2, and they are discussed in relation to our results throughout the manuscript.

Page 14, line 8*: Please explain what you mean by "The effect on the glory is similar to the τ case".*

We mean that the effect of v_e on r_e in the glory is similar to the effect of v_e on τ in the glory. We have rephrased accordingly (page 17, lines 11-12).

Page 14, line 14*: "which are flagged" as bad quality? As uncertain?*

As pixels where the pair of VIS and SWIR reflectances lies outside the Nakajima-King LUT. We have added this clarification (page 18, line 4).

Page 15, line 10-11*: "... these distributions cannot capture the cloud glory adequately." Do you mean that such distributions do not show the glory effect or that Mie theory is not adequate for such distributions? The size parameter for 1 µm particles (small cloud droplet) at 1.6 µm is still 3.9 and even higher at 0.6 µm. Are you really sure that Mie theory is not suitable?*

We mean that in such distributions the cloud glory effect is much weaker, as can also be seen in Figs. 6b and 6c for phase functions at 0.6 µm and 1.6 µm, respectively. We have rephrased accordingly (page 18, line 19).

Page 15, line 11*: "...adequately." Please add a reference.*

This part has been rephrased (see previous comment).

Page 16, Fig. 10*: Why is* v_e *indiced variability spread over such a large time period, especially for MSG-1 (6-11 UTC)?*

This spread should be attributed to the different characteristics of the 3.9 µm phase function in the backscattering directions. Specifically, glory features are wider compared to smaller wavelengths (see e.g. Figs. 11a and 6b), covering a larger range of scattering angles, hence the variability will be spread over a larger time period. This is added in the relevant description of Fig. 10 (page 20, lines 19-21).

Page 16, line 13*: Is the assertion about the 3.9 µm phase function separation for different* v_e *still valid if you rescale the plot (Fig. 11) as in Fig. 6? In principle, you should/could introduce a sort of phase separation index as in Cho et al. (2015) to quantitatively answer this question.*

Following the reviewer's suggestions, we introduced the Cho et al. (2015) phase separation index in our analysis. Index values at 172° scattering angle are indeed smaller in the 3.9 μm phase function compared to the 1.6 μm phase function (Figs. S5 and S7 of the supplement). This is also mentioned in the revision (page 20, lines 15-18).

Page 17, Fig. 14*: Which "part of the results" is expected? Why for "an optically moderately thick" cloud? Please explain.*

This sentence has been rephrased for clarity (page 21, lines 14-15).

Page 18, line 5*: "wider size distributions are expected": please give a reference.*

This expectation is based on the results of Miles et al. (2000). Added in page 22, line 4.

Page 18, line 5-9*: The glory issue in the continental case should be investigated in the same detail as the marine one. It is not clear which features characterise the phase function at these higher angles (177-178°) that cannot be explained like for the 172° scattering angle in the marine case. By the way, a scattering angle plot for the continental case should be presented.*

This part has been expanded. The relevant phase function is now shown (Fig. 6b) along with the respective separation index (Fig. S3). Scattering angles for the continental case are also shown in revised Fig. 12.

Page 20, Table 2*: Please add which cloud types (Sc, Cu...) have been investigated by Miles et al. (2000).*

Added in page 25, lines 22-24.

Page 20, line 11*: What is meant by "In marine only clouds"?*

We meant "marine Sc clouds". We have rephrased accordingly (page 25, lines 25).

Page 20, line 22*: Please explain/rephrase "further emphasized".*

Here we mean the fact that the conclusions drawn from the present study are similar to those from previous studies. We have rephrased this part for more clarity (page 26, lines 11-13).

Technical corrections

Abstract*: The verbs should be in the present tense, e.g. "are analysed" instead of "were analysed".*
Done.

Page 1, line 2*: "... (LWP), which is a crucial component..." ! marine low clouds are a crucial component, not LWP. Please rephrase.*
Rephrased.

Page 1, line 9*: detection ! observation.*
Changed.

Page 1, line 15*: "different underlying surfaces" ! please write land and ocean.*
Done.

***Page 1, line 13**: "Cloud_cci"!please write ESA's Climate Change Initiative (essential climate variables related to clouds) or something like this.*

Done.

***Page 1, line 15-16**: "more recent and advanced sensors provide high spatial and temporal resolution" ! "... spatial and/or temporal resolution"*

Changed.

***Page 1, line 20**: I think that the CALIPSO/CALIOP lidar and CloudSat/CPR should be shortly mentioned here.*

We did not mention CALIPSO/CALIOP and CloudSat/CPR since the focus is on passive imagers only.

***Page 1, line 21**: "routinely retrieved from passive VIS-IR" ! "routinely retrieved from e.g. passive VIS-IR" since also pure thermal algorithms exist, especially for cirrus clouds (e.g., Heidinger and Pavolonis, 2009; Holz et al., 2016; Minnis et al., 2016; Strandgren et al., 2017).*

We focus on the VIS-IR methods here. It is not relevant that cloud optical and microphysical properties are also retrieved from other wavelengths.

***Page 1, line 30**: "... biases reported..." ! please cite already here the papers you mention below.*

Done.

***Page 2, line 7**: "... not retrieved" ! at this place the sentence "While under most retrieval circumstances..." on line 15 would fit particularly well.*

The sentence was moved.

***Page 4, line 12**: The height above the equator could be omitted.*

Done.

***Page 4, line 19**: "diurnal basis" ! "hourly basis".*

Changed.

***Page 5, line 6**: "12 spectral channels between" ! "12 spectral channels in"*

Changed.

***Page 5, line 7**: Please mention also the HRV channel.*

Done.

***Page 5, line 7**: Please introduce the CCP algorithm in Sect. 2.2 and not here.*

This part was moved to the beginning of Sect. 2.2.

***Page 5, line 9**: "near wavelength" ! "centred at wavelength".*

We really mean 'near'. The central wavelength is not 0.6 μm but 0.635 μm. For clarity we mention the exact central wavelengths and the approximate values used further on in the paper in the revised manuscript (page 6, line 11).

***Page 5, line 13-14**: Please mention the operational calibration slopes to have an idea about the differences in calibration.*

We introduced the following phrase: 'These values can be compared with the corresponding operational calibration slopes of 0.0241, 0.0233, 0.0209, and 0.0236 mW m^{-2} sr^{-1} $(cm^{-1})^{-1}$, respectively.' (page 6, lines 6-7).

***Page 6, line 20**: "contained within" ! "filtered with"*

Changed.

***Page 7, line 1**: "approach" ! "selection".*

Changed.

***Page 7, line 5**: Please rephrase "along with differences..." as "but the details of the phase functions for these scattering situations depend on the effective variance".*

Rephrased.

***Page 7, line 7**: 180 ! 180°.*

Corrected.

***Page 7, Table 1**: The rows in the third column are not aligned with the rows in the second column, please correct.*

Corrected.

***Page 7, Fig. 2b**: Please add "scattering angle" to the x axis title.*

Added.

***Page 8, line 3**: "equinox" ! "vernal equinox".*

Added.

***Page 8, line 7**: "spatial coverage" ! "cloud cover".*

Replaced.

***Page 8, line 9**: "more clouds" ! "higher cloud cover".*

Replaced.

***Page 8, line 9**: "over the continental region and less over the marine" ! "over the continental and less over the marine region w.r.t. MSG-1".*

Rephrased.

***Page 8, line 11-12**: "their high spatial coverage with liquid clouds" ! "the high liquid cloud cover".*

Rephrased.

***Page 9, Fig. 4a**: Please add "scattering angle" to the y axis title.*

Added.

***Page 10, line 13**: "were based" ! "are based".*

Changed.

***Page 10, line 24**: "showed" ! "shows".*

Changed.

***Page 10, line 27**: "same" ! "corresponding" or "appropriate".*

Changed.

***Page 10, line 27**: "the LUT now covers" ! "the LUT for MSG-1 covers".*

Rephrased.

***Page 11, line 15**: "the phase function" ! "the phase function at 1.6 µm".*

Added.

***Page 11, line 16**: "the distance" ! "the angular distance".*

Added.

***Page 11, line 18**: "range" ! "intensity".*

Replaced.

***Page 11, Fig. 6**: Please plot larger ticks on the y axes.*

Done.

***Page 15, line 6**: "increase" ! "increases".*

Corrected.

***Page 18, line 3**: "will affect" ! "affects".*

Changed.

***Page 19, line 9**: "decreased flagged pixels" ! "decreased numbers of flagged pixels".*

Changed.

***Mayer et al. (2015)**: Should read Mayer et al. (2004).*

Corrected.

***Wood and Hartmann (2005)**: Should read Wood and Hartmann (2006).*

Corrected.

References

Seethala, C., Meirink, J. F., Horváth, Á., Bennartz, R., and Roebeling, R.: Evaluating the diurnal cycle of South Atlantic stratocumulus clouds as observed by MSG SEVIRI, Atmos. Chem. Phys., 18, 13283-13304, doi: 10.5194/acp-18-13283-2018, 2018.

Stengel, M., Stapelberg, S., Sus, O., Schlundt, C., Poulsen, C., Thomas, G., Christensen, M., Carbajal Henken, C., Preusker, R., Fischer, J., Devasthale, A., Willén, U., Karlsson, K.-G., McGarragh, G. R., Proud, S., Povey, A. C., Grainger, R. G., Meirink, J. F., Feofilov, A., Bennartz, R., Bojanowski, J. S., and Hollmann, R.: Cloud property datasets retrieved from AVHRR, MODIS, AATSR and MERIS in the framework of the Cloud_cci project, Earth Syst. Sci. Data, 9, 881-904, doi: 10.5194/essd-9-881-2017, 2017.