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Interactive comment on "Sensitivity of liquid cloud optical thickness and effective radius retrievals to cloud bow and glory conditions using two SEVIRI imagers" *by* Nikos Benas et al.

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General comments

The authors describe two diurnal cycles of liquid cloud properties, optical thickness τ and effective radius r_e, as derived from the CPP algorithm using data of two MSG satellites, Met-8 and Met-10, that observe the same area at the same time from two different positions. The paper addresses the question whether particular scattering geometries, cloudbow and glory, affect the observed cloud properties. Through the peculiarities of the phase functions for these angles the derived properties might be biased w.r.t. other "normal" scattering geometries and lead to spurious results, i.e. in the evaluation of diurnal cycles. To investigate this question the authors employ two SEVIRI instruments

that operate from two different positions and apply a Nakajima-King method to two pairs of satellite channels (0.6-1.6 μm and 0.6-3.9 μm). Furthermore, they assess the effect of the effective variance ν_e of the particle size distribution of the results and hint at a possibility to improve the determination of optical thickness and effective radius.
The paper is well structured and addresses an interesting topic that fits to AMT. However, I find that many uncertainties remain and some discrepancies in the results should be addressed and explained in a more direct way. Therefore, the manuscript should be published in AMT after major revisions.

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.

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 behaviour 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.

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)).

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 ν_e in this angle range. Can you see this "collapse" of the phase function w.r.t. ν_e in Fig. 7? Does this dependency of the phase function on ν_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 ν_e can be expected.

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?

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.

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

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 ν_e while r_e shows none in the glory geometry, and the short temporal dispacement between the small local minima in τ for the cloudbow and the local maxima in r_e (Fig. 12).

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

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...)?

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.

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.

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

Figures:

- I suggest to merge Fig. 3a with Fig. 4 and Fig. 3b with Fig. 12.
- Please use the same colors for MSG-1 and MSG-3 in all figures.
- 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.
- I suggest to move Fig. 5c to Fig. 6.

- 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.
- 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.
- Since only hours are used in the diurnal cycle plots I think that e.g. “05” or “5” would be better than “0500”.
- 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”.
- 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.
- Please add a (dotted) line at height 0 (r_e or τ difference = 0) in Fig. 7.

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.

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

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

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.

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

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.

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.

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

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.

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?

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.

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?

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

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

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

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.

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

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

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

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

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.

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

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

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

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

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?

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

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

Page 16, line 13: Is the assertion about the 3.9 μm phase function separation for different ν_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.

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

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

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 continetal case should be presented.

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

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

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

Technical corrections

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

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

Page 1, line 9: detection → observation.

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

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

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

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

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).

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

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

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

Page 4, line 19: “diurnal basis” → “hourly basis”.

Page 5, line 6: “12 spectral channels between” → “12 spectral channels in”

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

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

Page 5, line 9: “near wavelength” → “centred at wavelength”.

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

Page 6, line 20: “contained within” → “filtered with”

Page 7, line 1: “approach” → “selection”.

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”.

Page 7, line 7: 180 → 180°.

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

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

Page 8, line 3: “equinox” → “vernal equinox”.

Page 8, line 7: “spatial coverage” → “cloud cover”.

Page 8, line 9: “more clouds” → “higher cloud cover”.

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”.

Page 8, line 11-12: “their high spatial coverage with liquid clouds” → “the high liquid cloud cover”.

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

Page 10, line 13: “were based” → “are based”.

Page 10, line 24: “showed” → “shows”.

Page 10, line 27: “same” → “corresponding” or “appropriate”.

Page 10, line 27: “the LUT now covers” → “the LUT for MSG-1 covers”.

Page 11, line 15: “the phase function” → “the phase function at 1.6 μm”.

Page 11, line 16: “the distance” → “the angular distance”.

Page 11, line 18: “range” → “intensity”.

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

Page 15, line 6: “increase” → “increases”.

Page 18, line 3: “will affect” → “affects”.

Page 19, line 9: “decreased flagged pixels” → “decreased numbers of flagged pixels”.

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

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

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