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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Quantifying retrieval uncertainties in the CM-SAF cloud physical property algorithm with simulated SEVIRI observations" *by* B. J. Jonkheid et al.

Anonymous Referee #1

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General comments

The authors present a validation of the CM-SAF CPP algorithm for the retrieval of Cloud Water Path CWP from various passive sensors. In this paper, the focus is on the MSG/SEVIRI radiometer for which a simulator is also presented for the time efficient computation of synthetic satellite scenes in two spectral channels (0.6 and 1.6 μm). Since the authors intend to use the CPP algorithm for climate model validation against satellite data, an exemplary application to RACMO is shown as well.

The paper takles the important issue of weather/climate model validation using satellite data for the CM-SAF and should be published in ACP. Nevertheless, it must first go

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through major revisions.

Specific comments

The main deficiencies of the paper consist in

1. the description and evaluation of the accuracy of the SEVIRI simulator
2. the description of the results and the related figures.

1. SEVIRI Simulator (Section 3)

Section 3 starts with the motivation for the development of a satellite simulator (p. 4317, l. 26 – p. 4318, l. 7). Even if common, I find the use of the word “simulator” in this paragraph particularly confusing because the authors’ list contains simulators that are absolutely different from the simulator developed for this paper. It would be more valuable to first assert that “simulator” can mean different things depending on the context (p. 4318, l. 1–2) and only afterwards examples should be mentioned. Here, the COSP activities illustrated in Bodas-Salcedo et al. (2011) should be added. The libRadtran package instead is not a simulator but a radiative transfer model suite like 6S (Vermote et al., 1997), SBDART (Ricchiazzi and Gautier, 1998), or STREAMER (Key and Schweiger, 1998). So libRadtran, and its description (p. 4318, l. 26 – p. 4319, l. 4) should not appear here. In case, you can refer to the simulator of Bugliaro et al. (2011) that is based on libRadtran, or list further ones.

Section 3 then states that “a simulator can only be used to test algorithms that use the same assumptions”. I do not agree with this sentence. This would mean that ECSIM for instance has been financed by ESA and made open source just to feed the particular group of people that is using exactly the same assumptions, i.e. basicly the same group of people that has developed the simulator. A simulator should have the

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ambition to produce realistic simulations of the given instrument that can be used by anyone. Since the assumptions made are known, they can and must be considered when working with the simulated data. Please reconsider your assertion.

On p. 4319, l. 11–12, one of the main merits of the SEVIRI simulator is presented. Please describe explicitly all the merits but also all the limitations at this place or at another more appropriate location in the manuscript.

The CPP algorithm determines cloud phase as the same time as it derives its cloud optical and microphysical properties. To this end, a thermal window channel is needed, typically the SEVIRI channel centred at 10.8 μm. Is this channel output by your simulator? If not, why? I thought it is mandatory for CPP.

One of the main points regarding the simulator concerns the application of the Räisänen et al. (2004) method for the stochastic generation of subgrid columns for climate models. To my opinion, this is one of the key features of the simulator and deserves/needs more details. Main open questions are:

1. Which cloud overlap do you allow for? Climate models use different cloud overlap schemes and radiative transfer calculations should be performed using the same cloud overlap scheme as the climate model that produced the cloud input.
2. When you apply Räisänen et al. (2004) you produce a given number of realisations of a model box using a given number of subscale columns. How do you determine how many realisations you need and how many subgrid columns do you produce?
3. Inside every subgrid column you get rid of the vertical cloud variability and reduce the number of clouds to two at most, one liquid and one ice water cloud with fixed heights and in particular with two effective radii. Please indicate both lower and upper bounds of the two layers. At this point you should discuss the

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effect of these assumptions on the simulated radiances. What is the quantitative difference with respect to a calculation that takes care of the entire vertical cloud profile? Various aspects should be considered: a) One aspect concerns r_{eff}. What is the impact on the resulting 1.63 μm radiance when the entire r_{eff} profile is used? b) Are there consequences in view of the envisaged application, i.e. in view of the CWP retrieval? While COT only slightly depends on r_{eff} and on vertical water distribution, CWP depends strongly on r_{eff}. However, r_{eff} is usually only representative of the upper part of the cloud. Thus, when the upper part of the cloud shields its lower part, it is difficult to estimate the CWP of the lower part of the cloud. By reducing the entire profile to two layers I think that you make it easier to retrieve CWP for the entire cloud because your r_{eff} is representative of the entire cloud. Please discuss these aspects. c) Does multiple scattering between the two cloud layers play a role? Is there a variability related to the relative position of the two cloud layers? How large is it? d) In the presence of mixed phase clouds (i.e. both liquid and ice water in the same volume) you separate the two phases. What is the effect of this separation on the computed radiances?

Some details regarding the radiative transfer computations should be added: How many streams do you use for disort? How do you consider gas absorption? You probably have fixed gas profiles, don't you? Which ones? Do you correct for varying ozone and water vapour content when you apply the simulator to different atmospheres than the basic one? Do you consider aerosols in your atmospheric model? Do you allow for your simulator to use different surface albedoes in the two solar channels? What does the sentence "If only a single thermodynamic phase is present in a given column, the calculations are performed for a single layer." mean? Isn't this obvious? How do you consider the spectral response functions of the satellite instruments? Do you take care of instrumental noise?
You write that you are using a slightly different grid than in the CPP LUTs to avoid biased results. How can you be sure that you avoid biases? Every time you make an

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interpolation you introduce a (small) uncertainty in the resulting radiance and thus in the derived optical properties. Can you quantify this issue? Moreover, I think that you should add a table showing the sample points for all quantities, both for the simulator and CPP.
Instead of interpolating w.r.t. surface albedo you use the Chandrasekhar formula. To this end, you need α_{hemi}. Is it part of the LUT? Please clarify.

Further remarks concerning Section 3:

P. 4317, l. 27: Please insert appropriate citations for the simulators here and remove them in the following when the simulators are described.

P. 4318, l. 6: "ice crystal phase functions" should be replaced by the more general expression "cloud models" since additional factors other than the scattering phase function of ice clouds can affect the results.

P. 4318, l. 18: The use of BRDF in this context is not appropriate. First, BRDF is mainly used in conjunction with surface properties: the surface BRDF. Second, ECSIM computes TOA reflectances for a particular scene and associated solar and viewing geometry but not a distribution of reflectances. Please correct.

P. 4319, l. 24 and in the references: "Räisänen et al. (2006)" –> "Räisänen et al. (2004)".

P. 4320, l. 16: "four degrees of freedom: COT" –> "four degrees of freedom: COT_{total}".

2. Results and Figures

The main results are illustrated using Figs. 4–9.

1. All plots in all figures should be described separately and in detail. A sentence like "The CWP retrieval errors are illustrated in Figs. 5 and 6." is too general. Single features should be identified clearly in the corresponding graphics.

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2. I do not understand the difference between an RMS errror and the standar deviation in relative units. Please give the explicit formulas or a clear explanation of their meaning and how they are computed. Furthermore, you also distinguish between accuracy and precision of the retrieval. What do you mean by these two concepts in this context? I only know them from measurement theory where accurracy has something to do with the difference between the measurement and the real value, while precision refers to the degree of repeatability of the measurement.

3. Do you really always need all three quantities (mean, rms, std deviation)? Please reconsider your plots under this point of view. If yes, it would still be nice to reduce the amount of "line plots" (i.e. the amount of rows in Fig. 4, 6 and 8).

4. Colour tables are inappropriate: the blue colour for instance means something between 0.4 and 0.8. Please use colour tables that enable a clearer distinction between different values.

Further general comments regarding results and figures:

1. Results must be described in a quantitative way. Please avoid for instance in the entire Section 4 expressions like "for low optical thickness" and write instead "for τ<6" or "for τ<1" or whatever is appropriate.

2. In Section 4.1.3 additional explanations should be provided by the authors. In case of a partly cloudy climate model box, Räisänen et al. (2004) produce cloudy and clear subcolumns for which radiances are computed. Eventually, these radiances are averaged to one value that is fed into the CPP retrieval. So, I do not understand the motivation: "By construction this complication does not occur" is wrong to my opinion since you do have partly cloudy model boxes.

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The retrieval applied to a partially cloudy box/pixel should provide a sort of averaged COT, averaged r_{eff} and averaged CWP. Which values of COT do you consider when you discuss the accuracy (precision?) of the retrieval? Is your reference COT the mean COT of the pixel or the mean cloud optical thickness?

3. Section 4.2 shows a real application to a climate model which is the main reason for writing this paper. It would be interesting to see some more details of the comparison CWP in vs. CWP out (histograms?) and it would be very nice to see the comparison between CWP in vs. CWP out vs. CWP from MSG/SEVIRI (i.e. all stages from I to IV). Please add such a comparison.

4. Section 4.3 first nicely describes the probable reasons for the differences between CPP retrieved and predicted values of CWP found in Bugliaro et al. (2011). Since you also show that small optical thickness clouds are source of uncertainty for the retrieval, I do not agree with your conclusion "This stands in stark contrast with the findings of the current paper, where generally higher COT values are considered and the retrievals for liquid water clouds show only very small uncertainties.". I would state that your study covers in a systematic way all COT values and shows that small COTs ($\tau < 5$) are a challenge but that larger COTs are captured in a satisfactory way by the retrieval. Please rephrase. This conerns your conclusion as well, where the Bugliaro et al. (2011) study is mentioned.
The choice of the droplet size distribution is mentioned twice on p. 4327 as a possible source of uncertainty for satellite based retrievals but according to the literature (Rolland et al., 2000) this quantity is of very small concern especially for water clouds (uncertainties in optical thickness smaller than 3%). By the way, this is the reason why effective radii are used. It is only r_{eff} and τ that matter for remote sensing of (water) clouds at least when particle size is much larger than wavelength (geometric limit). Details of the particle size distribution are usually not accessible, apart from particular situations like the glory (Mayer et al., 2004). Please rephrase.

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Specific comments regarding results and figures:

P. 4321, l. 12–15: I do not agree with this sentence that the retrieval should perform perfectly for single-phase clouds. For small optical thickness for instance ($\tau < 2$) it is always very hard to distinguish between different effective radii, and this physical limitation is a point you hint at in the manuscript at some point. Please rephrase.

P. 4321, l. 23: "As a first test the influence on the solar zenith angle on the CWP retrieval" –> "As a first test the influence of the solar zenith angle θ_0 on the CWP retrieval".

P. 4321, l. 24: You mention COT values of 6 and 100 while the plots show values of 6.4 and 102. Please correct.

P. 4322, l. 3 and in the following: I am guessing whether $\theta < 72°$ should read $\theta \leq 72°$. Please check.

P. 4322, l. 3: Please indicate explicitly the set-up angles for the simulation. This is helpful in order to understand which parts of the parameter range are stressed in this test.

P. 4322, l. 13: Another possible reason could be the different cloud height. Please comment.

P. 4322, l. 13: Since different parameter grids play a role you should add the tabled parameter values of the CPP LUTs in Sect. 2.2 explicitly.

P. 4322, l. 15–17: This CPP retrieval issue concerning the use of climatological r_{eff} values should be mentioned in Sect. 2.2 already.

P. 4322, l. 18: Why is already 50° a critical values? Why are there these wiggles for $\theta_0 = 50°$ in RMS error and Std deviation for pure water clouds with $\tau = 6.4$?

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Interactive Comment

P. 4322, l. 20: "less of an overall bias": there is a very small bias for both liquid water and ice clouds and it is difficult to compare. If you want to compare yet, then I see a smaller bias for liquid water clouds than for ice clouds at large solar zenith angles. Please comment.

P. 4322, l. 23: Can you please indicate the values of the asymmetry factor of the Hess et al. (1998) ice crystals?

P. 4322, l. 26: "and hence different reflectances". This holds tru for the 0.6 μm channel alone (virtually no Rayleigh scattering at 1.6 μm). Please specifiy.

P. 4323, l. 1: Do you always set the surface albedo in the two solar channels to the same value? Why?

P. 4323, l. 4–: Please write down the parameter space explicitly.

P. 4323, l. 21: "why the retrieval errors decrease as the input COT approaches this maximum value": which retrieval error is decreasing in Fig. 5 for pure ice clouds? Why is there an increase in RMS error for $\tau \sim 100$ and then a decrease again?

P. 4324, l. 11–16: Which overestimation and which effect are you talking about? Please mention the precise place in the figure where you see such an effect. In the first row of Fig. 5 I can notice an effect of the albedo increase for f_{ice}=0, 0.02, 0.4 and $\tau < 5$ but where is the overestimation caused by the albedo increase? Please specify.

P. 4326, l. 14–15: In Bugliaro et al. (2011) all low resolution SEVIRI channels were simulated. Please correct.

P. 4327, l. 22: "are not necessarily neutral" –> "are not neutral".

P. 4327, l. 26: "subtle biases": Nobody knows whether a bias (=systematic error) is the result of such an interaction between simulator and retrieval. Please rephrase.

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Further Comments

P. 4313, l. 2–: This discussion of the possible ways of intercomparing weather/climate models and satellite data is interesting but misses one important point, the different spatial resolutions of these data sets (apart from additional issues like temporal resolution, or integration over time). Already these different spatial resolutions hamper a direct comparison of models and satellite data. This is the reason why the ISCCP simulator and other have been implemented, just to make such a "direct" comparison possible. Thus, the direct comparison is not as direct as it seems and the spatial grid used for the comparison is crucial. Furthermore, the simulation of satellite data from model fields is not straightforward neither because weather and climate models often lack microphysical information about clouds (and aerosols) but they almost always lack information about their optical properties. Finally, the value of stage II and stage IV are not mentioned at all. Please rewrite this paragraph in a more articulated way.

P. 4314, l. 11–15: Three-dimensional effects encompass the so-called plane parallel effect and the independent column effect. The first one is related to the unresolved internal variability of a pixel while the second is related to the neglected net horizontal photon transport between neighbouring pixels. The plane parallel effect is considered in Sect. 4.1.3 while the independent column effect has been neglected in this study. This sentence at the indicated position is not completely correct. Please correct.

P. 4315, l. 10–11: "can be attributed to radiative transfer model differences": the reason for the differences is rather the cloud model than the radiative transfer model since both studies make use of disort.

P. 4316, Sect. 2.2: Please indicate the selected cloud bottom and top heights and the atmospheric profiles used.

P. 4316, l. 23: Where does the cloud top temperature come from?

P. 4316, l. 24: How is CWP computed?

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P. 4317, l. 1: “the increased effect of three-dimensional cloud structures” –> “the increased three-dimensional radiative effects”.

P. 4317, l. 7: According to this explanation, the effective radius for ice clouds is actually a volumetric radius r_{vol} and not an effective radius r_{eff}. See Schumann et al. (2010) for details about r_{vol} and r_{eff} and their relationship. Please correct.

P. 4317, l. 14–18: Is there a reference for the cloud detection algorithm you use?

Figure 2: Please add the meaning of the symbols used (θ, θ_0, ϕ) to the caption as well as the surface albedoes used in the two channels (probably both = 0?).

Figure 3: In row a), the clouds in the figure on the right hand side should mimick the shape of the clouds on the left hand side. Why isn't so? Is the x axis title “Cloud Fraction” correct? In row b), the left figure should represent one subcolumn. But one subcolumn possesses no horizontal variability as shown in this plot. Please correct.

Figure 9: it should appear on one full page in the published version.

Technical corrections

Please make sure that you use the correct version of “on board”, “onboard” or “aboard”.

Please consider that “except” should be followed by “that” if it introduces another sentence.

Page 4312, line 24: “an key role” –> “a key role”.

Page 4313, line 15: “synthesised” –> “synthetic”?

Page 4314, line 17: “as large a Europe” –> “as large as Europe”.

P. 4318, l. 24: “to ECSIM standard input cloud scene” –> “to ECSIM standard input”.

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P. 4319, l. 13: “The SEVIRI simulator” –> “The present SEVIRI simulator”.

Page 4323, line 14: “extend” –> “extent”.

P. 4325, l. 28: “reflectancs” –> “reflectances”.

Page 4328, line 7: θ –> θ_0.

Figure 5: “avereged” –> “averaged”.

References

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