Response to Anonymous Referee #1

1. SEVIRI Simulator (Section 3)

#1 Use of the word simulator

We recognize the referee's concern at our use of the word “simulator”, as it touches upon an issue we regularly discussed ourselves until late in the project. It was initially assumed that a simulator is purely a forward model, simulating satellite images, rather than a more general term for synthetic satellite products (i.e. simulating level 1, 2 or 3 data). This inconsistent terminology has found its way into the paper, and should be resolved in the final version. We propose to use the term “forward model” for the derivation of the radiances; the term “simulator” would then refer to the forward model (in which case in simulates a level 1 product) or to the forward model + retrieval algorithm chain (in which case it simulates a level 2 product). In addition, we will refer to the paper of Bugliaro et al. (2011) when we introduce the simulator development done with the LibRadtran model.

#2 Using forward models (“simulators” in the paper, but see #1) to test retrieval algorithms

We agree that a simulator is built with the objective to be as generic and applicable for testing different types of algorithms. This will be true if the simulator is capable of simulating nature in a realistic manner. However, in case assumptions need to be made to perform the simulations (e.g. on the ice crystal shape, vertical distribution of the cloud particles, or 1-dimensionality or 3-dimensionality of clouds) the retrieval algorithms that apply similar assumptions are likely to be closer to the simulated “true” value than algorithms that make other assumptions. For example, if a retrieval algorithm and a forward model operate under very different assumptions of ice crystal habits (e.g. roughened hexagonal columns versus perfect plates), the resulting differences found in a step III evaluation (cf Fig 1 in the paper) will be very hard to interpret. An example of this is illustrated later in the paper, in which we found that single-phase liquid water clouds produce lower retrieval errors than single-phase ice clouds or multi-phase cloudy columns. For this reason, the effects of partial cloud cover were studied only for single-phase liquid water clouds. Similarly, including detailed vertical cloud structure or 3-D radiative transfer without understanding the effects of more basic deviations from the retrieval algorithms ideal situation is hardly a useful exercise. In conclusion, the less assumptions are made by the simulator, the more generic its application becomes. We will better clarify this point in the revised version of the manuscript.

#3 Description of merits and limitations

The main advantage of the forward model we developed and present in the paper is speed, obtained at the cost of limited vertical profile information and disregarding 3-D effects. We believe these pros and cons are already represented in the current manuscript, but we shall expound that discussion.

The simplification of the vertical structure results in uncertainties of a few % in the obtained reflectances in a test involving full vertical structure calculations of the model cloud field in Figure 9. The effect of using 1-D instead of 3-D calculations has been presented in our literature review. 3-D simulations would be preferable, but as stated above the computational costs are too high. Such a simulator cannot be run simultaneously with a climate model run so as to perform an online evaluation procedure. Although these issues were touched upon in the manuscript, we will expound them in the revised version.

#4 Cloud phase

The cloud top temperature of the input cloud (before applying the simplified vertical structure as in Figure 3b) is used as the brightness temperature in the 10.8 micron channel. This was erroneously not mentioned in the paper, but will be rectified.

#5 Cloud overlap scheme

The referee raises several questions regarding the cloud overlap scheme, which we address here.

1. In principle, any number of overlap schemes can be included in the simulator; maximum overlap or random-maximum overlap are two that are particularly straightforward in this regard. We chose instead for the Räisänen et al. (2004) stochastic cloud overlap scheme because this is used in the latest version of our regional climate model RACMO, in which we want to apply the simulator.
2. It is important to note here that the stochastic overlap scheme is only used when the simulator is applied to model output (as opposed systematically working through its input space as we do in Section 4.1). In such cases the number of subcolumns is taken to be equal to the number of subcolumns used in the model.
3. The liquid water cloud is situated between 1 and 2 km; the ice cloud is between 5 and 6 km. We indicated only the cloud top heights in the paper because these are most relevant to the radiative transfer.

The referees further concerns are more difficult to address; the difference between a full vertical cloud profile and the approximations used in this paper is difficult to quantify, because the former has so many realisations that an exhaustive treatment is too large an undertaking, even if one were to disregard the realisations that are deemed unphysical. This goes for both the referee's points about r_{eff} profiles and the separation of phases. As a test, we calculated the reflectances of the model cloud field in Figure 9 with full vertical profiles, and compared them to the reflectances obtained with our simplified method; the difference between the two is a few %.

Regarding the referee's point about r_{eff} being only representative for the upper part of the cloud, it is true that a single value for r_{eff} is always difficult to give in the presence of a vertical profile, although for example Platnick (2000) mentions several possibilities. We stuck to the relatively straightforward method presented in the paper for two reasons: it is computationally inexpensive, and by making it easier for the CWP retrieval in this way we bypass the problematic interpretation of a single r_{eff} value to focus on the effects of multiphase cloudy columns and broken clouds. Regarding multiple scattering between the clouds, we assume the referee means Rayleigh scattering in the layer between 2 and 5 km. This is generally not important, because it contributes little to the overall albedo of the atmosphere when there is a cloud present; only the Rayleigh layer above the cloud plays an important role due to its influence on the top of the atmosphere reflectances (cf. Wang & King 1997).

#6 Radiative transfer details

We do not use DISORT for our radiative transfer calculations, but the KNMI doubling adding code DAK, cf. page 4320 line 8 of the discussion paper. For ozone and water vapour we use a fixed profile, the mid-latitude summer profile by Anderson et al. (1986), which should be noted in the paper. Aerosols are not considered. Different surface albedos in the two solar channels are allowed but this possibility is not used in the paper, we should probably spend a few words on this. The sentence about single-phase cloud calculations being performed in a single layer may be stating the obvious, but we thought it best to include it for completeness. We performed out radiative transfer calculations for monochromatic light, and used CPP's internal line-to-band conversion to treat the spectral response function. The effects of instrumental noise are not included in our calculations. We will include these details in the modified version of the manuscript.

#7 LUT grid and biases

The radiative transfer code and many of the parameters used to build the LUTs in our forward model are identical to those used in the retrieval algorithm, so it is safe to say that the forward model is “biased” (in its non-technical meaning) towards CPP. To make sure that the forward model does not give results tailor-made to fit with CPP, we consciously chose to calculate the new LUTs on a different grid in geometry space (also on a different r_{eff} grid, incidentally), which is what we meant when we said we are avoiding biases. This can and should be explained better in the paper, although we feel that an in-depth exploration of the interpolation uncertainties adds little to the paper.

#8 Surface albedo interpolation

To use the Chandrasekhar formula, the LUT contains reflectances calculated for surface albedo values of 0, 0.5 and 1; from this, α_{hemi} and the product $t(\theta_0)t(\theta)$ can be derived. The ingredients are mentioned in the paper, but the exact process is not, which should be remedied.

#9 Further remarks

We agree with the referee's further remarks and corrections, and plan to implement them in the next iteration of the paper.

2. Results and Figures

#1 Main results

1. We feel that condensing sections 4.1.2 and 4.1.3 to “The CWP retrieval errors are illustrated in Figs. 5 and 6” is not entirely fair, because we continue to point out trends and features in those figures. Adding a more detailed description of what is going would make an improvement, though.
2. We assumed these terms would be self-explanatory, but we will add a description in the paper as follows:
   a) The relative RMS error is given by SQRT[<(CWP – CWP*)2>]/CWP*
   b) The relative standard error is given by SQRT[<(CWP - <CWP>)^2>]/CWP*
   c) The mean relative error is given by (<CWP> - CWP*)/CWP*

   Where <...> denotes a mean over the relevant geometries; CWP denotes the retrieved values as a function of geometry for a given cloud configuration over a given surface; and CWP* denotes the the true value for this cloud configuration. If we treat our retrievals as measurements, with different geometries standing in for repeated measurements, we feel that the concepts accuracy and precision are entirely appropriate: the RMS value gives a measure for the offset from the real value, while the standard deviation shows how far apart the error are from each other for a given cloud configuration.

3. In principle, only showing the relative RMS error already goes a long way to show the retrieval uncertainties. However, including the mean error gives more information about what a high RMS error means for a series of measurements (e.g. a climatological mean). Similarly, including the standard deviation of the retrievals gives information on whether a high RMS error can in principle be compensated for. We feel these reasons justify our use of all three rows in the figures.
4. The referee's point is well taken; we note, however, that the current colour tables are already a vast improvement over many common ones. We shall try to further improve the contrast.

#2 Further comments

1. Agreed, we will apply this in the next version of the paper.
2. When we apply the simulator to a climate model, the retrieval algorithm is run on the reflectances of each subcolumn and the resulting CWP is averaged. We neglected to state this in Section 3, and will do so in the modified version. For the purposes of the rest of Section 4.1.3 we compare the retrieved CWP with the averaged CWP of the partially cloudy pixel.
3. We disagree with the referee that Section 4.2 is the main reason for writing this paper. Our main reason was to quantify the retrieval uncertainties that one can encounter in model evaluation studies. Where the preceding sections were all of a very theoretical nature, we thought it would be illuminating to show how the procedure used in Section 4.1 looks when applied to a model cloud field. Comparison of a single RACMO cloud field with SEVIRI observations would add very little in that regard; the main purpose we want to use the simulator in RACMO is to gather statistical data over long timeseries.
4. We agree with the referee's points about, and will rephrase accordingly.

#3 Specific comments

We agree with most of the points made by the referee. Our response is given below:

P. 4321, l. 12—15: Changes will be made as suggested by the referee.

P. 4321, l. 23: Changes will be made as suggested by the referee.

P. 4321, l. 24: Changes will be made as suggested by the referee.

P. 4322, l. 3 and in the following: It should be $\theta < 72^{o}$, change will be made to clarify this point.

P. 4322, l. 3: Changes will be made as suggested by the referee.

P. 4322, l. 13: the liquid water clouds in our experiment are at the same height as in CPP.

P. 4322, l. 13: Changes will be made as suggested by the referee.

P. 4322, l. 15—17: Changes will be made as suggested by the referee.

P. 4322 l. 18: we will look into the reason for the wiggles; we have chosen 50^{o} because the upwards trend in the RMS error starts about there.

P. 4322, l. 20: The bias for ice clouds is slightly lower at small solar zenith angles, but not worth noting. We suggest removal of the offending sentence.

P. 4322, l. 23: Changes will be made as suggested by the referee.

P. 4322, l. 26: This will be specified in the modified version of the manuscript.

P. 4323, l. 1: We don't think it will add much to the current study if we treated the surface albedo at 0.6 micron and at 1.6 micron as independent variables, even if such instances occur in nature (icy surfaces being a prime example).

P. 4323, l. 4—: Changes will be made as suggested by the referee.:

P. 4323, l. 21: This issue was more visible in an earlier stage of the paper, which had a linear scale on the x-axis of Figure 5. The RMS retrievals increase steadily with COT, until COT=80, after which it decreases slowly. The reason for this is as stated: for these values of COT, the maximum value in CPP is retrieved.

P. 4324, l. 11—16: This will be specified in the modified version of the manuscript.

P. 4324, l. 14—15: This will be corrected in the modified version of the manuscript.

P. 4327, l. 22: Change will be made as suggested by the referee.

P. 4327, l. 26: Sentence will be rephrased as suggested by the referee.

Further comments

Following are our replies to the referee's comments:

P. 4313, l. 2—3: We agree with the referee that this paragraph should be rewritten in the modified version of the manuscript.

P. 4314, l. 11 – 15: We were under the impression that 3-D effects only referred to horizontal photon transport. We are hesitant to gather our treatment of sub-pixel variability under the umbrella of 3-D radiative transfer effects, since it is merely a superposition of 1-D treatments.

P. 4315, l. 10 – 11: We did not use DISORT in this study, but DAK.

P. 4316, Sect 2.2: Changes will be made as suggested by the referee.

P. 4316, l. 23: The cloud top temperature is obtained from the input model cloud field.

P. 4316, l. 24: This is described on page 4317, lines 19 – 24.

P. 4317, l. 7: This will be corrected in the modified version of the paper. We note, however, that this is the same definition of effective radius as is used in the CPP algorithm.

P. 4317, l. 14—18: The cloud detection algorithm is a modified version of the MODIS cloud detection scheme. The modification is made by Jerome Riedi from the university of Lille, and is presented in Roebeling et al. (2008). In the modified version of the manuscript we will refer to this publication.

Figure 2: Changes will be made as suggested by the referee.

Figure 3: We have constructed row a) so that the right hand side more or less follows the left. It is a slightly exaggerated “artist's impression” of how the stochastic scheme divides the cloud over subcolumns, something we feel it illustrates well enough. In row b), there is indeed no horizontal variation within the subcolumn, but there is still a vertical profile of cloud water content that we tried to illustrate here.

Figure 9: We agree with the referee.

Technical corrections

All technical corrections will be made as suggested by the referee. We thank the referee for catching remaining errors.

References

Anderson, G.P., Clough, S.A., Kneizys, F.X., Chetwynd, J.H. and Shettle, E.P., AFGL atmospheric constituent profiles (0 – 120 km), Tech. Rep. AFGL-TR-86-0110, 43 pp., 1986

Platnick, S.: Vertical photon transport in cloud remote sensing problems, J. Geophys. Res., 105, 22 919 – 22 935, 2000

Roebeling, R.A., Deneke, H.M. And Feijt, A.J., Validation of cloud liquid water path retrievals from SEVIRI using one year of CLOUDNET observations, J. Appl. Meteor., 47, 1, 206 – 222, 2008

Wang, M. and King, M.D.: Correction of Rayleigh scattering effects in cloud optical thickness retrievals, J. Geophys. Res., 102, 25 915 – 25 926, 1997

Response to Anonymous Referee #2

The primary motivation of our research is that we are seeking for a methodology that permits us to use CPP inferred cloud parameters for the evaluation of climate models. The interpretation of a direct comparison of CPP cloud parameters and climate model output is seriously hampered by the conflation of model errors and retrieval uncertainties. The purpose of our paper is to present an approach to separately quantify the retrieval uncertainties in the context of application of model evaluation, that is at spatial and temporal scales represented by climate models. Since independent validation measurements to identify the retrieval uncertainties of the CPP algorithm do not exist we have adopted the approach in which we regard the climate model as true and independent and have developed a forward model that's fast enough to permit us to scan the full range of cloud parameters and systematically quantify the uncertainty structure of the CPP retrieval algorithm.

The purpose of writing this paper is to present the approach, develop the simulator (equivalent to forward model plus CPP retrieval), apply it to model states, and quantify the uncertainties of the retrieval algorithm.

We believe that pointing out these various steps more clearly in the modified manuscript will help to address several of the referee's concerns.

1. Terminology

#1. “Quantify retrieval uncertainties”

In Section 4.1 we present the retrieval errors introduced by the CM-SAF CPP algorithm for a variety of cloud parameters, and therefore we feel justified in calling this a quantification of the retrieval uncertainties. We are aware that we do not consider an exhaustive list of sources for retrieval errors, which may have been what the referee was expecting, but we do not see how this term is misleading per se. A table summarising the uncertainties we find in numbers will be added to the modified manuscript.

#2. “Simulator”

First of all, we would like to draw the referee's attention to our reply to Anonymous Referee #1, in which we address our use and interpretation of the word “simulator”. There, we propose a slight change in terminology in the paper, which we will also use throughout the reply. Simply put, we now use “simulator” to refer to the process of converting model output to level 1 or level 2 satellite products; “forward model” and “retrieval algorithm” can then be seen as steps in this simulator.

That being said, the parameter space investigated in Section 4.1 is chosen to reflect the cloud parameters that are typically produced climate model, after the simplifications to the vertical structure discussed in Section 3 are applied. The link to climate models is perhaps not as strong as indicated in the introduction and Figure 1, but in our view it is still strong enough to warrant the use of the word simulator in this case. We propose to include a short description of this link at the beginning of Section 4 in the modified version of our paper.

For now, we feel that the current title covers the contents of the paper better than the proposed alternative.

2. Section 4, results

#1 Model-driven vs author-driven

We agree with the referee that there should be a clear description at the start of Section 4 (and elsewhere) linking the retrieval uncertainty study to the simulator.

Section 4.2 and Figure 9 appear in their current form to illustrate how the methods used in Section 4.1 would look when applied to a climate model cloud field. As such, our intention has never been to investigate in detail the retrieval uncertainties in that particular cloud field, but rather to reinforce the link between Section 4.1 and the climate model fields mentioned in the introduction, Section 3, etc.

#2 Section 4.1 error sources

Section 4.1.1 is a special case in that it deals with single-layer, single-phase clouds, i.e. the cloud fields investigated there coincide perfectly with the assumptions made in the CPP algorithm. Therefore, the only reasons we can think of when we see a difference between input CWP and retrieved CWP are

1. Difficulties in r_{eff} retrievals at low COT
2. Difference in cloud top height for our “pure ice” clouds
3. interpolation errors made when obtaining reflectances (in the forward model) and COT or r_{eff} values (in the retrieval algorithm) from their respective look-up tables.

The errors introduced by the latter source are considered small compared to others, particularly those introduced by the input cloud field not matching the assumptions made by CPP. In our view, the sources of retrieval errors shown in the figures are clearly explained in the body of the paper, but we agree with the referee that they should also be referenced in the figure captions.

We chose to show the CWP retrieval errors in our figures rather than COT and r_{eff} separately because in our perception CWP is the more fundamental of the three in climate models. CWP stems directly from cloud liquid and cloud ice content, which in most present-day climate models are treated as prognostic variables. Moreover, we discuss their respective contributions in the text, and we do not see the added value of showing the bias structures of COT and r_{eff} separately.

3. Other comments

L. 45, etc.: This will be corrected in the modified version of the manuscript.

L. 95 and Section 4.1.3: We use an independent column approximation to deal with partial cloud cover, this will be explained in the modified version of the manuscript.

L. 253: We will correct this in the modified version of the manuscript.

L. 351: We are interested mostly in CWP because it is the prognostic variable. We therefore choose to focus on the CWP retrieval uncertainties, and will reference the underlying causes (i.e. uncertainties in the retrieval of COT and r_{eff}) in the text.

“Fig. 6. As Figure 5 ...”: This will be corrected in the modified version of the manuscript.