Reduction of radiation biases by incorporating the missing cloud variability by means of downscaling techniques: A study using the 3D MoCaRT model

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We would like to thank all three reviewers for the thoroughly reading of the manuscript and the valuable comments that helped to improve the paper.

In the next sections we go over all comments and remarks of the reviewers.

1 Report #1 (Reviewer 1)

Reviewer #1 recommended the publication of the paper with minor revisions that divided in *major*, *minor* and technical/language comments. The language comments not commented here have been included into the manuscript exactly as suggested.

Major comments

Comment 1: ... I would therefore recommend to weigh the two parts of the manuscript (a: MOCART description and introduction; b: applications) accordingly (i.e., de-emphasize the applications). If, however, the authors decide to give equal weight to (a) and (b), it will be necessary to provide a more thorough overview of the literature than currently given ...

   Response 1: We included an extended overview of the literature related to 3D radiative transfer and stochastic cloud models.

Comment 2: ... the "reflectance difference" shown on the y-axis is, in fact the *relative*, not the absolute difference. This needs to be made clear in the revised manuscript - I would recommend to show the defining formula provided in the response: Delta_R = (R_coarse - R_ref) / R_ref. ...

   Response 2: Proper clarifications and the defining formula have been included in the manuscript.

Minor comments

p1547,l7 & p1547,l8: Mie theory can only be applied to spherical particles (or be extended to a few non-spherical particles). Mie theory can therefore not be applied to ice crystals and many aerosol types.
   ... as mentioned above, the *spherical* in this statement contradicts the previous statement. The authors can probably assume that the reader is familiar with this fact and can delete this sentence.

   Response 1: We assume that the reviewer refers to page 1548, lines 7–8. We agree

with the reviewer's comment. The sentence could lead to confusion. With the sentence "In order to calculate the optical properties of clouds and aerosols, the Mie theory can be applied.", it was meant that, in MoCaRT, the optical properties can be either computed by means of Mie calculations or by means of parameterizations. Other routines are not implemented in the current version. If particles cannot be considered spherical, e.g. most ice crystals and aerosols, the optical properties calculated by the Mie theory can be inaccurate. For better results, the optical properties calculated by other methods (e.g. *Mishchenko et al.* (1996); *Hess et al.* (1998); *Doicu et al.* (2006); *Rother* (2009)) should be provided explicitly as input. The sentence have been accordingly rewritten and a footnote added.

p1551,l1: "a method similar to Barker". This being a mainly technical paper, the description of the techniques is rather slim. Barker, for instances, describes various methods- in this paper, and at the very least, it needs to be explained what the "guts" of MOCART are - possibly in the form of a table. Yes, when going into detail, this would be a paper on its own, but some detail is adequate here.

   Response 2: Without the previous lines, it is not clear which is the method similar *Barker et al.* (2003) that we have implemented in our model. However, the paragraph is pointing out the problems caused by sharp forward-peaked phase functions and *Barker et al.* (2003) proposed a method to deal with this specific problem. We include more information in the manuscript for the sake of clarity.

Conclusion: I cant really see how solutions to handling 3D effects in, e.g., gas retrievals are offered in this manuscript, at least for operational retrievals. Avoid overpromising. How would, for example, MOCART help in the calamity that only 2-5% of, lets say, GOSAT retrievals are useable?

   Response 3: We agree that accounting for 3D effects is computationally expensive

and very challenging to be implemented in operational processing chains. But a better understanding of the radiation–clouds interaction will surely improve atmospheric retrievals and 3-D radiative transfer (RT) is closer to nature than 1-D RT. For instance, as pointed out in the conclusion, for methane and carbon dioxide retrievals with GOSAT only cloud-free observations are used. If the interaction of radiation with clouds could be better characterized (either with MoCaRT or with another RT model), pixels containing some cloudiness could still be useful. Currently, there are some studies proposing corrections of (1D) atmospheric scattering effects for carbon gas retrievals with GOSAT (*Bril et al.*, 2007; *Oshchepkov et al.*, 2008). These studies proposed a parameterization of the scattering effects. The study presented in this paper choose a different approach: downscaling the cloud properties to a finer spatial resolution.

Language comments

p1544,l6: "esp." → "especially" (do not use unknown abbreviations)

Response l1: *esp.* is not an unknown abbreviation, or at least not one invented by us. According to the Oxford Dictionary (http://oxforddictionaries.com/definition/esp.?q= esp.):

esp.

abbreviation

especially.

esp. has been changed to *especially* in the manuscript.

p1544,l10/11: "transfer. In turn," → "whereas".

Response I2: The sentence:

"Three-dimensional radiation models can deal with much more complexity than the one-dimensional ones providing a more accurate solution of the radiative transfer. In turn, one-dimensional models introduce biases to the radiation results."

has been rewritten:

"Three-dimensional radiation models can deal with complex scenarios providing an accurate solution to the radiative transfer. In contrast, one-dimensional models are computationally more efficient but introduce biases to the radiation results."

p1544,l14: fix "fine-resolved" (English incorrect).

Response I3: "spatially fine-resolved cloud fields" have been changed to "cloud fields at higher spatial resolution". Also changed to "high-resolved cloud field" in later appearances.

p1545,l7: revise "shadowed" and "extra-illuminated"

Response I4: It has been changed to "shaded and extra illuminated" in manuscript.

p1545,l9: either "cover" or "fraction", not both.

Response I5: Although "cloud cover" and "cloud fraction" are probably more popular than "cloud cover fraction", this last term is also in use in the atmospheric community. Actually, in our opinion it better describes the concept of "the fraction of the sky that is covered by clouds" (what exactly is considered a cloud is a different matter, as pointed out by reviewer 1). The concept "cloud cover fraction" have been previously used

in, e.g. *Wood and Bretherton* (2006); *Coakley Jr and Bretherton* (1982), http://sacs.
aeronomie.be/info/cloudfrac.php, http://gcmd.nasa.gov/KeywordSearch/Metadata.do?
Portal=GCMD&KeywordPath=Parameters|CRYOSPHERE|SNOW%2FICE|SNOW+COVER&
34024&MetadataView=Full&MetadataType=0&lbnode=mdlb1, ...

Cloud cover fraction has been changed to *cloud fraction*.

p1546,l3-4: Incorrect English/Grammar: "at the cost of renouncing to the desirable
exact mathematical solution and considerably increasing the calculation time"

Response I6 Rewritten:

". . . at the cost of forsaking/abdicating/giving up the exact analytical solution and of
increasing considerably the calculation time."

p1550,l3: "a bunch of photons" rewrite without using slang.

Response I7: *bunch* → *bundle*.

p1551,l4: Revise word order (this sentence doesnt work)

Response I8: Rewritten:

"In order to validate the MoCaRT model, we present in this section a comparison
of MoCaRT with the consensus results of the Intercomparison of Three-Dimensional
Radiation Codes (I3RC) project"

p1553,l22: "seem to be twins" sounds poetic, but probably shouldnt be used in a sci-
entific paper - because its a very qualitative statement

Response l9: We changed "seem to be twins" to "are almost identical". We believe that the next sentence in text clarified scientifically the poetical meaning: "The mean, maximal and minimal values agree better than 0.1 %".

p1558,l9: "convenience" not the right word here

Response l10: *convenience → benefit*

p1562,l3: "The neglect of" does not work.

Response l11: "The neglect of" is widespread in scientific papers. We are open to suggestions.

2   Report #2 (Reviewer 2)

Reviewer 2 considered the manuscript a "well-written" paper where a study following "appropriate methodology" is presented. He considered MoCaRT a new "highly capable and versatile" radiative transfer model with "new tweaks in simulation methodology" and recommended the paper for publications with minor modifications.

Minor comments

Page 1545, line 9: Due to improved cloud detection capabilities, satellite estimates of global cloud cover have gone up since the 1995 study cited in the paper. Because there is no single best estimate of cloud cover (partly because there is no clear-cut definition of what exactly should be considered a cloud, and what should be a population of particles floating in clear air), I strongly recommend deleting the 62% value and only say something along the lines of about two thirds or over 60%. Referencing more

recent studies on cloud cover could also help, but is not crucial.

Response 1: The value 62% has been changed to "about two thirds" in manuscript and the references have been updated.

Note that considerations about cloud fraction also play an important role in the downscaling approach. The current downscaling algorithm set the cloud free subpixels to zero LWC. However, it could easily be adapted to give these subpixels a distribution with small LWC values to simulate the cloud water values below the detection threshold of the satellite.

Page 1550, lines 6-9: It is not clear to me how the third method differs from the second one. A bit more detail would be welcome to clarify this.

Response 2: The third method described in the paper refers to the so-called "continuous absorption method" and differs from the second one, the so-called "weighted-scattering method". The weighted scattering method can be regarded as the radiative transfer of a bundle of particles in a scattering–absorbing medium. At each interaction event, the "energy weight" of each "bundle of photons" is reduced according to the single scattering albedo (probability of not being absorbed) at the interaction position. In the continuous absorption method, the medium is considered to be purely scattering and the energy weight of the "bundle of photons" is reduced during the trajectory between scattering events according to the Beer's law. Note that in this energy reduction, only the absorption optical thickness is considered.

Explanatory sentences have been added.

Page 1551 lines 1-2: Because the *Barker et al.* (2003) paper describes several methods, a few key words identifying the method implemented in MoCaRT would help.

Response 2: See *Response 2* to reviewer 1.

Most Figures: Many figure labels are rather small, and I strongly suggest increasing them.

Response 3: The labels of the figures have been enlarged. Since there are 5 plots per row they are still small but hopefully readable.

Page 1559, line 25 to page 1560, line 5: These sentences repeat the information in the figure caption and are not necessary.

Response 4: Although the information of this paragraph is already contained in the figure 6 caption, the authors would prefer to keep this redundancy for readability reasons.

Figure 6: While reflectivities themselves are of interest, it would also be useful for readers to estimate how the differences in reflectivities would affect satellite measurements of cloud properties. Even a simple conversion from R to tau using a 1D look-up table would give readers a better idea about the practical significance of differences. This would also connect the paper more closely to the main focus of the journal, atmospheric measurements.

Response 5: Reflectivity is a function of cloud optical depth among others. By means of the asymptotic theory for thick atmospheres (see e.g. *King* (1987); *Nakajima and King* (1990)) or, as recommended by reviewer 2, by a 1D look-up table, the reflectivities can be transformed into optical depths and the differences found in this study interpreted in terms of optical thickness. Although we agree with reviewer 2 that the results presented in this manner (optical depth) could have more impact than presented as we did (reflectivities), we prefer to leave the reflectivity graphics, since the

main focus of the paper was to reduce radiation biases by means of downscaling techniques. We will put the study in context by adding some convenient references where the results have been shown in terms of optical thickness, e.g. *Varnai and Marshak* (2002); *Zinner and Mayer* (2006).

Figure 8: It took me a little while to figure out the meaning of the thin line, and so I suggest describing it in the figure caption. I would also point out in the text a remarkable feature of the figure, that 3D and small-scale variability always reduce reflectivity. (This means that cloud sides intercepting extra incoming sunlight is not the dominant effect even for low sun.) Also, it would be interesting to see how scene albedo behaves, and to discuss why the differences peak well after noon. Finally, the last two sentences of the figure caption are unnecessary (the first of the two repeats information from the text).

Response 6: We explained the meaning of the thin red line in figure 8 and added the feature pointed out by reviewer 2 that 3D and small-scale variability always reduce reflectivity. Also, some words about the maxima have been added.
Page 1562, line 28 to page 1563, line 4: These sentences dont seem to be well connected to the earlier parts of the paragraph, and so they could be reworded or moved elsewhere.

Response 7: We have reformulated these sentences and placed in a new paragraph. Since in the first part of the paper we introduce a new 3-D RT model, we think that it is appropriate to include —even if they are well-known in the atmospheric RT community— some general ideas about 3-D radiative transfer.

Small language issues

Response 8: All proposed spelling corrections have been incorporated into the

manuscript.

3   Report #3 (Reviewer 3)

Reviewer 3 considered that the paper would fit into the ACP journal but had some reservations for the paper to be appropriate for AMT arguing that "there is no clear path offered on how the findings of this paper will be applied in satellite retrievals".

Response:

We presented in this paper a new 3-D Monte Carlo radiative transfer model that can be used for sensitivity studies in inhomogeneous scenes, as it is the case of the present study. Additionally, the paper can be regarded as a validation of the downscaling algorithm presented in *Venema et al.* (2010). It is true that we do not present any cloud retrieval product (e.g. optical depths), neither the impact that spatial resolution has on any of them. For such a study we refer to, e.g., *Zinner and Mayer* (2006); *Varnai and Marshak* (2002). In our case, we focused on the radiation fields leaving out the inversion process. However, we showed the impact of the spatial resolution on reflectivities and that the biases can be reduced by properly downscaling the cloud properties. Our findings are based on about 50 cloudy scenes with different structure and different illumination geometries, which makes them rather general. Since retrieval products depend on radiances, the implications on retrievals are straightforward. Moreover, downscaling techniques are gaining attention in the last years for improving remote sensing products and this work is enshrined in this context.

We leave the decision of the adequacy for the AMT journal to the editor.

Specific comments

(1) One thing that is extremely important to get straightened out is the resolution of the CRM cloud field at its original form and when used for simulations: I can find 40x40 in P1556L7, P1556L8, P1557L11 and P1561L25, while I can find 100x100 in P1554L23, P1555L23 (implicitly), P1559L11. Which one is it?

   Response 1: The horizontal resolution is 100 m by 100 m. It has been corrected throughout the paper.

(2) When the Venema scheme is applied to produce the subgrid variability, does it use any info from the original field? And since a lot of the remote sensing discussion is on clear vs. cloudy, can the subgrid variability produced by the scheme also result in clear pixels? While it is acceptable that a full description of the scheme is not repeated here, something about its capabilities and required input needs to be mentioned.

   Response 2:

   From every original fields, the coarse means and the coarse cloud fractions are computed and used as input to the algorithm. Furthermore, from all original high-resolution fields we have computed an average isotropic power spectrum and fitted this empirical function to the coarse power spectrum to extrapolate them to the small scales.

   In a real application, the extrapolation of the coarse power spectrum can probably be made using a power law fractal function. These LES clouds did not follow this behavior accurately. Alternatively, the power spectrum at small scales could be estimated from a higher resolution measurements, such as an airborne imager or particle probe.

(3) Since the work is presented to have remote sensing implications why is the error

presentation framed in terms of domain averages? I would expect to see something like frequency distributions of errors at the pixel level, and yes, I understand that to create reference results at the pixel level is computationally much more expensive (how noisy are the reference radiance fields in Figs. 4 and 5?)

Response 3: The fact that we presented the errors in terms of domain averages does not mean that the results do not have remote sensing implications (see e.g. *Kato and Marshak* (2009)). There are spaceborne atmospheric sensors of a wide variety of spatial resolutions providing gas concentrations as well as cloud products. One could consider that the whole domain represents one instrument pixel with coarse resolution. Nevertheless, we agree with the comment and consider the reviewer's suggestion for a next study. The noise level of all clouds is low and actually the same for all resolutions, since each "coarse" pixel was considered as a composite of 4×4 subpixels with identical properties during the RT simulations.

(4) Figure 8: Why are transmitted and absorbed flux panels smaller in size than for reflected flux? Why is the difference not shown for these panels? Do the authors imply that these fields are less important? Why are you calling the absorbed flux "power"? Why does the caption talk about flux "density" when actual fluxes are shown? (Id use "density" if the fluxes were normalized)

Response 4: The reason for the different plot sizes was for having all in the same raw, just logistics. We put all the same size and include the missing bias red-line in T and A.

We used "flux density" by inertia, since it was used throughout the paper. The terms "flux" and "flux density" are sometimes ambiguously interchanged in the literature describing the same quantity. We will follow the convention that "flux" refers to *energy per unit time per unit area* and that "flux density" refers actually to "spectral flux density", i.e. flux per unit wavelength (wavenumber or frequency). Since the fluxes shown in this

section are integrated over the whole solar spectral range, all "flux density" appearances have been changed to "fluxes".

(5) In P1554L2 the authors mention an "incomplete 3D view of clouds" (from current measurement techniques). Im curious as to what technique they have in mind (example).

Response 5: To the knowdlegde of the authors there is no current experimental technique which provides an instantaneous three-dimensional view of cloud properties such as *LWC*, *cloud droplet concentration*, or details about the droplet size distribution itself. In-situ measurements of cloud microphysical properties from aircraft, balloon, or zeppelin platforms provide highly valuable information about the droplet size distribution, but only along the one-dimensional (1-D) flying track. Remote sensing techniques such as cloud radars, lidars and microwave radiometers provide two-dimensional (2-D) horizontal "snapshots" —in case that they are mounted on a spaceborne or aircraft platforms— or 2-D vertical sections of cloud properties —in case they are part of ground facilities. Multi-angle spectrometers like MISR or scanning radars provide a more general view of the clouds, but still not the whole 3-D picture. The spatial distribution of clouds is better probed by means of remote sensing devices, but the remotely sensed information is not as accurate as the one acquired in-situ. A better approximation of the three-dimensional (3D) cloud structure can be achieved by the combination of all available measurements by different techniques. However, this combination is not possible (or very difficult) in most of the cases, since usually there are no simultaneous measurements of the same cloud scene and, if the measurements are available, the combination is not straightforward since the instruments involved have different sensitivities, integration times, spatial resolutions, collocation, etc. Even in the best case, there will always be gaps of information in some region of the cloud field that will have to be filled by means of simplifications (homogeneity, adiabaticity, etc) or by unappropriated mathematical manipulation as, e.g. interpolation, what would introduce artificial

14

homogeneity.

(6) Last sentence of the abstract: it is stated what is done, but not what the finding is (as is appropriately done in the preceding sentence).

   Response 6: The abstract have been extended with the findings. Additionally, we pointed out in the abstract that we introduce a new RT mode.

(7) P1548L20-24. Just wanted to point out that the way CPPHA is described, it is implicitly equivalent to random cloud overlap which is a pretty poor approximation.

   Response 7: We just mentioned the capability of the MoCaRT model to calculate the radiative transfer making use of CPPHA. We did not claim it to be an accurate approximation. Is is included in the model for flexibility and for comparison purposes.

(8) I think that the two mentions of Jensens inequality (P1560-1561) are quite esoteric for the general reader. Explaining this a bit better (ie., the reflectance curve is convex) would be helpful.

   Response 8: The sentence "... This is a direct consequence of concave (reflectivity) and convex (transmissivity) functions as stated by Jensen's inequality. ..." has been included in the introduction.

(9) P1560L9-11, while true most of the time, this is strictly not always true in the left panel of Fig. 6.

   Response 9: The sentence:

   "The higher the reflectivities (i.e. higher optical depths), the higher the differences

15

between the coarse clouds and the reference ones."

has been changed to:

"Notice the general tendency that the higher the reflectivities (i.e. higher optical depths), the higher the differences between the coarse clouds and the reference ones."

(10) Typos, other errors, and suggestions for different word choices

Response 10: Most of the remarks/suggestions have been considered.

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