Referee #2 Response

We thank the reviewer for his or her many helpful and insightful suggestions and comments. We have responded to each one below, as well as noted changes to the manuscript.

There is no reference at all to previous studies concerned with the simulation of satellite radiances starting from weather/climate models. Such studies include but are not limited to Jonkheid et al. (2012); Bugliaro et al. (2011); Otkin et al. (2007). Furthermore, simulator suites for different sensors like the EarthCare Simulator ECSIM have not been mentioned neither. This should be corrected.

Similarly, the new aspects addressed in this paper have not been adequately illustrated by the authors. This should be added to the introduction.

We apologize for this major oversight. We have added short descriptions of these previous works in Section 1 (after line 13), together with comments on how our current work differs.

Since the focus is on clouds, Sect. 2.3 is the key aspect of the paper. Here, references to previous studies (Venema et al., 2010; Bugliaro et al., 2011, and more) should be made and similarities/differences explained. Furthermore, one example (i.e. one Figure) of the generation of cloud fields at high spatial resolution starting from the model output would more intuitively sustain the explanation (especially the "clumping" procedure).

We have now addressed some of these differences generally in the introduction. But we add text to section 2.3 a discussion of differences between our sub-gridcolumn cloud generation method and the spectral downscaling approach of Venema et al. (2010) and Bugliaro (2011).

With regard to a figure, we do not think we can adequately demonstrate the entire method with a figure, and prefer to describe it with words and with references for details to Norris et al. (2008) and Norris and da Silva (2013). Nevertheless, we have greatly expanded our description of the clumping algorithm, hopefully to make it clearer to the reader.

p. 4114 l. 5: *What is the meaning of "rank" in this context?*

This has now been explained in much greater detail in the text.

Entire Sect. 2.3: *One important point should be the consistency of the cloud downscaling with the model physics if one intends to use such simulations for model evaluation. How consistent is the procedure presented here with the model? Why don't you use the second procedure described in p. 4114 l. 14– 27 since you say that it is 'very much akin to the internal GEOS-5 treatment of cloud overlap'?*

We address these questions in the final paragraph of the expanded Section 2.4.

p. 4115 l. 4 – p. 4116 l. 4: *Is it really necessary to explain this issue in this level of detail? Of course, the point you make here is essential for the correct generation of high resolution cloud fields with consistent optical properties but what's the point for this discussion? If you consider this part necessary, please integrate it in a better way into the manuscript.*

We agree that this section is too long and too detailed. We have shortened it greatly. It was originally included more as a "note to self" in the manuscript and that was a mistake. We just summarize the main point now, without the details.

p. 4113 l. 14: *Where do the effective radii in the model come from? Do you use one effective radius for all N cloudy subcolumns? Is this realistic enough? Are there vertical profiles of effective radii?*

We use the GEOS-5 effective radii, which is a simple pressure and temperature based effective radius profile applicable to each gridcolumn individually (i.e. each gridcolumn has a different profile). This same effective radius profile is used for all subcolumns in the gridcolumn. We have added some more text to this effect at p.4113, l.14. As to whether this is realistic enough: ultimately, no. But it is a start for the current introductory study. It represents another element we can explore in future studies. We notice that Jonkheid et al. use a uniform r_e in each of their two cloud layers, while Bugliaro et al. use a constant droplet concentration across the domain (at least for liquid condensate) and an r_e that therefore varies with LWC. In actual clouds, clearly neither of these assumptions will be true, and our pressure and temperature based r_e is no better either. The number concentration will vary horizontally and vertically in response to vertical velocity variations and activation of CCN. We will look at different effective radius models in future work.

p.4112l.12: *Even for the 1km pixels internal variability is an issue (plane parallel error) that you neglect here. Couldn't you downscale the cloud field to even smaller scales to produce such variability? Why do you neglect this variability?*

Yes, we could certainly conduct our downscaling to sub-1km scales and then average back to 1km pixel radiances for input to the MODIS retrieval algorithm. We expect that the plane parallel bias at this scale would be small, but it would be interesting to test that, as well as to study cloud edge effects (MODIS "clear sky restoral") at this finer scale. Our main reason for not doing this yet is both the larger computational cost and that we expect it to be a second order effect. But we will investigate it as future work.

Why do you compare retrieved cloud properties from real and simulated satellite scenes instead of comparing model clouds against clouds retrieved from real MODIS granules? Your procedure adds an additional degree of freedom since you have to apply the retrieval twice. This questions is related to the general strategy and future applications of the simulator. Please explain your motivation in the introduction ***p. 4108 l. 22–26*** *in a more detailed way (retrieval validation against known truth is evident, but model validation is not).*

We have tightened up the p. 4108 l. 22-26 discussion to be more clear and have added a discussion of your question (viz., Jonkheid et al type I vs. type IV) just after our discussion of Jonkheid et al. (2012) in

Section 1 and also further discussion near the end that section, where we address the limitations of type IV comparisons caused by forward model accuracy.

Title and manuscript: *"equivalent sensor radiance" is not an established concept and needs a definition in the manuscript. It reminds of 'equivalent black body temperatures' in the thermal spectral range but has a specific meaning valid only in this paper. For this reason, I would also choose another title that does not make use twice of this concept like the current one.*

We have changed the title to be more appropriate to what our software product is all about. We agree that "equivalent sensor radiance" term can be confusing.

p. 4106 l. 21–22: Please add a reference about the fact that high clouds (i.e. thin cirrus) have an overall net warming effect.

We have done so. Thank you.

p. 4110 l. 19: What is the wavelength dependency of the LUT?

There is no wavelength dependency per se. A Cox-Munk albedo is explicitly calculated for each MODIS channel under consideration. The LUT is a function of wavelength only to the extent that wavelength is one of indices in LUT. A user with a different sensor would need to produce their own Cox-Munk LUT made for the channels of that specific sensor. The software however only depends on simply having the table available for whichever sensor it is.

p. 4111 l. 27: Kratz's paper is for AVHRR channels. Have you applied this technique to MODIS channels? Can you apply it to new different sensors as well? If not, this will decrease the flexibility of your simulator. Please explain.

The exact technique described in Kratz's paper has been applied to a variety of sensors. This very technique has been applied to all the

MODIS channels, which is why this particular paper is referenced. Additionally it has been applied to channels of VIIRS, SEVIRI and a number of airborne sensors and correlated-k distributions generated accordingly. However, correlated-k does not have to be used in order to perform these simulations. Absolutely any method that would allow a user to obtain an optical thickness profile for various gaseous absorbers for a specified atmosphere will be sufficient. We have added a clarification to that effect in the paper.

p. 4112 l. 8: Can you give a reference for this assertion?

We do not have a reference, but we have made calculations and this is what we found to hold. We have added a comment to that effect in text. This of course may not be true for a different sensor. But the software does not depend on having a specific number of levels. A user may specify as many levels as they desire, of course at expense of execution time. We of course wish to complete the calculations in an amount of time reasonable for global studies. Local and regional studies may be performed at higher vertical resolution without any adjustments to the code.

p. 4116 l. 12: Disort is a 1D radiative transfer solver. Thus, you neglect 3D radiative effects. Please explain your choice.

A 3D code that accounts for horizontal photon transport (vertical transport is already handled by DISORT) is only justified if the model provides realistic 1km and smaller horizontal cloud fields, which it doesn't even come close to (model is run at 0.25° with IPA sub-columns followed by a 'clumping' approach as described in Sect. 2.3).

Our simulator code is designed is such a way that it is not tied to a particular RT core and it can be easily swapped by a user for something else that they like better.

p. 4116 l. 19: A large number of streams is important to reproduce details of the phase function like the rainbow and the backscatter glory (e.g. Mayer et al., 2004), for the accurate modelling of the forward *peak additional effort is needed, as you describe below at l. 24. Please correct this.*

We have reworded this paragraph for clarity. Additional processing that we mention is applied to phase function before simulation so that we can use fewer streams and not suffer a severe loss of accuracy in resulting radiance.

p. 4118 l. 4–5: The incorrect location could come from a retrieval bug. How can you exclude this possibility?

In this particular case we know that there was no retrieval bug by simple examination of incoming source model profiles. The cloud the model presented to simulator was a thin, high cloud in upper atmosphere with low cloud top temperature and pressure. We know that the actual cloud seen by MODIS was not likely to be a high cloud because it had no signature in 1.38μm channel and was not visible in any of the absorbing CO_2 channels that are used to register such clouds. Therefore we have some confidence in result in this case.

Of course a retrieval issue is always a possibility at any time. This is one of the reasons why we've built this simulator. From the remote sensing viewpoint we can always go back to the source model data, check just where exactly a particular cloud formation was located and remedy any potential retrieval issues that might arise.

We have added some clarifying text for this comment.

p. 4118 l. 23–24: Please indicate the geographical area of this granule.

We have done so. Thank you.

p. 4118 l. 27: Please explain SWIR here or refer to caption of Fig. 5.

We have expanded the acronym. Thank you.

p. 4119 l. 7–8: You are explaining the black stripe in Fig. 6, please *refer explicitly to it in the text. Why is it visible only in the simulated data?*

The operational MODIS cloud mask MOD35 product is having trouble performing cloud detection on simulated data. We have added explanation and some discussion to that effect.