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**Geoscientific
Model Development
Discussions**

Interactive comment on "Evaluation of ice and snow content in the global numerical weather prediction model GME with CloudSat" *by* S. Eikenberg et al.

Anonymous Referee #1

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The study presents a comparison between the GME model and CloudSat observations (reflectivities and retrievals of ice water content). The topic of the study is important for the evaluation of models' microphysical schemes and their impact on the hydrological cycle. The paper is generally clearly written, although the methodology is not properly explained. I reccommend the paper to be published after the authors adress the issues below.

GENERAL COMMENTS

The methodology needs to be explained in more detail. I think Section 4 needs to be re-written. It is not clear to me how the Cloudsat data processing is done (Section

4.1). Also, the model-to-observation approach needs to be expanded (Section 4.2). I would suggest to put all the technical details of the CloudSat obs and Quickbeam in section 3 (changing the title of the section). Then, section 4 would focus only on the methodology and data processing aspects. Also, some of the criteria used to filter the data (section 4.3) are not explained or justified. Plese see specific comments below.

The IWC retrievals from CloudSat are subject to large errors, whose impact in the comparisons should be discussed in the paper. For instance, the partition between IWC/LWC in the CloudSat retrievals is somewhat arbitrary: all ice for T<-20C, and a linear partition in between -20 and 0. What's the model ice fraction (with respect to total condensate) in the mixed-phase region? Is it very different from the CloudSat assumptions?. The only-ice and only-liquid cloudsat retrievals can be used to see the sensitivity of the results to the liquid/ice partition assumed in the the retrievals.

The authors have made an effort to present the evaluation of the representation of microphysical processes in the context of improvements of precipitation skill. That is a strong point that is sheldom made in evaluation papers, as it shows how the evaluation exercise presented here can be integrated in the model development cycle. This is worth exploiting, and I'd encourage the authors to link the improvements that they see in the evaluation of the microphysical scheme with the improvements in the skill scores (i.e. why the changes observed in IWC give a better precipitation skill?), even if it is only done in a speculative fashion. This will add interest to the paper.

Figures are of godd quality, but adding titles to each plot and legends will make their interpretation easier, without having to refer to the caption to know which plot is which.

SPECIFIC COMMENTS

- P424, L9-21. Not all readers will be familiar with these scores. Some extra information would be helpful. Why are they chosen? What do they represent? How should they be interpreted?

- P424, L24. CloudSat orbit period is ∼99 minutes. It is also worth mentioning that the orbit is sun-synchronous, with constant local solar time overpass for a given latitude band.

- P425, L18. I think "A priori profiles of temperature" is not accurate. My understanding is that the ECMWF temperature profiles are used to provide a priori profiles of the parameters that define the PSD. The temperature profile is not retrieved, it is used as ancillary information.

- P426, L18-19. "a moving average is applied onto the CloudSat CPR data". A more detailed explanation of the data processing is needed.

1) Is this moving average applied to IWC, reflectivity or both datasets?

2) What's the lenght of the moving average?

3) If this moving average is applied to the reflectivities, how is it computed? Are the reflectivities linearly averaged in dB space?

4) How are the thresholds (for Z and IWC) accounted for in the moving average? Are these thresholds applied before of after the moving average?

5) What's the resolution of the dataset after the moving average is applied? Do you keep the original resolution or reduce the resolution to the length of the moving average?

- P427, L5-10. Details about Quickbeam are a little bit distracting. Better placed in section 3.

- P427, L10-20. These lines discuss Figure 2, but they do not provide details on the methodology. Does the GME model account for subgrid variability? If it does, is this included in the model-to-satellite approach? How is IWC defined, is it the grid-box mean or it is the in-cloud mean? This needs to be explained and linked with the cloudsat data processing using a moving average.

- P428, L1-6. It's true that the forward modelling has many uncertainties (e.g. ice crystals modelled as soft speheres). However, forward models that use similar assumptions are used in the retrieval algorithms, and therefore these uncertainties are also present in the retrievals.

- P428, L8-18. Why criteria (2) and (3) are diagnosed from model output but aplpied also to CloudSat? This might be justifiable in the extratropics, where the meteorology is dominated by large-scale systems and the data assimilation system will help constrain the model's meteorology. Is there any evidence that model clouds and cloudsat clouds are correlated in space and time in the tropics? If that's not the case, then this filtering applied to CloudSat will operate as a random filter. After applying all the criteria, ∼75% of the samples are thrown away. Also, figure 4 suggests that in the tropics, the IWP is reduced by one order of magnitude, which implies that 90% of the condensate is thrown away in the comparison in that region. This seems too agressive. A total column attenuation of 3dBZ or more can be caused by absorption by water vapour in moist atmospheres, which can be a problem in tropical regions. Some extra analysis is required to justify the application of these criteria.

- P429, L10-11. I don't think that statement is correct. The thresholds applied to the model and obs (-26dBZ,0.001gm-3) ensure that any model cloud included in the comparison would be observed by cloudsat should it exist. In fact, figure 3f shows that the GME reflectivities of the highest clouds (T<-50) are well within the cloudsat sensitivity limit.

- P429, L20-30. "IWC in the tropics is largely connected to small scale events, which the microphysics scheme is not able to capture due to model's resolution". This is confusing. Figure 4 shows that the convection criterion substantially reduces the IWP in the tropics, which means that the model is capable of producing IWC at small scales. If you mean that the model is not able to produce IWC in small scale events, then I think this is incorrect. However, you may want to imply that the microphysics scheme in the GME model is not connected to the convection scheme (is this correct?). This

information is relevant as it is related to the main aim of the paper, and it also links with the justification of the filtering criteria. If the objective of the study is to evaluate the new mycrophysical scheme, then throwing away all the ice that is not treated by that scheme is fine. If the objective is to evaluate the model's IWC (as the title suggests), then the analysis should include as much condensate as possible.

- Following on from the previous comment. In the tropics, the selection criteria seem to be reducing more the IWP for cloudSat than for the model, why is that?

- P430, L21-22. "GME1007 reproduces the shape of the distribution of CloudSat very well". This seems an overstatement to me. It reproduces the shape better than GME, but there are still large differences with respect to CloudSat.

- P430, L23-24. This may be true for -30<T<-10, but be aware that the IWC scale is logarithmic, so small differences (even just one-bin shift) can be substantial.

- P431, L1-2. I find this sentence confusing.

- P433, L1. " the choice would be the model-to-observation approach". This statement is not justified based on the results presented in the paper, as the majority of the analysis is done using the observation-to-model approach. I'd suggest to emphasise the complementariety of both approaches, and the need to apply a consistent filtering.

FIGURES

- Fig 2. Why the number of samples are so different between the model and cloudsat? The model data are lower resolution than cloudsat, so I would expect Nmodel<Ncloudsat.

- Figures 2,3,4,5,8. Please add titles to each plot.

- figures 4,6,7. Please add a legend in one of the plots.

Interactive comment on Geosci. Model Dev. Discuss., 4, 419, 2011.