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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.

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We thank the reviewer for the useful comments. We feel that the majority of the comments are due to either unclarities concerning the data processing and the criteria, or the fact that the evaluated model IWC is grid scale only. We have made an effort to improve the formulations in the respective paragraphs, as described in detail below.

General comments:

1. **Reviewer comment (RC)**: 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

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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. Please see specific comments below.

Author Reply (AR): Section 4.1 has been modified according to the specific comments, see below.

We understand how the information on QuickBeam in Section 4.2 might be a little confusing. Yet, we think that is the best-possible place to put it. It does not belong to Section 3, as suggested, since this is dedicated to CloudSat and its retrieval. We feel that in any other section than 4.2, it is even more distracting. As one of the tools we apply, it belongs in the methodology Section 4.

The criteria Section 4.3 has been modified according to the specific comments see below.

2. **RC**: 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 $T<-20^{\circ}C$, and a linear partition in between -20 and 0.

   AR: Exactly this is the purpose of our Fig. 2, which illustrates the non-uniqueness of converting the measurement Z to the model variable IWC. Variations in the particle size distribution cause the large spread and lead to the largest retrieval error. Another error (but difficult to assess) is caused by the partitioning between liquid and frozen phase, as stated in Section 3 of our manuscript where the retrieval is described. In Section 4.2 we stated that this may lead to a false estimation of IWC.

   RC: What's the model ice fraction (with respect to total condensate) in the mixed-

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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 retrievals.

AR: The ice fractions of CloudSat, GME, and GME1007 were calculated for the whole period with and without our criteria and limits, and are displayed in the attached Fig. 1. The three data sets aggree on the mixed phase occuring mostly between 0 and -40° C, though both model versions see the transition zone at higher temperatures than CloudSat. Note that our criteria are able to reduce much of the noise and improve the agreement with CloudSat, especially at cold temperatures for GME and at warm temperatures for GME1007.

3. **RC**: 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 seldom 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.

   AR: We agree that it is most desirable to enhance the investigations in improved precipitation skill. For this, high-resolution ground based precipitation is needed. So far, these data are only available to us for Germany. Besides of being diagnostic vs prognostic, a major difference between the two microphyiscs schemes is the treatment of the particle size distribution of snow. The prognostic scheme uses a more sophisticated parameterization of the snow intercept parameter based on measurements of Field et al. (2005). Using this empirical relation yields smaller snow crystals at colder temperatures, which slows down aggregation and sedimentation, but increases depositional growth. Aggregation at cold temperatures

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is also reduced by a temperature dependent sticking efficiency in the prognostic schemes. These modifications are discussed in Seifert and Crewell (2008) as well as Baldauf et al. (2011).

Using these more sophisticated parameterizations makes little sense in a simple diagnostic scheme, because such a diagnostic scheme cannot represent the residence time of snow in the atmosphere explicitly anyway. Therefore these modifications of the microphysical process parameterizations were never introduced in the old GME microphysics. Because the new prognostic scheme is much more physically-based than the old diagnostic scheme, DWD has not spent it's limited resource on understanding why exactly the older simpler scheme is inferior.

4. **RC**: Figures are of good 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.

   AR: Figures have been changed accordingly, see Figures section below.

Specific comments:

1. **RC**: 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?

   AR: Paragraph has been modified to: "*... is undertaken. The frequency bias (FBI, Eq. 3), the ratio of the frequency of forecasted to the frequency of observed events, indicates whether the model over- or underforecasts precipitation events. The equitable threat score (ETS, Eq. 4), the fraction of hits adjusted for hits expected by chance, indicates how well forecasted events correspond to observed events. Figure 1 shows ...*"

2. **RC**: 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.

AR: The sentence has been modified to: "*As a part of the polar-orbiting, sun-synchronous A-Train, CloudSat has an orbiting time of 1.5 h, with a constant local solar time overpass at a given latitude band.*"

3. **RC**: 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.

   AR: We agree. The sentence has been modified to: "*The a priori profiles, dependent on temperature (provided by European Centre for Medium-Range Weather Forecasts, ECMWF) and reflectivity factor, are used as initial values and help constrain the retrieval.*"

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

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

   (b) What's the length of the moving average?

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

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

   (e) 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?

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AR: We agree that Section 4.1. might be a bit too scant. It has been modified to: " ... *with the nearest neighbour technique. Due to the coarser resolution of the model, this means that one model profile is assigned to several adjacent CloudSat profiles. ... Additionally, an along-track 37-profile moving average is applied to all CloudSat data to take the coarser horizontal model resolution into consideration. The original horizontal resolution of the CloudSat data is maintained, but by applying the running mean clouds in the observations become broader and in-cloud reflectivity maxima are extenuated. After this pre-processing, in order to account for instrument and retrieval algorithm sensitivities, only data (from model and observation) which are firstly within the CloudSat CPR sensitivity range and secondly deemed trustworthy are included in the investigations, i.e.,...*"

5. **RC**: P427, L5-10. Details about QuickBeam are a little bit distracting. Better placed in Section 3.

   AR: See AR to general comment 1 above.

6. **RC**: P427, L10-20. These lines discuss Fig. 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 is it the in-cloud mean? This needs to be explained and linked with the CloudSat data processing using a moving average.

   AR: The GME is (currently) operating the Tiedtke-convection scheme which does not produce any subgrid contributions of cloud ice or snow. Only for the radiation scheme, a convective cloud cover is diagnosed to describe the effect of subgrid cloud ice (no snow). The IWC calculated by the gridscale microphysics is described as a gridbox mean. We agree that this is information essential to the reader. It has been included in Section 2: "*Whether this improvement in terms of surface precipitation is connected with improved representation of grid-scale ice is investigated in this study.*" The abstract has been modified to "*The present*

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study evaluates the global numerical weather prediction model GME with respect to the grid-scale parameterization of frozen particles, both ice and snow, ...". The introduction has been modified to "*The representation of grid-scale ice in the two model versions is evaluated...*". Section 2 has been modified to "*Four hydrometeor classes are implemented in the model: Cloud ice and water, snow, and rain. They are avilable as grid-scale parameters.*".

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

   AR:We completely agree, which is why we undertake both approaches. Each has its pros and cons.

8. **RC**: P428, L8-18. Why are criteria (2) and (3) diagnosed from model output but applied 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, Fig. 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 aggressive. A total column attenuation of 3 dBZ 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.

   AR: Precisely because the model has problems in getting time and space of a cloud right, we applied the filtering. With it we can ensure that the remaining data are for the most part correlated. The large reduction of IWP in the tropics takes place for the CloudSat data, not the GME data, see AR to specific comment 11 below. In Fig. 4 the attenuation criteria is not displayed, since this figure is in the model-space, i.e., shows IWCs not reflectivity factors.

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

   AR: The minimum detectable reflectivity of CloudSat does not correspond to an exact minimum detectable IWC value. Rather, it depends on the particle size distribution parameters. Furthermore, "Bins with radar signals near the noise floor may or may not be included in retrieval calculations depending on the cloud mask value..." (Austin et al., 2009). CloudSat does detect a certain number of small reflectivity factor values at the lower sensitivity limit above -50°C (Fig. 3e). However, the retrieval obviously does not produce any small IWC values from this. To be more precise, those IWC values that the retrieval does produce, are spread over such a wide IWC range, that no maximum in Fig. 3a is discernible below -50°C. For the model (Fig. 3b), one would be able to extend the maximum to temperatures below -50°C and one would find a reasonable enough reflectivity factor maximum, too (Fig. 3f). The statement has been modified to the following: "*... the maximum of the frequency of occurrence reaches up to lower temperature regimes than for CloudSat, most likely because the CloudSat retrieval does not produce a clear IWC maximum from the measured reflectivity factors in these heights, but rather produces a broad range of IWC values.*"

10. **RC**: 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 re-

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duces 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 microphysical 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.

AR: In the tropics, cloud ice can form through smaller and larger scale processes. The small-scale processes cannot be resolved by the model, but by the observations, which is why tropical IWP from CloudSat is larger than model IWP in this region. In fact, one would expect the global maximum of IWP to be located in the tropics, which is not the case for the model.

However, the model does have some IWP in the tropics and this is the part which is contributed by the larger scale events. Since the convection criterion still eliminates so much IWP, these events are mostly of convective nature (e.g., mesoscale convective systems in tropical West Africa, which are often initiated through larger scale convection).

Convective tropical events are often highly variable in time, and are the most difficult events for a model to predict correctly in both space and time. Therefore we deemed it more sensible, to exclude these fast-changing events completely from the comparison and rather concentrate on stratiform events for the time being. First and foremost, our goal was to achieve robust features, rather than giving an all-encompassing picture.

In GME the convection scheme and the large-scale microphysics scheme interact only through the grid-box mean, i.e. the resolved variables. In contrast to the

IFS or COSMO model, there is no detrainment of cloud liquid water or cloud ice from the convection scheme, only detrainment of vapor. This is another reason why we focused the evaluation on large-scale resolved spatial structures of IWC, because we know that GME has fundamental deficiencies in representing the interaction of deep convection and large-scale clouds. In the new global model, ICON, which is currently being developed, we will improve the representation of these processes.

We have changed the respective parts of our manuscript to better reflect that our focus is indeed on the evaluation of the microphysics schemes and the resolved IWC (see AR to specific comment 6 above for the modifications in the manuscript). Nevertheless, our analysis also sheds some light on the more general question of global IWC and SWC distributions, especially the fact that the global IWC is dominated by snow. Here we benefit from the fact that snow is part of the resolved IWC in the prognostic scheme, while it is sub-grid in almost all climate models.

11. **RC**: 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?

    AR: The GME misses much of the IWC linked to smaller scale clouds, because of its low resolution. CloudSat on the other hand does detect the IWCs of the smaller scale clouds. The selection criteria are designed to kick out those smaller scale clouds, which the model is not capable of capturing due to its design, and therefore mainly reduce the CloudSat IWC.

12. **RC**: 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.

    AR: Sentence modified to: "*In general, GME1007 reproduces the shape of the distribution of CloudSat better than GME...*".

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13. **RC**: 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.

    AR: Sentence modified to: "*... in the mid-latitudes and polar regions. Here, the peak of maximum frequency of occurrence is located at roughly the same IWC, shifted by two bins (i.e., 0.02 gm^{-3}) at the most.*"

14. **RC**: P431, L1-2. I find this sentence confusing.

    AR: In the tropics, according to Fig. 6, GME1007 generally overestimated IWC, but underestimates the occurrence of large IWCs. According to Fig. 4b, the overestimation of IWP in the tropics is not as large as in other regions, note the logarithmic scale. We hypothesize that the underestimation of large IWCs partly compensates the general overestimation, resulting in the not-so-large IWP overestimation in this region.

15. **RC**: P433, L1. "*the choice would be the model-to-observation approac*". 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 emphasizing the complementarity of both approaches, and the need to apply a consistent filtering.

    AR: Though it is only the zonal means which we only show for one of the two approaches, we see your point. Still, for the case of limited resources, we find it necessary to vote for one of the two approaches, the one we deem more trustworthy.

Figures:

1. **RC**: Figure 2. Why are the numbers of samples so different between the model and CloudSat? The model data are lower resolution than CloudSat, so I would expect $N_{model} < N_{CloudSat}$.

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AR: The model does have a lower resolution than the observational data in the first place. But after the nearest neighbour interpolation of the model data onto the CloudSat track, as written in Section 4.1, the model data are on the same grid as the CloudSat data; one model profile is used a couple of times, because the model has a lower resolution than CloudSat. Due to this, clouds are generally larger (i.e., broader) than in the observations. With the running mean applied to the CloudSat data alone, we seek to decrease this discrepancy, and indeed the clouds become broader in CloudSat. But even with the running mean, clouds in the model remain broader still. That is why $N_{model} > N_{obs}$.

2. **RC**: Figures 2, 3, 4, 5, 8. Please add titles to each plot.

   AR: Titles have been added.

3. **RC**: Figures 4, 6, 7. Please add a legend in one of the plots.

   AR: Legends have been added.

References:

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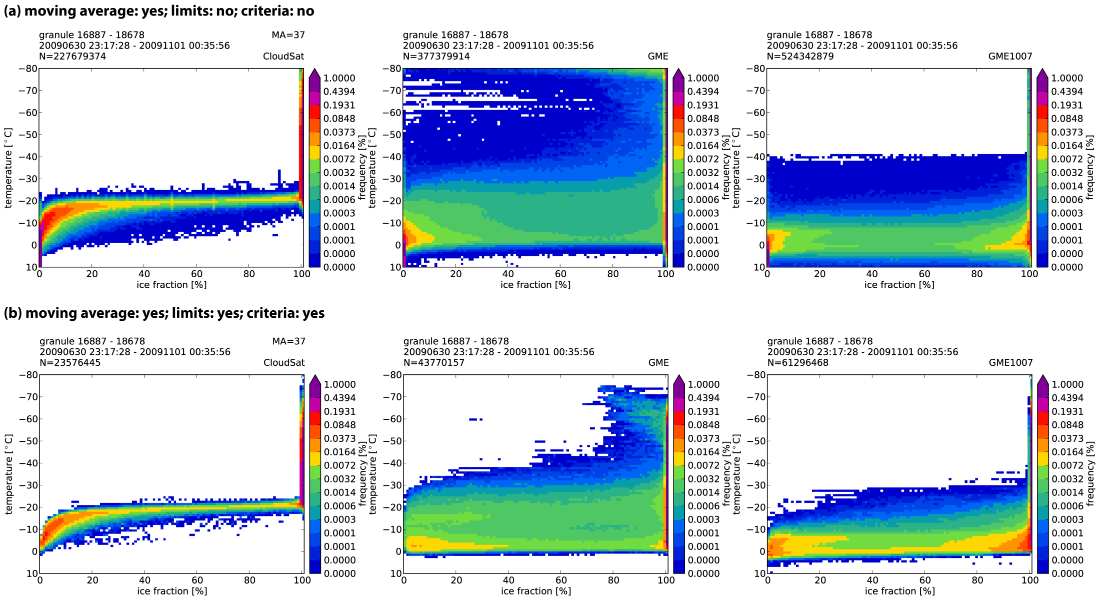

Fig. A: Ice fraction as a function of temperature for 1 July 2009 to 31 October 2009. Left: CloudSat, middel: GME, right: GME1007. Top row (a): no criteria and no limits (according to CloudSat sensitivity range) applied. Bottom row (b): all criteria and IWC and limit applied (temperature not applied). Each data set is normalized with its total number N of included pixel.

Fig. 1.

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