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Geoscientific Model Development Discussions

Interactive comment on "Numerical framework and performance of the new multiple phase cloud microphysics scheme in RegCM4.5: precipitation, cloud microphysics and cloud radiative effects" *by* Rita Nogherotto et al.

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First, we would like to thank all the reviewers for their careful reviews and constructive comments, which helped to improve the quality and clarity of the paper.

Anonymous Referee #2

The authors introduce a more sophisticated and complex cloud parameterization in the regional model RegCM4, which allows a more realistic representation of the microphysics processes than the standard scheme. After describing the new scheme, they evaluate the representation of precipitation, clouds, and TOA radiation against

observations for the new and old scheme, using satellite simulators for the cloud part. While the improvement of precipitation is unclear, the representation of clouds is clearly better using the new cloud scheme, particularly in the upper levels, leading to an improvement of the simulation of radiation at TOA. This paper is perfectly within the scope of the journal and well written as well as clearly presented. The topic is of particular interest as it shows how a more physical representation of microphysics in models can lead to a better representation of cloud/radiation when compared to observations. Besides, the authors show once again how a multi-observational dataset approach help understanding the model's biases, while using a consistent and solid method to compare model and obs via the simulators. However, the problematic in the introduction could be substantially improved and some information regarding the observations in the manuscript is missing. In addition, the authors failed to explain the reasons behind some of their results, which make me think the paper need a major revision before being published. My detailed comments are listed hereafter.

Main concerns:

1) Although the authors used the COSP package, they didn't describe which version of the package is used. Depending on the version, they might have used the new CALIPSO cloud phase diagnosis (ver 1.4), which allows distinguishing ice clouds from liquid clouds. This would have been particularly interesting in this study, i.e. the ice-to-liquid ratio vs. T or z. Even though, the COSP version used here is anterior to 1.4, the authors should consider using the vertically resolved cloud fraction of CALIPSO to assess their model. It would give us more information about how the model represents the vertical structure of clouds (better than only 3 vertical layers, low mid and high).

We added in the introduction: "...the new parameterization is also assessed using the recently available Cloud Feedback Model Intercomparison Project (CFMIP) Observational Simulator Package (COSP, version 1.3.2) (Bodas-Salcedo et al., 2011)". We have used COSP version 1.3.2 because the version 4 was not available when we started our work. We decided to choose the three layer cloud representation because we wanted to compare our results with those by Franklin 2013 and because we were specifically interested in the representation of high vs. low clouds. Future studies will be carried out using the vertically resolved cloud fraction from CALIPSO and the other simulators (MODIS and CloudSat).

2) The introduction misses some important references to stress the importance of having a more realistic representation of microphysics in climate models and what has been already done in the field as well as in the observations. For example, how cloud phase determination affects the GCMs/RCMs, does it really matter? Cesana et al (2015) (also Komurcu et al (2014)) showed that the climate models particularly under-estimate the super-cooled liquid clouds compared to observations; and a more complex microphysics helps reducing the problem. Tan et al (2016) recently showed that better representing those supercooled liquid clouds (constrained using CALIPSO) might drastically change the equilibrium climate sensitivity of climate models. Moreover, there has been a lot of work on the observed cloud phase that is not mentioned here. It could be helpful for the reader to know that. For example, liquid and ice particles may co-exist for hours (Korolev et al., 2003) and sometimes during days (de Boer et al., 2009). Also, observations showed substantial presence of supercoold liquid at temperature as low as -35°C, in agreement with insitu observations (Cesana et al., 2016).

We modified the Introduction in the following way:

"Simpler microphysics schemes treat the cloud water prognostically and precipitating water diagnostically (e.g. Rotstayn, 1997; Pal et al., 2000). Observational data show that between -23 °C and 0 °C the occurrence of supercooled water is not negligible (Matveev, 1984), and liquid and ice particles can co-exist for hours and sometimes even days (e.g. Korolev et al., 2003; de Boer et al., 2009). Often cloud schemes diagnose the fraction of cloud water in the ice phase based on the local temperature (e.g. DelGenio et al., 1996). The diagnostic partitioning of cloud water into the liquid and ice phases assumes implicitly that processes within the cloud are fast compared to the

model time step, i.e. that the cloud variables are always in equilibrium. Therefore, a diagnostic representation is unable to describe the temporal variability and evolution of mixed-phase clouds and a prognostic treatment of cloud ice and water is necessary to represent the microphysical processes of the two phases (including their contrasting sedimentation rates). More complex microphysics schemes have been therefore introduced to treat separately the cold and warm cloud microphysics by solving prognostic equations for cloud liquid water and ice (e.g. Fowler et al., 1996; Lohmann and Roeckner, 1996). These schemes are especially important as climate models approach resolutions at which cloud physics processes, including convection, need to be explicitly described without the use of parameterization schemes (e.g. Prein et al. 2015). Recently, several studies have illustrated the importance of using a more realistic representation of cloud microphysics in climate models. For example, Cesana et al. (2015) and Komurcu et al. (2014) showed that climate models tend to underestimate the supercooled liquid clouds and models that prognose separately the liquid and ice mixing ratio give a better representation of cloud properties."

3) In some part of the manuscript, the authors do not explain the reason of the simulated bias. I think of the low cloud problem, which affects the TOA radiation in Sect. 3.2 and 3.3

Done (see the following points).

4) Finally, not enough details are given regarding the observations used in the manuscript. The authors should mention where they got it and what is the resolution and time period they used.

Done (see the following points).

Minor comments:

Abstract

Line 5-> five Done.

Line 8-10: A little bit confusing as not 10-year are used for COSP comparison. Also, I would not say the COSP simulator but either satellite simulators or the full definition the cloud feedback. . . package.

We modified the following text: "Various fields from a 10-yr-long integration of RegCM4 run in tropical band mode with the new scheme are compared with their counterparts using the previous cloud scheme and are evaluated against satellite observations. In addition, an assessment using the Cloud Feedback Model Intercomparison Project (CFMIP) Observational Simulator Package (COSP) for a 1-yr sub-period provides additional information for evaluating the cloud optical properties against satellite data." Introduction

Line 54: Please define COSP.

Done.

Section 2.

The authors should consider doing a small summary of the new scheme at the beginning of Sect. 2.2 as it is done for the old scheme in Sect. 2.1. It would highly help readers not expert in model development and readers in general to identify the main changes.

We think that the beginning of Section 2.2 already contains a summary of the scheme: we have modified the text at the beginning of Section 2.2 from "The model includes four hydrometeors..." to "The scheme includes four hydrometeors..." as we think this was misleading.

Line 256: 5 -> five

Done.

Sect. 2.2.2: The authors state that one year of simulation might be enough to draw solid conclusions, which I also believe. To strengthen this statement, though, the authors

might use CMIP5 model outputs that use the same core as RegCM4 (e.g. ecearth) and show that the inter-annual variation of COSP fields is smaller than the model-obs bias.

As we have already shown in the response to comment 16 of reviewer 1 and in the response to the comment for line 327 below, the clouds for the year we selected is quite representative of the long term average cloud cover in the ISCCP dataset. It is unclear to us what the suggestion by the reviewer is concerning the use of CMIP5 data, since the RegCM4 is quite different from all CMIP5 models so that comparison with the CMIP5 output would probably not help much in this regard. Finally, we note that in the literature the use of one year is considered to be sufficient for a first order evaluation of model clouds.

Section 3 Did the authors use the cfmip-obs ISCCP dataset, which are designed to be consistent with the simulator? If so, please mention it and refer to the website.

We modified the text in the following way: " The evaluation of total cloud cover is carried out using the GCM simulator-oriented International Satellite Cloud Climatology Project ISCCP cloud product (Pincus et al., 2012), which was prepared to facilitate the evaluation of the model simulated clouds within the framework of the Cloud Feedback Model Intercomparison Project (http://climserv.ipsl.polytechnique.fr/cfmip-obs). Data are averaged over the JJA and DJF 2007 seasons during the daytime, at a horizontal resolution of 2.5°x2.5°."

Tab. 2: It's a detail but SUB results should appear in the left column rather than the mid column to be consistent with the order to which it appears in the figures: SUB ! MIC!OBS

Done.

Figure 3: A difference and/or a bias plot in Fig. 3 might help identifying the improvements. A correlation between obs and simulation could be added to tab 2 and I bet it

would be higher for the MIC scheme, highlighting the fact that even though the mean is worse in MIC than in SUB, using MIC scheme improves the distribution of clouds in the model.

We added the correlation values in Table 2 and modified the text as follows: "In general, both schemes capture the horizontal distribution of clouds over the band domain in both seasons, with maximum cloud cover over the ITCZ and the mid-latitude storm track regions of both hemispheres. However an analysis of the spatial correlation between the two schemes and the observations reveals that the new parameterization improves the horizontal distribution of clouds (Table \ref{tab:tot}): while the SUB scheme tends to overestimate the magnitude and extension of total cloud amounts across the ITCZ, the MIC scheme shows a slight underestimation In addition, use of the MIC scheme improves the stratiform cloud cover between 30 and 45° S, yielding higher spatial correlation values compared to those obtained with SUB."

Line 325: The authors might add “GCM-oriented” CALIPSO estimates to be more specific.

Text modified: "The GCM-Oriented CALIPSO Cloud Product GOCCP data (Chepfer et al., 2010), 2° x 2°, are used for the model evaluation as they are designed for comparisons with output from the CALIPSO satellite simulator."

Line 326: Please use a more recent reference for CALIPSO: Winker et al., 2010, doi:10.1175/2010BAMS3009.1

Done.

Line 327: Which version of CALIPSO-GOCCP did you use and what about the resolution and the time period? Judging from the figure, it seems to be only one season and 1degx1deg grid. I would strongly encourage the author to at least pick the 2x2deg grid and averaged over all available seasons to smooth the noise. As mentioned before, the inter-annual variation is lower than the mod-to-obs bias anyway and should change

the pattern of the bias. The 1x1deg grid is also very noisy because of the poor overlap due to CALIPSO polar orbit.

We used the 2x2 deg grid (added in the text) and averaged over the same season simulated by the model, following Franklin (2013). In Fig. 1 we show that the average cloud amount for the available climatology (JJA 2006-2010) does not substantially differ from the chosen season (JJA 2007), suggesting that the inter-annual variation in global mean high, medium and low total cloud fractions is not large and it is reasonable to choose only one season:

Line 346: liquid droplets rather than cloud droplets.

Done.

Table 3: same as for Tab 2, SUB ! MIC ! OBS and maybe the authors should adding correlation numbers.

Done.

For MISR, same questions as for ISCCP and GOCCP, are these from CFMIP-obs? And what is the resolution?

The following text was modified: "An even more accurate analysis of cloud vertical distribution can be carried out with the use of the Multi-angle Imaging SpectroRadiometer MISR (Muller et al. 2002) data. MISR uses nine cameras providing images with approximately 275 m sampling in four narrow spectral bands, spanning much of the angle range over which cloud reflectivity varies. This leads to a more accurate retrieval of albedo than the use of a single camera. Naud et al. (2002), however, found that in the case of multi layered clouds MISR often “sees” through the thin upper level clouds and mostly refers to low level cloud layers. The MISR retrievals can be processed to produce joint histograms of Cloud Top Height (CTH) and Optical Depth (OD) used specifically for a comparison with the COSP output and available on the CFMIP observational dataset website. To compare with the MISR retrievals, we postprocessed the

RegCM4 data with the MISR simulator described in Marchand et al. (2010)."

Line 367-369: May these low clouds be the shallow cumulus cloud, implying that the RegCM4 model struggle to represent the transition from strato to shallow cumulus clouds, as many other models? Besides, it is in agreement with the few too bright problem, too few low clouds but optically too thick (e.g. Nam et al., 2012, doi:10.1029/2012GL053421)

We added the following text: "While an underestimation of low clouds is a common problem in climate models (e.g. Nam et al., 2012; Zhang et al., 2005) a reason for the overestimation of thick clouds found here may reside in the fact that even if in real systems only part of a 100 km grid area experiences a strong upward motion, the mean vertical velocity for the whole model grid box is upward, leading to an updraft for the entire gridbox. A reason for the overestimation of low optically thick clouds can be related to the coarse horizontal model resolution (100 km) which does not resolve surface-heterogeneity, topography and shallow mesoscale circulations. Future studies will evaluate the model performance at higher horizontal resolutions." Section 3.3

Did the authors use the CERES-EBAF data, specially designed for model evaluation? Please, clarify and define the resolution and time period.

We used the CERES ERBA-like Monthly Geographical Averages (ES-4) observations (Wielicki, 2011), with an horizontal resolution of 2.5° x 2.5° (added in the text) because we wanted to compare our results with those observed in the same season. CERES-EBAF are only available until 2005. While the explanation for the CRFlw is straightforward, the upper cloud issue does not explain all of the CRFsw bias, and the authors do not refer to the other reasons of the bias. As mentioned before, the low clouds have been shown to be mostly too reflective in many GCMs for quite a while now (e.g. Nam et al., 2012; Zhang et al., 2005). It seems to be also the case for RegCM4. The large bias in the CRFsw remains even in region where the upper and lower clouds are well reproduced by the model (e.g. around 45°S). Could

this be because of the thin low clouds missed by your model as shown by the MISR simulator analysis? Could you i) locate these thin low clouds on a map and ii) propose an explanation of why they are so optically thick? We modified the following text: "The excessive upper level cloud cover in the SUB run yields too much SW reflection and therefore the domain-average SW values are about 40 W m$ˆ{-2}$ lower than observed. The MIC scheme, by reducing the upper level cloud cover, reduces the upward SW flux and therefore yields values closer to observations (domain average bias of 10 W m$ˆ{-2}$). However a substantial bias still persists in areas where both high and low clouds are well represented (e.g. around 45° S). This bias can be attributed to the underestimation of thin low clouds as shown by the MISR simulator analysis (Figure \ref{misr}). Even if the overestimation of low cloud reflectivity is a common problem for many GCMs \citep[e.g.][]{nam2012too,zhang2005} a reason for our overestimation of low optically thick clouds can be related to the coarse horizontal resolution (100 km) which does not resolve surface-heterogeneity, topography and shallow mesoscale circulations. Future studies will evaluate the model performance at higher horizontal resolutions." When looking at the full CRF, i.e. the sum of CRF$_{SW}$ and CRF$_{LW}$ (Figure \ref{fig:cretot} and Table \ref{tab:cre}), we see that essentially the model biases tend to compensate, yielding values close to each other for the two schemes and not far from observations (although on a domain average the MIC is still closer to observations by a few W/m2). In some tropical monsoon regions the longwave gain in the SUB scheme appears to be larger than the shortwave loss, leading to an overall heating which is less pronounced in the MIC scheme.

Line 418: cirrus instead of stratocumulus.

Done

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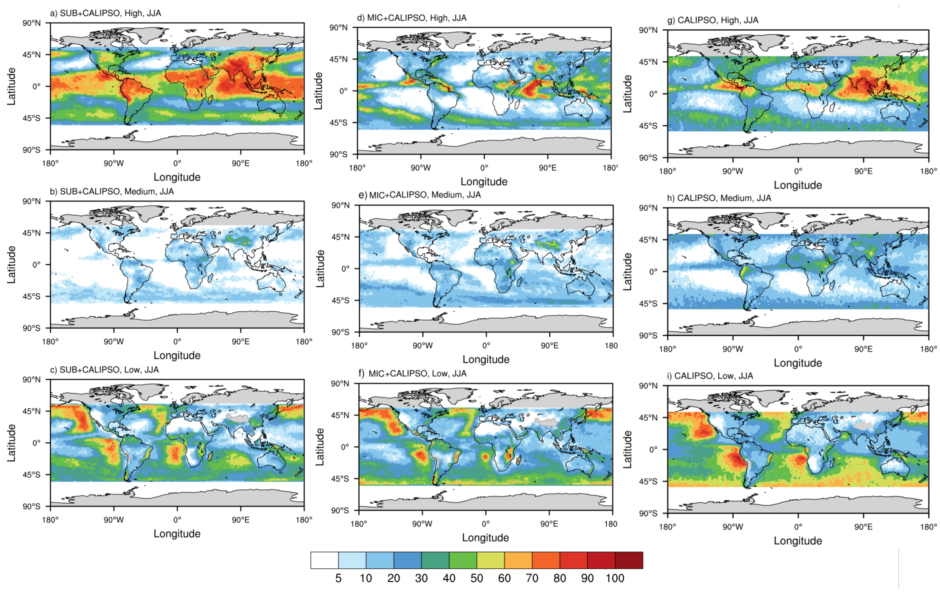

Fig. 1. Same as Fig 4. in the paper with modified panel g) h) and i) that now show observations for the available climatology (JJA 2006-2010).