1. Decision

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.

2. 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).

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).

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

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.

3. Minor comments:

Abstract

Line 5: five

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.

Introduction

Line 54: Please define COSP.

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.

Line 256: 5 ➔ five

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.

Fig. 2 is very difficult to read in its present form. The authors should consider adding either the bias compared to observations or may be just the difference between the two experiments to help the reader locate the differences.

Moreover, no explanation is given for these differences. Could the authors at least guess the main reason for this slight improvement (line 296)?

Time period and resolution of the obs?

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.

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

Figure 3: Again, 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.

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

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

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.

Line 346: liquid droplets rather than cloud droplets.

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

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

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)

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.

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?

Line 418: cirrus instead of stratocumulus

4. References

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de Boer, G., Eloranta, E. W. and M. D. Shupe (2009), Arctic Mixed-Phase Stratiform Cloud Properties from Multiple Years of Surface-Based Measurements at Two High-Latitude Locations, J. Atmos. Sci., 66:9, 2874-2887, doi: 10.1175/2009JAS3029.1

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Zhang, M., et al. (2005), Comparing clouds and their seasonal variations in 10 atmospheric general circulation models with satellite measurements, J. Geophys. Res., 110, D15S02, doi:10.1029/2004JD005021.