Review of “The Impact of Cloud Microphysics and Ice Nucleation on Southern Ocean Clouds Assessed with Single Column Modeling and Instrument Simulators”

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Recommendations: Major revision

General comments :

This study compares the microphysical representations of the two schemes implemented to the ECMWF’s global model, Integrated Forecast System (IFS), and those of the observation obtained by the Macquarie Island Cloud and Radiation Experiment (MICRE). A single-column model experiment is conducted using the tendency obtained by the global model. The two schemes compared are the scheme used in IFS (IFS) and the two-momentum scheme developed and improved by the authors (MG3). Finally, the authors compared the different implementations of MG3.

The evaluation and the improvement of the cloud microphysics scheme by comparing with the observation and the other schemes are continuously demanded to achive better representation of cloud distributions and characteristics in atmospheric models. A variety of observational data can be chosen for the model evaluation. The authors clarified the uniqueness and importance of the observation used in this study for the evaluation of supercooled liquid clouds, which are common at higher latitudes.

The validity of the use of a single column model (SCM) for testing cloud microphysical schemes must be more explained. SCM is widely used but has limitations. It is generally known to be useful for testing convection whose main process is vertical mixing. If it is used for a mid-latitude weather system, horizontal advection is a main source of the tendency. I do not understand whether in this approach, the hysteresis of the cloud microphysics processes is used for the update of SCM.

This study only compared a single location. I like this methodology, but it is not well understood whether the validation at a single location is generalized to other areas. The authors should discuss how the generalization and the universality of the cloud microphysics evaluation.

The upward-pointing cloud radar is severely attenuated under precipitating or convective clouds. Radar signal comparisons shown in Figures 4 and 5 should be carefully re-examined. The authors discuss upper cloud properties, but the radar signal above 5 km altitude is not reliable. On the other hand, the numerical model results with a simulator show distinct distributions in the upper layers. I suspect that the attenuation of the radar simulator is not properly calculated.

In the sensitivity experiments in Section 3.2.3, the authors pointed out the severe dependency on the assumption of cloud fraction and sub-grid decomposition, as well as the timestep. It is generally recognized that it is not straightforward to evaluate the cloud properties of GCMs. The authors should summarize which properties can be evaluated, and the others are more dependent on the model assumptions. This type of caution should be emphasized in the introduction and the discussion sections. The authors may also note differences in the methodology from the cloud evaluation of global storm-resolving models.

Specific comments :

p. 3, L 58: The type of the radar (W-band) and the frequency must be indicated in this section.

p. 4, Section 2.2.2 and 2.2.3: Specify the method or the definition of the cloud fraction. What are the assumptions of the size distribution? I suggest the full details of the assumptions of the size distributions of all the hydrometeors should be tabulated. The results of the simulator severely depend on them.

p. 5, Section 2.2.4: What is the methodology of the treatment of the sub-columns? Some are described in a later section. This section is the right place to be fully explained.

p. 5, L 128-131: As pointed out in General comments, the result from the attenuation calculation of the radar simulator is not understandable. The authors should quantitatively show the quality of the radar simulator in terms of the attenuation.

p. 6, L153, "the formation of layers with zero fall velocity": I do not understand what "zero fall velocity" means. Are they cloud water or cloud ice, which possesses zero fall velocity?

p. 6, Figure 1: It isn't easy to distinguish the differences among the results from this figure. The authors could consider a clearer method to show the difference. A possible improvement is to show the difference from the observation for the two numerical results in separate figures.

p. 11, L251, "The median values of MG3 LWP are comparable to those for the IFS and the observational datasets.": The median value of MG3 LWP is 220, and that for the IFS is 69.8. They are not comparable.

p. 11, L267, “a greater occurrence of low reflectivity in the upper troposphere related to ice cloud”: The radar signal is highly attenuated for the upper troposphere. How reliable are the upper tropospheric clouds?

p. 11, L268, “reflectivity-height histogram”: It is generally called a Contoured Frequency Altitude Diagram (CFAD).

p. 11, L272 and after, “left” and “right”: These should be smaller and larger values, respectively.

p. 11, L274-275, “The model simulations contain a large occurrence of hydrometeors above 5 km that the surface radar would be unable to detect.”: Why so many smaller values of radar reflectivity are detected above 5 km for MG3 and IFS (Fig. 5)? This result suggests that the simulator’s calculation of attenuation is not adequate.

p. 11, L280, “This suggests that the MG3 is likely better representing this high-altitude cloud.”: The high clouds are severely affected by the model tuning. In which aspect the high cloud is better represented?

p. 14, L285, “affected by the model sub-grid representation”: It must also be affected by attenuation, I guess.

p. 14, L285-307: The description of this part illustrates the difficulty in assessing the nature of GCM clouds. When evaluating them by comparison with observations, the cloud properties in the GCMs are greatly influenced by the assumptions used in the GCMs, and thus, the assumptions themselves are evaluated in some cases. This problem illustrates the difficulty of this study and could be emphasized more. The definition of cloud cover and the subgrid model assumptions are important elements of this uncertainty. In the introduction and the discussion section, this issue, in particular, must be highlighted.

p. 14, L314-315, “the analysis shows the total low cloud cover in the simulations is reasonable”: Please show this result.

p. 15, L336-337, “There is overall more supercooled liquid water”: Please indicate where is the location of the 0-degree altitude.

p. 15, L339-340, “A common problem is a sensitivity of microphysical processes to the length of the timestep”: This serious problem is pointed out abruptly. Do the authors summarize the artificial factors that severely affect cloud characteristics? I speculate that the IFS is not so much affected by the timestep.

p. 15, L349, “supercooled liquid water (SLW)”: SLW is already defined at L337. It does not need to be spelled out here.

p. 17, L353, “by regime”: What is the regime?

p. 18, L377, “a deeper melting layer”: Where is the melting layer? How deep is the melting layer of the MG3 scheme?

p. 21, L425, “the MG3 CAM6 variable fall speed formulation”: What is the detail of the formulation of the fall speed?

p. 25, before the end of Section 3.2.3: A lot of the sensitivity results are shown. The authors should summarize the sensitivity experiments by itemizing the main results or in a table quantitatively.

p. 27, L515, “above”: Is this higher in altitude or larger in signal?

p. 27, L517, “While some of this difference is expected due to attenuation of the observed radar reflectivity that results from rain on the radar radome”: This caution must be explicitly described in the main text.

p. 27, L523-329: This paragraph explains the usefulness of the single-column model. I agree. However, I suspect that there is a limitation or caution if it is applied to mid and high-latitude areas.

p. 28, L537-539, “The sensitivity simulations show that differences in the autoconversion to rain and accretion onto rain loss processes for liquid water at sub-freezing temperatures are the primary cause of these differences, and the MG3 scheme could be adjusted to enhance the liquid loss process and reduce the LWP in deeper clouds.”: If the authors claim that this part is the main conclusion, it should be more emphasized when summarizing the sensitivity results in Section 3.