Review for “A modified parameterization of stratiform cloud microphysics for the Community Earth System Model”

The study (Pant et al.) discusses development of the stratiform cloud microphysics scheme to represent large-scale clouds in climate models. This work shows that an improved treatment of the interactions between aerosols and large-scale stratiform clouds can be achieved by better representing the secondary ice production in the model. In doing so, authors were able to obtain more realistic predictions of cloud properties using the Single-Column Atmospheric Model (SCAM6), which they validated using the observations from the MC3E campaign over Southern Great Plains.

Ice processes in climate models are uncertain. Through this work, the authors show that model predictions of cloud properties can be sensitive to both primary and secondary ice production processes, and hence, these processes could play a vital role in improving the prediction and evaluation of clouds in models. I recommend its publication in “Atmospheric Chemistry and Physics” after the following comments are addressed.

Major comments:

Line 100: The subsection “Treatment of aerosols” can be improved. Where were the biological and non-biological aerosols derived from? Are any of these sources biomass burning? CAM6 uses MAM4 (Liu et al. 2016) for the treatment of aerosol species and processes. Was this entirely replaced by the scheme provided by Phillips et al. (2009) or only some processes? What are the seven modes in the aerosol model called? Please provide additional information.

Line 271: It looks like the precipitation between May 12th and 13th is being largely overpredicted by LS24 (Figure 1a). What processes are causing that? It also seems like the initiation of the precipitation is off by a few hours in all the simulations. Do the authors know a possible explanation for that? It would be useful to add some discussion for that in the text.

Figure 4: It appears that the “no-SIP” case does not really cause any marked difference in the ice-phase cloud properties relative to the control simulation (Fig. 4). Is this because the model is not sensitive to SIP processes or is SIP physically not very important for ice-phase and mixed-phase processes? Can the authors please provide some explanation for that?

Minor comments:

Line 71: The double-moment stratiform microphysics options in CAM6 are MG1 (Morrison and Gettelman, 2008) and MG2 (Gettelman et al. 2015), and not Morrison and Grabowski (2008). Please check the citation.

Line 76: “were omitted Liu and Penner (2005)”. Typo?

Line 76: Is the citation correct? Liu and Penner (2005) studied Primary Ice Production mechanisms via homogenous and heterogeneous nucleation modes. As far as I know, they did not discuss any mechanism for SIP. Please check.

Table 1: Why are there different sigmas for each species? Do these sigmas correspond to different aerosol modes? Also, do the geometric mean diameters correspond to the sizes at which these aerosols are being emitted?

Eq. 3: Please cite Morrison et al. (2005) for the “lambda_x” parameter.

Eq. 5: How do you obtain nx,0?

Line 138: Do you mean “in-cloud droplet activation follows Abdul-Razzak and Ghan (2000)”? Wouldn’t k-Kohler theory be used to only estimate the activation supersaturation? In-cloud activation would require calculating the maximum ambient supersaturation for activation, which we cannot get simply from the k-Kohler theory. Also, is there a reason why Ming et al. (2006) was not used for in-cloud activation?

Line 255: Were the temperature and wind fields also relaxed to the observations for the SCAM6 simulations? If these are atmospheric isotherms, I suggest the plots to be made with respect to isobars or vertical height.

Line 263: What does “LS25” stand for?

Line 270: Figure 1(a) and (b) have not been separately referenced when being discussed.

Line 271: What does “LS24” stand for?

Figure 2: What are the temperatures on the y-axis? All the text and marker sizes in the plot can be increased for better readability. Furthermore, adding standard errors/standard deviations for the simulated LWC and CDNC would also be useful.

Figure 2b: It looks like the variability in CDNC from LS24 is much larger than what is being suggested by the observations. Similar variability is not very evident in the LWC data. Can the authors comment on that?

Line 320: The limit for the homogenous freezing is mentioned as -36C, but In section 2.2.7, it is mentioned as -35C. Is it the former or latter? I would suspect 1C difference at these temperatures should substantially change the INPs. Can the authors please confirm?

Line 324: What is classified as the “mixed-phase region”?

Table 4 and 6: What is an “Unconditional average”?

Table 4 and 6: How do the shortwave and longwave cloud forcing values change between all these experiments? Have they been evaluated?

Line 388: “deg”. Typo.

Line 391: I am slightly confused about the discussion regarding the ice nucleation and the distribution of the ice particle number concentrations. Homogenous freezing would be “effectively turned off” above the -35C mark where the ice particles would be greater in number in the control simulations. But shouldn’t the aerosol-induced freezing result in a larger ice number concentration at warmer temperatures? Can the authors please explain.

Line 444: I am not sure which scheme is LS25, the discussion in the manuscript only mentions LS24 throughout the writeup