Response to the Reviewer

Format: The reviewer's comments are quoted in italic

Line number in the response refers to the revised manuscript with tracked changes

Quotation in red color stands for revised/added text in the revised manuscript

We thank the reviewer Dr. Andrew Gettelman for the additional comments. Below is our response to each of the comments, and the revisions to the main text accordingly.

I think the authors have done a very good job responding to the reviews. I appreciate that they have averaged the data to be near the same scale now between models and observations, and that truncation has been applied to ice with a size threshold representing the observations. I think this will be publishable with a few further clarifications as noted below.

I still think that for cloud mass and number, you need to use in-cloud values. Unless you are also sampling zeros from the observations (i.e. the running sample includes zeros when there is not cloud). Please clarify.

Yes, we included the zero values of Ni and IWC (i.e., clear-sky condition) when averaging into coarser scales. This is why we are comparing the 100-km scale observations with grid-mean quantities from model output. We included a clarification in line 186 – 187: "When applying the running average, both in-cloud and clear-sky conditions (i.e., where Ni and IWC values are zero) are included in the averages."

For Figure 4, I don't see how truncation for sizes greater than 62.5 microns will INCREASE or DECREASE the number of ice crystals in bins larger than this. I think there is something incorrect about the truncation.

This is a very good question. We can see that the original Figure 4 in the previously revised manuscript can be confusing. In that figure, we applied new gamma functions to ice and snow after size truncation and obtained new size distributions based on the size-constrained Ni and IWC. That is why that even for particles larger than 62.5 μm, the number concentrations per bin size (unit: L^{-1} μm^{-1}) of the new gamma functions were different from the original gamma functions. Because that original figure could be misleading, we revised it to a new Figure 4 in order to show the impact of size truncation on the same gamma functions as the reviewer suggested, instead of calculating new gamma functions. In addition, we added two subpanels to show the impact of size truncation on the PDFs of Ni and IWC for all model output. Below is the new Figure 4 and the revised text describing it.

Line 206 – 212: "To visualize the impact of the size truncation on simulated data, we employed methods similar to Gettelman et al. (2020) and reconstructed the simulated particle size distributions for snow and ice in Figure 4 a, using gamma functions from Morrison and Gettelman (2008). Compared with the observations, the number density for combined ice and snow is overestimated for smaller particles (< 400 μm) and underestimated for larger particles (> 1000 μm). After applying size restriction, the PDF of simulated Ni and IWC show increasing probability of small Ni and decreasing probability of small IWC due to the removal of small particles (Figure 4 b and c)."

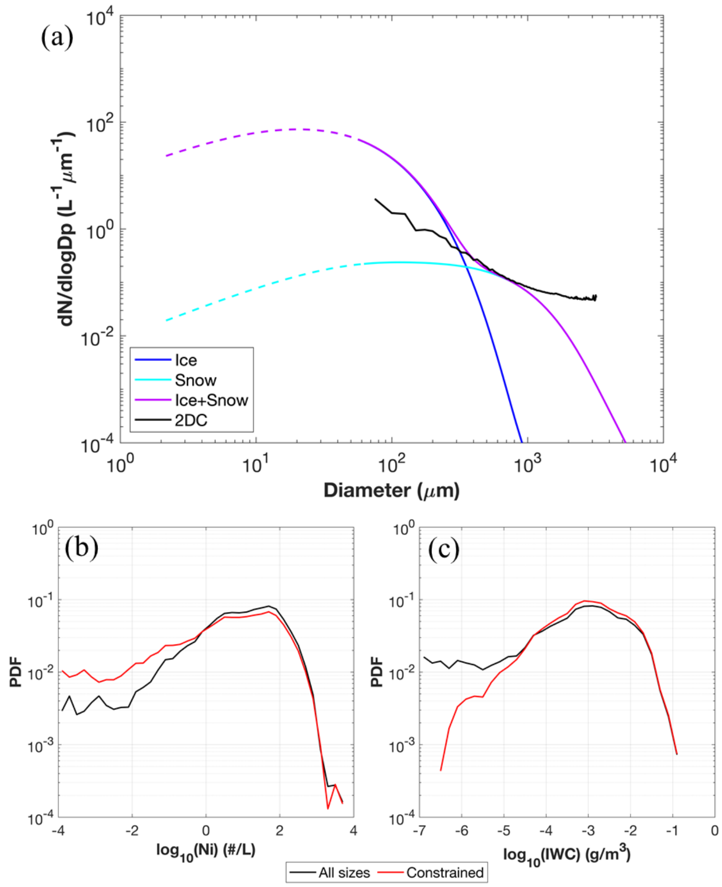

Figure 4. (a) Observed size distribution (black line) and reconstructed size distributions from simulated ice (blue) and snow (cyan). Size truncations to diameters < 62.5 µm (dashed lines) are shown for simulated hydrometeors, while the remaining particles (≥ 62.5 µm) (solid lines) are used for comparisons with observations. Size distributions for combined ice and snow in the simulations (purple) are also shown before and after the size restriction. (b) and (c): PDFs of Ni and IWC in the simulation before and after size truncation.

The ice is handled better, (truncation), but I still think there is something missing in the comparisons: the factor of 3-10 difference seems difficult to understand at the large scale. I think there is still something apples-to-oranges about the comparisons (see comment above). The comment below provides an avenue to address this.

You cannot really say that the models underestimate IWC (e.g., line 395, line 408). They underestimate IWC and number in the size range observed from the observations, but that could be a bias in the size distribution. The conclusions should probably be clarified on this point with making the deviation more specific to a size range.

We agree with the reviewer that the comparison result is only applicable to the size range being evaluated. We added this clarification in section 5 Discussion and conclusions (line 402 – 404): "Differences in the particle size distribution, such as lower number density of larger particles (> 1000 µm) in the simulation

(Figure 4 a), may also contribute to the underestimation of IWC by the simulation. All the comparison results on IWC, Ni and Di are only applicable to the size range being evaluated (≥ 62.5 μm)."