Reply to review by Minghui Diao

This manuscript uses the ICE-L field campaign measurement to compare with simulations from the Unified Model. In addition, idealized simulations from a 2-D model, the KiD model, were used to conduct additional simulations and examine downward moisture flux. The UM model included a recently developed module – CASIM, which enables the analyses of dust particles and their impacts on liquid and ice hydrometeors. The overall organization of the writing is straightforward. The sensitivity tests on various heterogeneous nucleation parameterisations provide valuable information. The reviewer has a few major comments, followed by some minor comments. A major revision is recommended before being considered for publication at ACP.

1. *For the ICE-L field campaign, the 2DC data are restricted to > 125 micron. However, it is not clear if the model outputs of ice water content (IWC) have considered the size cutoff in ice crystal size distribution. From Figure 5 axis label and caption, it seems that ice crystal number concentration (Ni) has been restricted to > 125 micron. But in the legend of Figure 4, qt did not mention any size cutoff. Please clarify this in the main text besides the figure legend.*
   Also, can the authors comment on possible impacts of the comparison results if small particles were included in the comparison? For example, would the model show better or worse results compared with observations?

   Reply: The measured ice water contend comes from the integrated 2D size distributions for sizes >125 µm. After some discussion with Andrew Heymsfield and consideration of additional available data from the 2DS instrument, we have now included data for crystals down to a size of 50 µm. This is possible for the ICE-L data as the impact of shattering in the lenticular wave clouds is probably minimal due to the overall small ice crystal sizes. The validity of 2DC data for small ice crystals in the ICE-L data-set is supported by the agreement with 2D-S data, that has a lower detection limit.

   The modelled values have been derived by integrating over the ice particle distribution, which, however, is not explicitly represented in the model. The modelled size distribution is derived from the ice mass mixing ratio, number concentration and assumed shape parameter. For the number concentrations this had already be done in the original manuscript. We have now taken the same approach for the ice mass mixing ratio (which is also used to correct the total condensate). Differences are generally very small.

   If there were a significant amount of ice crystals smaller than 125 (50) µm present in observations (and not in the model), the comparison to modelled number concentration would improve in the region, where heterogeneous freezing dominates (west of ~ -105.1°W). In the part of the cloud influenced by homogeneously formed ice crystals, the model would compare less well with the observations in terms of ice water content and number concentrations for flight legs B and A (improved fro flight leg C). Underestimating the presence of small ice particles affects the comparison of number concentration much more than that of ice water content.

   Changes to manuscript: We updated Fig. 4 d-f and Fig. 5 a-c using the corrected model ice water content values (including additionally 2D-S data and using a threshold of 50 µm). We now also discuss the implications for the comparison from the measurement restrictions to larger ice particles in section 3.2.
   modifications / extra text: lines 345-356

2. *Another main comment is related to measurements of 2DC and CDP. In Line 133 – 135, the authors commented that 2DC probe is used for IWC and Ni, and CDP is used for cloud number concentration (I assume that you mean liquid droplet number concentration?). However, in previous studies, we found that 2DC may measure some large drizzles, while CDP may measure some small ice. A detailed discussion about separating liquid and ice from 2DC and CDP measurements was given in D'Alessandro et al. (2019, J. Climate), https://doi.org/10.1175/JCLI-D-18-0232.1, "Cloud Phase and Relative Humidity Distributions over the Southern Ocean in Austral Summer Based on In Situ Observations and CAM5 Simulations". Can the authors comment on the potential impacts on the model evaluation, if some ice was misidentified as*

liquid in CDP measurements, and some liquid was misidentified ice in 2DC measurements? Some sensitivity tests on possible variations of IWC, LWC, Nice and Nliq derived from field observations would be helpful.

Reply: Drizzle drops form through droplet coalescence. Drizzle droplets are highly unlikely to form in lee wave clouds at this temperature over the timescales for a parcel to transit through the cloud. Therefore the 2D-C is unlikely to detect liquid for this case. Detection of ice by the CDP is more likely, however the number concentrations will be ~1000 times less than the droplet concentrations and so will not contaminate the droplet number concentration.

Changes to manuscript: We added some text to alert the readers to the possible misclassification of ice as liquid particles and vice versa.
modifications / extra text: lines 345-356

3. *Homogeneous freezing has been briefly mentioned in a few places, but there are not many discussions on the quantitative impacts from it compared with heterogeneous nucleation. For example, even though homogeneous freezing is more dominant at colder temperatures, such as at below -37 C, ice crystals formed by homogeneous freezing can sediment into lower altitudes, and therefore being misidentified as ice formed via heterogeneous nucleation. Can the authors comment more on this sedimentation effect?*
The reviewer suggests that the authors quantify IWC and Ni into two categories – those originated from homogeneous freezing versus those from heterogeneous freezing. Would this be possible for the model used here? For example, additional lines can be added to Figure 4 (d-f) analysis of qt and Figure 5 (d-f) analysis of Ni, to quantify these two components.

   Reply: Ice crystals formed by heterogeneous nucleation indeed can influence ice cloud properties at lower levels by sedimentation. This is why we limited the comparison of Ni to the heterogeneous freezing parameterisations in Fig. 6 to the cloud part at the upstream edge (west of -105.15°E). Unfortunately it is not possible to trace ice formed by different processes in the UM-CASIM model. However, we have conducted an additional test simulation, where we have switched of homogeneous freezing (using DM10 for heterogeneous freezing). The results from this simulation are now included in Fig. 4 and 5. In the region west of -105.15°E the ice number concentration, cloud droplet number concentration, and cloud / ice mass mixing ratio are identical to the simulation with homogeneous freezing. This justifies our approach taken in Fig. 6. Some discussion has been added regarding the impact of homogeneous freezing on the cloud region further downstream.
   Changes to manuscript: modifications / extra text: lines 214-218, 304-307, 320-322

4. *In the conclusion and the result section, when comments were made on whether the model performance is good or not, it seems a little arbitrary. One suggestion is to add some comparisons with previous studies, or with the older versions of the same model. If improvements are seen compared with previous work, then it is more convincing that this model performs better.*

   Reply: see reply to major issue 1 from RC2.

   Changes to manuscript: see reply to major issue 1 from RC2.

Minor comments

- *Line 199, "the he modification". Typo.*

  Reply: Thank your for pointing this out. Correction done.

- *Line 161, recommend adding a full description of notations for temperatures and altitudes used in this study. For example, there is t_ct for cloud top temperature, but later in Figure 10, the axis label uses T_min for cloud top temperature. Please be consistent. Cloud thickness is defined as z_c, but the definitions of z_ct and z_cb is not explicitly mentioned (I assume they are cloud top and cloud base height, respectively).*

  Reply: Thank your for pointing this out. Correction done.

- *In equation (2), there are notations of z_ct,t and z_cb,t. How are they different from z_ct and z_cb?*

  Reply: Thank your for pointing this out. Correction done.

- *Line 168, "32.1 K" should be in Celsius.*

  Reply: Thank your for spotting the errors in the units. Corrections done.

- *Line 243, "temperature basis", biases?*

  Reply: corrected

- *Line 275, the authors mentioned that "while significant cloud glaciation also only occurs in the downdraft region, ice crystal number concentration increases further downstream". Is there any explanation why significant cloud glaciation only occurs in the downdraft region? It seems counter intuitive that downdraft leads to glaciation and new ice crystal formation.*

  Reply: This is the impact of sedimentation from the homogeneous freezing region as evident from the difference between the simulations with and without homogeneous freezing parameterisation. This was already state in the sentence you comment on here. We have rephrased the sentence to make it clearer and also refer to the new simulation without homogeneous freezing.

- *Line 277, "in the model the air parcels likely experience larger vertical displacement", is there any evidence of the parcel displacement? Is it possible that other factors could lead to higher ni, such as homogeneous freezing is being activated too early, allowing too little clear-sky ice supersaturation?*

  Reply: There is no direct measure of vertical displacement in the observations, as this is a Lagrangian measure. Deviations between the parameterised and actual homogeneous freezing can lead also to the observed discrepancies. We included this in the text.

- *Line 278, "ice crystal population at observed along flight legs", delete "at observed"? In the same line, "a earlier", an earlier.*

  Reply: corrected

- *Line 279, "... ice crystal number masking the depositional growth", should it be "ice crystal number <and> masking the depositional growth"?*

  Reply: reformulated to make sentence clearer.

- *Line 292 - 293, "the longevity of ice crystal... related to smaller average ice crystal mass. . .", what is the meaning of ice crystal mass? Do you mean the mass of individual ice crystals, or the total ice water content?*

  Reply: Ice crystal mass refers to the mass of the average ice crystal (as calculated based on the assumed distribution and the prognostic variables of ice number concentration and mass mixing ratio). We clarified this in the text.

- *Line 298 – 299, "the overestimation in initial ice crystal number is either related to the heterogeneous freezing parameterisations used or a too large diameter of the newly formed ice crystals." Heterogeneous freezing generally forms fewer ice crystals than homogeneous freezing. Is it possible that the high ni here is contributed by homogeneous freezing? In addition, the comment on the model having too large ice crystals and therefore overestimating Ni doesn't seem right. If the diameters of the newly formed ice crystals are too large, they would sediment faster and reduce the ice crystal number concentration. In addition, if the total water content is conserved, forming too large ice crystals would lead to fewer ice crystals, not more ice crystals.*

  Reply: To clarify the impact of homogeneous freezing, we have conducted an additional simulation, in which homogeneous freezing is switched off. It is evident from comparing this to the existing simulations that there is no impact of homogeneously formed ice crystals on the ice crystal number concentration in the part of flight track we are discussing here.
  As to the argument with ice crystal size: The observations only measure ice crystals larger than 125 µm. Newly formed ice crystals can be smaller and hence the first detection of ice crystals

does not only depend on heterogeneous nucleation but also vapour deposition. If in the model, newly formed ice crystals are larger than in reality and we apply the same threshold of 125 µm, crystals will be detected earlier, i.e. closer in time to the nucleation event. Hence, the number of ice crystals will appear larger than in the observations, as a larger fraction of the heterogeneously formed crystals exceed the 125 µm threshold and are detected. We have added a sentence to clarify our argument.

- *Line 305, "observations if", observations of?*

  Reply: corrected

- *Line 311, "This data". Data should be in plural form. This typo occurs in several places, including figure captions and the "data availability" section. Please use a global search to correct them all. Same for Line 321, observation data . . . is, should be are.*

  Reply: corrected

- *Line 328, "but introduces", and introduces?*

  Reply: corrected

- *Line 330, here both DeMott et al. (2010) and Tobo et al. (2013) are mentioned as the ones giving the best agreement. But in the conclusion section, only DeMott (2010) is mentioned. Maybe the conclusion can provide more comments on the best agreement based on specifically what variables.*

  Reply: We have expanded the text on this issue and also include the DM15 parameterisation.

- *Line 398, -45 deg c, "c" should be C.*

  Reply: corrected

- *Line 403, 37 deg C should have a minus sign.*

  Reply: corrected

- *Line 451, the equation (t + A*gamma) should be (t0 + A*gamma)? If not, what is "t" here?*

  Reply: corrected

- *Line 459, please add a comma between "clouds" and "reflecting". Some other sentences are too long as well without a comma to separate different parts of the sentences.*

  Reply: done. We checked the manuscript again and added commata where we deem them appropriate.

- *Line 464, several log_10 didn't have the 0 in subscript.*

  Reply: corrected

- *Line 476, w = 0 ms, should be m s-1.*

  Reply: corrected

- *Line 479, please clarify the meaning of each term in the equation.*

  Reply: added

- *Equation 6. K should be deg C*

  Reply: corrected

- *Equation 7. K should be deg C. Also, there is a km unit. Should be C?*

  Reply: corrected

- *Line 511 – 512, this would be a good place to add comments on previous model evaluation studies and compare with the results shown here.*

  Reply: To our knowledge this is the first study, where a model simulations of mixed-phase orographic wave clouds are directly compared to observations. So this is unfortunately not possible.

- *Line 512, 1 ms, should be 1 m s-1.*

Reply: corrected

- *Line 539, -30 K and -40 K, should be deg C.*

  Reply: corrected

- *Figures 1, 2, 3. Suggest adding labels to three segments as A, B, C, and use texts and arrows to highlight them in Figure 1b. It would make it a lot easier to match them with the figure legends and lines in Figures 2 and 3.*

  Reply: done as suggested.

- *Figure 2. The green shade is making the green lines harder to read. Suggest changing the shading to grey color. One of the green lines (cyan?) should be changed to another color, like a blue or orange color. Similarly, the two green lines are too similar in Figures 3, 4, 5 and 6, and some of the supplementary figures.*

  Reply: done.

- *Figure 7 caption,* "*difference . . . between . . . (upstream) and . . . (downstream)." This can be misleading as if the difference is calculated by upstream minus downstream. Please add a sentence after that, such as* "*That is, differences are calculated as down- stream values minus those in upstream".*

  Reply: A sentence has been added for clarification.

- *Figure 10 caption, 2500 mand, should be 2500 m and.*

  Reply: corrected

- *Figure 10, any description on the white, grey and black lines in the contour plots?*

  Reply: We noticed these are more confusing then helping so the contourlines have been removed in all plots.

- *Figures 11, 12 and 13 b, is T_min the same as t_ct? Please be consistent with the text.*

  Reply: Figure labels have been changed to be consistent with the text.