Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2019-940-RC2, 2020

Atmospheric Chemistry and Physics Discussions
Open Access

Interactive comment on "Vertical redistribution of moisture and aerosol in orographic mixed-phase clouds" *by* Annette K. Miltenberger et al.

Minghui Diao (Referee)

minghui.diao@sjsu.edu

Received and published: 23 February 2020

Reviewed 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

Printer-friendly version

Discussion paper

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?

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.

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 freez-

ing 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.

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.

Below are some minor comments:

Line 199, "the he modification". Typo.

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).

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

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

Line 243, "temperature basis", biases?

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.

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?

Line 278, "ice crystal population at observed along flight legs", delete "at observed"?

In the same line, "a earlier", an earlier.

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

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?

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.

Line 305, "observations if", observations of?

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.

Line 328, “but introduces”, and introduces?

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.

Line 398, -45 deg c, “c” should be C.

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

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

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.

Line 464, several log_10 didn’t have the 0 in subscript.

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

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

Equation 6. K should be deg C

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

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

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

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

Line 541, 0.1 g kg, should be g kg-1.

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.

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.

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 downstream values minus those in upstream".

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

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

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

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2019-940, 2020.