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

Anonymous Referee #2

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This study presents an evaluation of the Unified Model (with the CASIM module) using wave cloud aircraft observations. Multiple heterogeneous freezing parametrizations are examined and compared with the observations. In addition, a large number of the KiD model simulations are used to examine the impact of the mountain wave period and cloud-top temperature on the cloud evolution. Finally, the authors provide a conceptual model based on the acquired knowledge, which estimates the wave cloud-driven redistribution of water vapor.

I find this manuscript quite comprehensive and well written. I like the conceptual model idea, although it is obviously case-dependent (as suggested by the authors as well). However, I think that additional work needs to be done before this manuscript can be published in ACP (Major revisions).

Major comments: - UM results: I am not convinced that there is a very good agreement between the UM and the observations (l. 239-244). The statement in the text is subjective, that is, in terms of percentage, the specific humidity errors are very large, on the order of up to several tens of percent (see fig. 2b). Same for the vertical velocity amplitude - errors of 1 m/s can be on the order of ~50% relative to the observations (e.g., leg 2 – fig. 2b). As the UM simulations serve as a benchmark for the KiD simulations, the implications of a weak agreement between the UM and the observations could be significant.

- Homogeneous freezing regime: given the fact that for a large part, the heterogeneous freezing parametrization is examined here, the UM simulations, and hence, also a large fraction of KiD simulations, are "contaminated" by homogeneous freezing in the top of the cloud layer (e.g., fig. 7), which obviously non-linearly impacts the underlying heterogamous cloud layer. The authors did not refer to the homogeneous freezing parametrization in the UM and how well it corresponds with the parametrization in the KiD model. Now, I presume that the UM is too complicated to vary the homogeneous freezing parametrization, but I suspect that the influence of other parametrizations can be examined in the KiD model. An additional approach to address large parts of this comment would be to run the UM initialized without homogeneous freezing influence on the cloud layer (e.g., by offsetting the temperature profile to higher values while retaining the RH profile). I wonder how much would the results of this study change in that case (e.g., deviations of the UM from the observations)?

- Deposition nucleation is not mentioned at all in the text, although the current understanding is that its efficiency significantly increases when we approach the homogeneous freezing regime (e.g., Kanji et al., 2017, https://journals.ametsoc.org/doi/full/10.1175/AMSMONOGRAPHS-D-16-0006.1).
I understand that we would typically except immersion freezing to still account for most of the nucleation, but the deposition mode should still be mentioned in the introduction as well as in the model description, even if it is eventually omitted from the simulations.

- Negligible impact of rain processes in the KiD simulations and neglecting rain processes in the conceptual model: the minor impact of rain could be driven by: a. the dominance of the homogeneous ice precipitation from cloud top, b. high cloud droplet number concentration that leads to weak collision-coalescence in the model, and/or c. influence of the collision-coalescence kernel implemented in the model. I am not convinced that rain processes are indeed negligible and whether they should be neglected in the conceptual model. If necessary, I presume that adding a "rain component" to the conceptual model should not be a difficult task, given previous conceptual models for warm clouds (l. 435-436).

- Importance of periods longer than 1000 s (stated in the abstract and l. 554-557): do not believe this conclusion, as the authors did not consider different obstacle heights (eta values I presume). As a result, the impact of the (likely maximum) vertical velocity is nearly ignored in the parameter space evaluation, although it should definitely impact the longevity of the wave cloud, among other factors, via ice processes, especially in the homogeneous freezing regime, where we would expect to lose all condensated droplets relatively fast.

Minor comments: l. 49-52 – That is a rather complex sentence. I suggest rephrasing or breaking into two separate sentences.

l. 109 – ICE-L acronym definition is missing.

l. 109 - please add parentheses to the citations.

l. 109 – inconsistent date (November 17th) with the following sections and figures.

l. 127-130 – These two sentences seem redundant - repeating information already provided in the Introduction.

l. 146 - Suggest consistency in the number of fractional coordinate digits.

l. 147 – focusses –> focuses

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l. 167 - Please define the source for this eta value - I will presume that it is equivalent to the obstacle (mountain) height

l. 168 – 32.1 K - Suggest consistency about the temperature units (C instead of K).

l. 173 – Eq. 2 I do not find consistency in the definition of the different z ranges.

l. 175 – (nothing to revise here) – Figure 9 is very nice.

l. 201-202 – What is the parametrization for ice hydrometeor fall velocity used in UM and KiD? This may have a substantial impact on the results presented below.

l. 242 – I presume “basis” should be “bias”

l. 252-253 - If the correction is made for 0.02 g/kg, what is the importance of 0.0001 g/kg in fig. 1?

l. 282 - I can only see some sort of an ni agreement in fig. 5f. I can’t interpret the max ni intersection in panel e as model-observations agreement. Using that parameter as a measure of model performance could be misleading.

l. 321 - “data .. is . . .” - "Data" is the plural form of "datum" - please correct the text and figure captions accordingly (is –> are, etc.)

l. 330 - best agreement with DM10 and TB13 - how do you define the best agreement? In some aspects (e.g., absolute ni values), the log scale in fig. 6 is misleading because I would suggest that the best agreement is with DM10 and DM15.

l. 365 - 0.1 g/kg - these are mixing ratio units, not flux units. I suggest providing a more consistent terminology throughout the text (also related to the conceptual model).

l. 403 - a minus sign is missing for the temperature

l. 406 - I suggest adding a reminder to the reader about which two simulations are discussed here.

l. 445-446 - I presume that the definition of Gpot is the integration of qv minus qs(Tmin), yes? Please clarify, or alternatively, describe eq. 4 earlier in the text.

l. 473 – “saturation)” - redundant parenthesis

l. 479 – inline equation - not all terms in this equation are defined.

l. 492 – pre-scribed –> prescribed

l. 500-501 & Fig. 13 - I suggest adding a panel that shows the difference between the conceptual model and the KiD model. The discrepancies I see in Figure 5 in the SI (especially in panels c and d, which are shown in log scale) suggest that the differences can be quite significant when the time period and Tct parameter space are examined. Also, shouldn't there be consistency regarding the discussion/use of total water (qt) and water vapor (qv) throughout the text in figures (7-13)?

l. 504 - I presume that 'b' is missing when referring to fig. 13.

l. 538 - Except for the conceptual model, I did not encounter any discussion about the results based on varying cloud thickness. I suggest adding some information to the text and figures, or removing it from this discussion about the conclusions.

Fig. 1 - Suggest changing the cyan, blue, and gray curve colors - they mixed with the colormap. Also, the units in the colorbar are confusing here. Also, What altitude do the black contours represent? This should be specified.

Fig. 1 and discussion in the text - Is 10ˆ-7 kg/kg above the aircraft instrumentation uncertainty level for IWC and LWC? This should be discussed and justified in the text, i.e., what is the “true” extent of this cloud field given measurable justified quantities?

Fig. 2 - I suggest redefining the altitudes for each flight leg

Fig. 4 - I'm having a tough time reading the axes labels

Fig. 3-5 - please provide a title for each panel stating the flight leg and/or altitude. At the moment, it is hard to follow the text.

Fig. 5-6 – suggest adding a legend instead of directing the reader to fig. 4 every time.

Fig. 7 - blue for positive values in panels a,b is counter-intuitive. I suggest flipping this colormap.

Fig. 9 - Please correct qv–>qt in the legend

Fig. 10 - The figure caption is not complete, e.g., last sentence, circle markers, the definition of the two simulations, etc. The varying contour colors in panels b,d are quite confusing.

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