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Atmospheric Chemistry and Physics Discussions

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Interactive Comment

Interactive comment on "Ice phase in altocumulus clouds over Leipzig: remote sensing observations and detailed modelling" *by* M. Simmel et al.

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Response to comments of reviewer 2 on

"Ice phase in Altocumulus Clouds over Leipzig: Remote sensing observations and detailed modelling" by Simmel et al.

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Interactive Discussion

Discussion Paper

http://www.atmos-chem-phys-discuss.net/15/1573/2015/

We thank the reviewer for his/her constructive suggestions and for generally accepting the paper when the proposed revisions are realised.

Comments of the reviewers are cited in *italic*. Revised manuscript with highlighted changes is given as supplement.

This paper has great potential as a comparison between modeled and observed mixed phase clouds. Its strengths are the high quality remote observations and the relatively direct modeling approach that allows for straightforward implementation and comparison of different ice nucleus (IN) concentrations and ice crystal shapes. The paper stumbles before reaching the finishing line, so I encourage the authors to improve the paper to its potential. There are a number of problems with the analysis and presentation, as detailed below, but these are relatively minor aspects that can be improved with modest effort. The major shortcoming of the paper is the complete absence of comparison between modeled and observed properties in section 5. This is the section where the most interesting science is finally addressed, through variation in IN concentration and ice crystal shape and fall speed. Is it possible to vary these parameters and obtain results that compare with the lidar and radar observations with higher fidelity? And as *such, can the suitability of IN parameterization or crystal habit representation be evaluated? As a single example, there is discussion of the "stronger tilting of the virgae" for nonspherical ice (page 1589). This seems like a perfect aspect to compare to observations. It is only one example, and in general, there needs to be a much more thorough and, to the extent possible, quantitative comparison between modeled and observed mixed-phase properties in this section.*

The presentation of observations is extended by showing IWC and LWC for both cases. Comparison between model and observation seems to be difficult for case 1 where the observations are close to the detection limit. Additionally, the INP parameterization of DeMott is rather insensitive to the number of aerosol particles at rather high temperatures of -5 C. For case 2 it seems to be clear that either too many INP or non-spherical particles could easily lead to an overestimation of the ice-phase and even the complete depletion of the liquid phase which is in contradiction to the observations.

Conclusions about possible ice shapes being consistent with (a) laboratory studies and (b) our observations are drawn in the final section.

The following points should also be addressed:

- Abstract: "warm temperatures" should be "high temperatures" (air is warm, temperatures are high).

- Pg 1574 line 22: “attributed the aerosol” should be “attributed to the aerosol”.

The changes were done according to the suggestion of the reviewer.

- Pg 1574 line 25: I do not understand the statement that only biological particles form ice above -15 C. In the parameterization employed, which is mostly describing dust, IN exist at much higher temperatures.

There is an ongoing discussion of this topic. In laboratory studies, it was shown, that biological material is able to initiate ice at those high temperatures. However, there are at least two possibilities for dust to form ice above -15 C: (a) Pure dust is also able to form ice above -15 C if only enough material (surface) is available. This is a question of detection limits in lab studies (frozen drop fractions). Experiments with large drop on freezing arrays at least hint to this possibility. (b) Dust is mixed with biological material (forming soil dust). Ice formation in this case is triggered by the biological material at least at higher temperatures.

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- Pg 1575 line 1: should be "to what extent".

The changes were done according to the suggestion of the reviewer.

- Pg 1575 line 5-6: reference needed for this statement.

Statement was removed from the text since it was too speculative.

- Pg 1576 line 16: define GDAS.

- Pg 1576 line 24: "could be observed" should be "was observed".

- Pg 1577 line 1: "an LWP" should be "a LWP".

The changes were done according to the suggestion of the reviewer.

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- Note: I stopped correcting minor grammatical errors after section 2. Authors, please proofread the paper carefully.

Careful proofreading was done.

- Pg 1577, sec 3: Asai-Kasahara type model should be described more thoroughly, e.g., be clearer on cylindrical geometry, boundary conditions, etc.

For initialization of the Asia-Kasahara model, only a vertical profile of temperature and humidity is needed (now shown in new Fig. 7). No additional boundary conditions are needed. The radius was chosen to be 100 m for the inner cylinder and 1000 m for the outer cylinder. In an Asai-Kasahara model the ratio of the radius of the inner cylinder to the radius of the outer cylinder is the dominating parameter and the chosen value of 1:10 is a typical value for an Asai-Kasahara model setup. The results are sensitive to the radius ratio when the outer cylinder is chosen too small. Then the influence of the inner on the outer cylinder increases and the outer cylinder cannot serve as a proper background any more. However, the geometric configuration of the model is not intended to describe or to match the geometry of the clouds (and cloud-free spaces in between) as observed. It should rather be understood as a possibility to describe a vertically resolved cloud evolution and to provide the possibility of horizontal exchange

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with a cloud-free background (see also response to reviews 1 and 3).

The manuscript was changed accordingly.

- Sec 3.1.1: Regarding "Immersion freezing occurs as soon as liquid drops above a certain size limit are present", why is there a drop size dependence? Freezing probability should be related to IN properties, not to volume of drop.

One possible way of the drop volume to influence freezing is the concentration of solved chemical species which may lead to a freezing point depression in the case of relative large aerosol particles with relatively little water mass. The size limit is intended to make sure that there are supercooled drops available for freezing and to avoid the freezing of aerosol particles which are present in the same joint spectral liquid-phase field.

- Sec. 3.2.1 and Fig 5: What is the advantage of using a stochastic forcing for vertical velocity? It seems to only add complexity, with no obvious illumination of new physics. Why not force with a deterministic, e.g., sinusoidal, vertical velocity, for example?

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A simpler profile was tested (constant up- and downdrafts for given times with short linearly interpolated transitions between both), however, it appears that this more complex stochastic forcing gives more realistic results and better shows the variety of cloud's LWC and IWC since it better matches the temporal patterns of the updraft. Additionally, the more often changes between up- and downdrafts on smaller time scales provide a certain horizontal exchange between inner and outer cylinder which is important for the supply of fresh INP.

- Pg 1583 line 27: I think "presence time" is clearer as "residence time".

The change was done according to the suggestion of the reviewer.

- Sec 4: Comparison with figures and reported results is not straightforward: for example, figures are in g/kg, observations are in kg/mËĘ3. Please be consistent.

To be consistent, all model results were changed to g/m^3.

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- Fig 1: Left panel should be labeled as log10 of IWC.

The change was done according to the suggestion of the reviewer.

- Fig 1: What is the purpose of the dashed box? Maybe I missed it in the text, but it should also be specified in the caption.

Fig. 1a and 1b show different height and time ranges. Since Fig. 1b shows a larger part of the data, the dashed box indicates the region shown in Fig. 1a. This was clarified in the revised manuscript (new Fig. 1).

- Figs 6, 8, 10, 12: How useful are these comparisons? The differences between the panels are so small that it is not clear to me that they need to be presented graphically. The numerical results such as max LWMR and max IWMR may be adequate, unless details of the plots are specifically discussed in the text.

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In our opinion, there are quite significant differences between the cases shown which were already mentioned in the manuscript. In Fig. 6, the main differences can be seen in the liquid phase (contours), but also in the ice phase (increase of the ice phase cloud base). Fig. 8 shows substantial differences in both phases, Fig. 10 again shows differences mainly in the liquid phase. Fig. 12 (upper panels) show large differences in both phases, whereas Fig. 12 (lower panels) illustrates the different timing due to the changed forcing.

- Figs 6, 8, 10, 12, 14, 15, 17, 18: Maximum ice and liquid water values are reported with 7 significant digits. It cannot be that such accuracy is valid. (Also note that there is some inconsistency in using max LWMR versus max. drop water, etc.).

There is no valid 7 digit accuracy. This was changed to 3 significant digits. In the revised paper, LWC and IWC are used consistently.

- Figs 15 and 18: The captions state that liquid is denoted by color and ice water mass is denoted by contours. That seems to be backwards.

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Yes. This was changed.

- Fig 16: In the bottom of the left panel, please confirm that all lines are plotted (i.e., are they identical and cannot be distinguished?)

All lines are identical. Compared to the liquid fraction, the ice fraction is so small that changes as modelled in the sensitivity runs are too small to affect the liquid phase considerably (compare LWMR (now LWC) maxima in Table 3 of case 1).

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 1573, 2015.