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Atmospheric Chemistry and Physics
Discussions

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 1 on

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

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*.

General coments

The authors study two altocumulus cloud case studies that were observed over Germany via ground-based remote sensing. The cases were selected to represent the warmest possible ice formation and more typically cold ice formation within altocumulus. The authors apply an axisymmetric 1D model with spectral microphysics. The model is initialized with observed or model-derived thermodynamic profiles. Varying assumptions are made regarding prescribed vertical motions, aerosol and ice nucleus properties, and ice habit. Generally little work has been done on altocumulus microphysics, but that which has been done requires more review in the introduction and *conclusions to motivate this work and to place the results into context. The approach is generally sound, but not enough details are provided to allow the work to be reproduced. The observations should be shown and described more completely. Overall, this work merits publication after revisions to the manuscript that can readily address specific comments below.*

Specific comments (page/line number if relevant)

1. The scientific questions to be addressed are not adequately stated. Ice nucleation is discussed in the very short introduction, but no questions are targeted for this study. This is perhaps related to the problem that the authors provide no background on altocumulus. Has any study simulated such clouds before? Why did the authors choose to use a model? Why this model with elaborate microphysics but simple dynamics? Has any literature drawn conclusions about altocumulus relevant to this study? Does this study produce conclusions that are consistent with past literature? References should include Fleishauer et al. (JGR 59:1779, 2002), for instance.

Altocumulus clouds are a good example for shallow mixed-phase clouds with comparably simple vertical structure — at least for the single-layered cases as they are considered here. Therefore, a dynamically simple model setup with prescribed vertical velocity was chosen to remain close to the observations. Feedback of microphysics on dynamics is not considered to concentrate on primary microphysical effects and to avoid

misleading conclusions about secondary effects due to changed dynamics. A model intercomparison study by Ovchinnikov et al. (2013, doi:10.1002/2013MS000282) has shown that bulk microphysical models tend to underestimate ice growth by vapor deposition due to the underlying ice distribution assumptions. In contrast to this, bin models directly simulate the shape of the distributions and, therefore, no assumptions concerning the shape have to be made.

The underlying topic is mixed-phase microphysics and the interaction between the three phases of water. It is well-known that due to the different saturation pressure of water vapor with respect to liquid water and ice, a mixed-phase cloud is in a non-equilibrium state which, nevertheless, may lead to a quasi-steady existence (e.g., Korolev and Field, 2008, JAS). To study those interactions, a bin model is suited well, since condensational/depositional growth is not only described by saturation adjustment but by a detailed description of sub-/supersaturation of each size bin resulting in different growth rates. This automatically results in a very detailed description of the Wegener-Bergeron-Findeisen (WBF) process which drives the phase interaction.

The main drivers for this phase transfer are vertical velocity (leading to supersaturation and subsequent droplet formation) and ice particle formation and growth (WBF starts) leading to sedimentation of the typically fast growing ice particles (WBF ends due to removal of ice). The motivation of this work is to shed more light on the relative contributions of the different processes involved in these complex interactions (see also response to review 3).

The manuscript was changed accordingly.

2. The observations that motivated the selection of these cases, and which are relied upon, are not adequately shown and their uncertainty properties are not described.

Figure 1b makes a good start at showing case 1 cloud conditions, but other case 1 figures are truncated in time. Please show all five of the following fields between 23:45 and 0:40 for case 1 (providing important context for the narrow 20-minute window used for the study) and for case 2: lidar backscatter, radar reflectivity, retrieved IWC, retrieved LWC, retrieved vertical wind. Only the first is shown for the full time range for case 1. LWC is never shown now. Also please report the stated or estimated uncertainty properties of IWC, LWC and vertical wind speed. Are there no clear-air vertical wind retrievals from the Doppler lidar? Please explain why the vertical wind speeds shown in Figure 2 appear as they do for lidar. Finally, please show plots of the initial soundings used, including RHI and RH.

Additional pictures are shown for both cases in the revised version. However, for case 1 full time range is shown only for 2 parameters (RC signal, radar reflectivity, new Fig. 1) because at 0:22 h, a new cloud appears to form at a lower level (compare also humid layer in profile, Fig. 7) around 3000 m.

Accuracy of the IWC is +/-50 %. For the LWC calculated by the scaled adiabatic approach the same order of magnitude applies. Vertical wind speeds are measured directly by evaluation from the recorded cloud radar and Doppler lidar spectra. Errors are +/-0.15m/s for the cloud radar and +/-0.05m/s for the Doppler lidar. These errors are mainly due to the pointing accuracy of the two systems.

The Doppler lidar (right panel) shows the motion of small cloud droplets at the predominantly liquid cloud top. Hence, in this plot the cloud-top turbulence becomes visible. The cloud radar (left) mainly shows particles falling from the top layer, therefore, particles are mainly moving downwards (green color). Only at the very top at about 4300 m particles are small enough to still be lifted upwards (yellow colors).

There are no possibilities to derive clear air velocity with a coherent doppler wind lidar, because this instrument depends on tracer targets like aerosol particles or cloud droplets. However, clear air motions around a cloud is a very interesting quantitiy which can, e.g., be derived with radar wind profilers. See for example: http://www.atmos-meas-tech-discuss.net/8/353/2015/amtd-8-353-2015.pdf

Initial soundings (T, rh, rhi) are shown as new Fig. 7.

The manuscript was changed accordingly.

3. The authors acknowledge that the specification of vertical winds is a controlling parameter, but they do not discuss the general nature of these winds, which seems to be important to understanding the relationship of the model setup to the large-scale conditions. Is the mean vertical wind described large-scale in nature whereas the stochastic components are turbulence? I would expect the updrafts and downdrafts within altocumulus to be driven by cloud-top cooling rather than large-scale winds. In the case that cloud-top cooling-driven turbulence is driving mixing between downdrafts and updrafts, I would expect it to drive the supply of IN. However, the authors state that the mean wind is driving the supply of IN. Does that mean that large-scale convergence is driving the supply of IN to updrafts and downdrafts whereas turbulence does not play a role in the supply of IN?

We share your statement that cloud-top cooling is an important driver for altocumulus clouds. We consider this effect to be included in the observations as well as in the prescribed vertical velocity.

In the paper, we state that the supply of IN is driven by the horizontal exchange with the outer cylinder. The horizontal exchange is driven by the change of vertical wind speed with height (see Eq. (4)). This means that turbulence (which is responsible for the direction of vertical wind speed) plays a major role in IN supply.

The mean vertical wind can be considered as large-scale driving force, however, due to the model configuration, the strength of the mean updraft has to be chosen larger than observed.

4. The model vertical resolution is 25 m, but what is the size of the inner and outer cylinder? How was it decided how large to make the inner and outer cylindrical coordinates? Are results sensitive to the specification of cylinder relative size? Is the inner cylinder considered to be the whole 20-min cloud observed (both updrafts and downdrafts) whereas the outer cylinder is the air surrounding the cloud? If so, how much air surrounding the cylinder? Or is the cylinder specified to be an updraft element size, similar to deep convection studies?

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 with a cloud-free background (see also response to reviews 2 and 3). Neither is it intended to directly model the cases presented. They rather should serve as frame to judge whether the model simulations lead to results close enough to reality to apply the model to sensitivity studies. Therefore, the 60 minute model runs are not compared directly to a 20 minute period of observations. However, the inner cylinder gives the relevant results for both, updrafts and downdrafts.

The manuscript was changed accordingly.

5. Aggregation and riming are neglected? Please provide some literature support for why that would be appropriate or otherwise explain.

The observed clouds are rather shallow and a large fraction of the ice is formed at/near cloud base which means that there is not that much possibility of ice particles to rime. Aggregation can be neglected due to the rather low ice particle number concentrations for case 1 (relative little probability of collision between particles) and the relative low temperatures for case 2 (reducing sticking efficiency). This assumption is corroborated by the findings of Smith et al. (2009, doi:10.1029/2008JD011531) stating that water vapor deposition (and sublimation), balanced by sedimentation are more important than accretional growth.

The manuscript was changed accordingly.

6. (1581/21) M1 and M6 both have lower free troposphere aerosol. Why did you choose M6 for case 1 and M1 for case 2? How did you apportion 1e5/kg aerosol among the three modes?

For case 2 the upper free troposphere aerosol distribution of M1 was used, whereas for case 1 the lower free troposphere aerosol of M6 was used. The choice of the aerosol distributions is quite arbitrary, however, one intention was to use M6 LFT measurements with and without a polluted layer for case 1. Nevertheless, the polluted layer run was not reported since Lidar observations showed no polluted layers for case 1. For the UFT, no polluted layers were observed in Petzold et al., therefore, we decided to use M1 from the beginning.

We assume that 1e5/kg particles are larger (in radius) than 250 nm according to the parameterization of DeMott et al. to calculate and initialize the temperature-dependent INP field. This has to be considered separately from the AP distributions used for the initialization of the combined AP/drop spectrum. Those are taken as described in the paper cited.

7. (1586/6, 1578/4) DeMott et al. (2010) did not analyze measurements colder than –9 C, to my knowledge. Did you extrapolate their relationship to colder temperatures? If so, how did you decide at what temperature to stop extrapolating when approaching 0 C?

Yes. DeMott et al. (2010) only shows observations for temperatures below -9 C. We extrapolated the relationship to higher temperatures (-5 C). We did not have to decide where to stop the extrapolation in these case studies since in the model used ice formation by immersion freezing could only take place in the vicinity of drops which were only present at temperatures below -5/-6 C.

The manuscript was changed accordingly.

8. (1579/2) Because the relationships in Mitchell et al. (1996) and past literature have been derived from observations over limited size ranges, it is not uncommon to use more than one relationship to represent columns of various sizes (e.g., Sölch et al. QJRMS 136:2074, 2010, table AII). Please provide sufficient information re exactly which relationships you used and over what size ranges for this work to be reproduced.

We used the relationships in Mitchell et al. (1996) in their Tab. 1 for hexagonal plates and hexagonal columns. The mass-dimension power laws were transformed to aspect ratios for the given shapes. For columns, three size ranges (30 to 100 μm, 100 to 300 μm, and above 300 μm in diameter) with different coefficients are given whereas for plates the coeficients are valid for diameters from 15 to 3000 μm.

The manuscript was changed accordingly.

9. (1575/5) Could preconditioned ice nuclei be nucleated as warm as -1 degrees C or some other temperature limit? Please explain mechanistically how preconditioning could introduce ice nuclei relevant in this study, with reference to literature and relevant temperature range.

The statement was removed from the text since it was too speculative.

10. (1576/30) IWC is shown to 2000 m in Figure 1, which apparently is warmer than 0 C according to the text, which states that IWC extends to only 3000 m. Please clarify.

Indeed there is no ice detected below 0 C. The IWC is derived by the parameterization of Hogan 2006 which computes IWC as a simple function of radar reflectivity and temperature. The equation is mathematically valid for T>0 C, so the usage of this equation has to be restricted to temperatures below 0 C. That restriction was, however, not done properly done in this case. The figure was therefore corrected and now shows IWC only up to 0 C.

11. What is the model time step used?

For the dynamics as well as for the microphysics a time step of 1 s is used.

The manuscript was changed accordingly.

12. (1582/22) It is stated that "ice forms primarily at cloud base". Does this mean that ice is primarily nucleated at cloud base? Cloud base is warmer than cloud top, so I would expect more rapid nucleation at cloud top. Please explain.

When drops form at cloud base all available INP active at cloud base temperature can contribute to primary ice formation in the immersion mode. The unfrozen droplets are transported further upwards which results in cooling. Nevertheless, if the cloud is relatively shallow (which is the case here) the temperature difference between cloud base and top is rather small. Therefore, the additional number of active INP causing ice nucleation in the upper parts of the cloud remains also relatively small. Therefore, in summary, more ice particles are nucleated near cloud base than near cloud top in the cases presented here.

Techical corrections

1 (1576/3). Please define TROPOS.

TROPOS is the Leibniz-Institute for Tropospheric Research.

The manuscript was changed accordingly.

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