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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 3 on

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

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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.

General comments: The authors simulate two mixed-phase cloud layers that were observed by remote sensing. However, the dynamical model used is unsophisticated, and key observations needed to initialize, force, and validate the simulations are unavailable. For instance, the study concludes that IWP is sensitive to IN number, but there are no IN measurements to assess how much IN number should be varied in the sensitivity study. Hence the conclusions must necessarily be regarded as tentative. Furthermore, only two cloud cases are examined, limiting the conclusions' generality. If the authors wish to simulate a case study, then I recommend that they choose a more complete dataset to simulate, one that uses more accurate (e.g. in situ) measurements. Also, I recommend that they use a more sophisticated model (e.g. LES). If the authors wish to do an observational study, then I recommend that they exploit the instruments they have. Given the facts that the set of instruments is incomplete, that *none are in situ, but that they can be run continuously, the instruments seem better suited to assessing climatological relationships between variables. If, instead, the authors wish to invest the time to maximize the usefulness of the present study, I would attempt to better quantify the statement "the liquid phase is mainly determined by the model dynamics (location and strength of vertical velocity) whereas the ice phase is much more sensitive to the microphysical parameters (ice nuclei (IN) number, ice particle shape)." In particular, instead of varying w_ave from 0.1 to 0.4 m/s, I would vary it by "observed" values taken from obs or reanalyses or the literature. Instead of varying N_AP by a factor of 10, vary it by the suitable range given by values in the literature. That would provide a better sense of the practical sensitivity of LWP and IWP to w_ave versus N_AP. Consider doing likewise for the other sensitivity experiments.*

The general idea of the paper is to use a two step approach: In the first step, the model is used to simulate a cloud which is close to the one observed (in terms of model input – e.g., temperature/humidity profile, vertical velocity – and output – e.g., cloud evolution, liquid and ice phase). If this is done successfully, the model can be used for a second step which is a sensitivity study with respect to certain poarameters. This sensitivity study is done within the 'model world' by varying the respective input data (e.g., INP number) and comparing the results.

The variation of N_AP is in the range of the observations which are the basis of the paramneterization used. Only the very high concentrations were omitted since no polluted layers were observed by the lidar.

It must be stated that the variation of the w_ave is caused by the model configuration: If w_ave is chosen to be smaller than about 0.1 m/s the model will not be able to reach supersaturation and to form a cloud due to the horizontal exchange with the background. On the other hand, if w_ave is chosen much larger than 0.4 m/s, the

downdrafts will be too weak and too short to lead to cloud-free spaces in between the clouds.

Specific comments:

The abstract is well written, but the introductory section could more clearly introduce the main issues that will be addressed in the paper. What is the gap in knowledge, and how will it be addressed in the subsequent sections?

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). For this purpose, 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 1).

p. 1576: "The liquid part of the cloud extends from about 4250 to 4450 m height at temperatures of about -6 C according to the GDAS reanalysis data for Leipzig." Some of the discussion relates to the temperature at which various IN are active. Therefore, it is of relevance to know: What are the error bars on the temperature measurement? I wouldn't expect a reanalysis to be terribly accurate.

Temperature errors of the GDAS data compared to radiosonde profiles over Leipzig has been determined to be +/-1K during the DRIFT-project by Patric Seifert (see http://onlinelibrary.wiley.com/doi/10.1029/2009JD013222/pdf). These errors seem to be sufficiently small to allow for a strong connection between temperature (as deduced from GDAS reanalysis) and potential ice formation processes.

p. 1577: "For the model studies an Asai–Kasahara type model is used (Asai and Kasahara, 1967). The model geometry is axisymmetric and consists of an inner and an outer cylinder." By today's standards, the Asai-Kasahara model is crude. Instead, I recommend using a large-eddy simulation (LES) model. These days, LES are affordable and easy to configure. If not LES, then I recommend trying a prescribed dynamics model like the Kinematic Driver (KiD) model, because it will provide flexibility and control.

Maybe the term "Asai-Kasahara" is misunderstood. As it is explained, vertical dynamics is prescribed which to our understanding is rather similar to the KiD model. Only the model geometry assumption (cylinder-symmetric with an inner and an outer cylinder) and the exchange between the cylinders (see Eq. (4)) relates to the Asai-Kasahara model.

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 1 and 2).

p. 1577: "Since during the above mentioned observations no measurements of the IN are available, the parameterization of DeMott et al. (2010) is used assuming that all IN are active in the immersion freezing mode." The observations needed to address the scientific questions are lacking. Consider focusing your efforts on addressing a question that your instruments are better positioned to answer.

In general, ambient INP measurements are sparse and typically not available for long-term observations. We do not think that this fact should deter us from investigating those cases. It is a common approach to use certain assumptions (here about INP) and to check how the model results based on those assumptions compare to observations. Additionally, sensitivity studies are carried out to check how important the respective parameter is for the whole situation.

p. 1579: "For case 1, profiles from both methods show a similar general behaviour but the radiosonde profile of Meiningen measured at 00:00 UTC is used since it provides a finer vertical resolution than the GDAS reanalysis data. However, for case 2 the Meiningen RS profile misses the humidity layer at the level where the clouds were observed and, therefore, GDAS reanalysis data for Leipzig at 21:00 UTC were chosen." Apparently, the observations are too inaccurate to initialize the simulations.

The Meiningen profile was not representative for Leipzig for case 2. Therefore, the GDAS profile was chosen as a substitute. Despite the coarser height resolution, cloud formation was triggered in the model when vertical updrafts similar to the observed ones were prescribed. Again, we have to emphasize that the aim of the study was not to model the observed cases in detail but more to obtain reasonable model results that allow for sensitivity studies which are in turn transferable to the "real world".

p. 1581: "Since no in situ aerosol measurements are available, literature data is used." The dataset is inadequate for the purpose of studying sensitivity to IN.

The Lidar shows that no dust layers or similar pronounced features concerning aerosol could be observed. Therefore, we consider it reasonable to use those literature data. The aim of the study is not to study sensitivity of the clouds with respect to INP on the basis of observations. If this was the case, we would have to have measurements of both, cloud ice phase as well as INP, to obtain e.g., statistical correlations between both data sets. However, we use a two step approach mentioned above which allows us to perform the sensitivity study in the 'model world'.

Please also note the supplement to this comment:
http://www.atmos-chem-phys-discuss.net/15/C2032/2015/acpd-15-C2032-2015-supplement.pdf

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