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Atmospheric Chemistry and Physics Discussions

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Interactive Comment

Interactive comment on "Modelling of cirrus clouds – Part 1: Model description and validation" *by* P. Spichtinger and K. M. Gierens

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General Comments:

This paper contains two parts: The first is a technical description and validation of a new model formulation for cirrus cloud microphysics. The second is an exploration of the cirrus cloud processes generated by this model using a sensitivity test approach. I found the paper to be interesting, new, and highly relevant, but overwhelming in the amount of information presented. I recommend that the authors submit the model description as a technical note and the science results as a separate paper. In my opinion, the text is too long and the paper contains too many figures (29) for a single contribution.

In the model development portion of the paper, additional description of the justification for the modeling approach based on observations or the literature would be useful. What level of complexity is required to produce a useful simulation in 2D or 1D? When are the approaches being taken in this paper significantly different than those that have been taken by other modelers? What parameters have the largest influence and/or the largest uncertainty? When approximations are being made to the numerical solution, what advantage in speed vs. accuracy is gained? Although there is discussion of these types of issues in the text (e.g., in the choice of distribution type), the paper would be stronger if the authors provided more context and motivation for the model development effort that they have undertaken. When observations or insights based on observations or previous modeling studies are mentioned, a citation or data should be provided. When the treatment in this model is not significantly different than previous models, this should be mentioned and streamlining of the text should be considered (e.g., discussion of Koop homogeneous freezing parameterization). The treatment of heterogeneous freezing should be better described and justified, or maybe completely neglected since it is not utilized in this paper. The validation of the model could be presented in a way that provides physical insight into the importance of the model assumptions (e.g., discussion about ice crystal shape).

In the science results presented by the authors, several sensitivity studies are performed. The results indicate the strong influence of vertical velocity fluctuations at a variety of scales and vertical wind shear on cirrus cloud evolution and vertical structure. The results are new and interesting and will be of great interest to ACP readers who follow the cirrus cloud literature. In particular, the influence of the sedimentation in the top of the supersaturated zone on the underlying relative humidity evolution and nucleation events is really neat. That being said, the presentation could be improved. Some suggestions are made along these lines, but I think that if these results were parsed into a separate paper, there would a great opportunity to put the results into context with previous modeling work and atmospheric observations. With a minor amount of restructuring and additional context, a much stronger paper would emerge.

Specific Comments:

Page 604, Lines 2-4. Please provide a citation or justification for why it is generally assumed that homogeneous freezing is the dominant freezing process at low temperatures.

Page 604, Lines 8-18. Here, I think it would be more useful to focus on how the model that will be described in this paper differs from previous modeling efforts. Why do we need a new model? What new classes of problems can one investigate with this model that one cannot with previous models? It would be useful to consolidate this information in one location and use it as motivation for the undertaken model development effort.

Page 609, lines 14-15. More justification based on the literature would be useful here. Is the ice crystal shape important for some processes (e.g., radiation, sedimentation)? What do you mean that ice crystal size is a *vague notion*?

Page 616, lines 19. Is equilibrium a good assumption? Do previous models that have included the Koop homogeneous freezing formulation make this assumption?

Page 617, line 18. Here, it would be better to justify the choice of 1.33 by showing an example calculation. Utilizing a shift in the mean mass of the aerosol distribution is an interesting idea, but its influence on the model results is not clear.

Page 617, line 1-5. The description of heterogeneous freezing is confusing. Are you implying that all aerosols are considered ice nuclei? A clear description and physical justification for the treatment of heterogeneous freezing should be provided, or perhaps this discussion should be removed since heterogeneous freezing is not a part of the modeling results presented in this paper.

Page 620. With regard to discussion of the deposition coefficient, please explain what you mean by most models work well with a deposition coefficient greater than 0.1. As I understand it, varying this parameter can have a very large effect on the number of ice crystals generated during homogeneous freez-

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ing and on the humidity evolution. In a recent paper, we use atmospheric observations and simple modeling to constrain the deposition coefficient to be greater than 0.1 for small ice crystals during homogeneous freezing (Kay and Wood, in review for publication in *Geophysical Research Letters* Available at: http://www.cgd.ucar.edu/cms/jenkay/papers/KayWood_revised_Feb2008.pdf).

Page 628. It is not clear why simulations with Na=10,000 and neglecting the shift in the mean mass of the aerosol size distribution are being performed. Although this is described later (page 632), I am still confused.

Page 629, line 12. It would be useful to provide a quantitative comparison or a difference plot, especially since the comparison is plotted on a log-log plot.

Page 629-630. The results at large vertical velocities are very different than the Karcher results; however ice crystal number densities of 1000 cc are not observed in the atmosphere at cold temperatures (e.g., Peter et al., (2003 ACP)).

Page 631. Given that pressure and temperature co-vary in the atmosphere, are the described sensitivities to pressure variations reasonable for typical atmospheric conditions?

Page 631. Does the sensitivity to aerosol size distribution width change when the deposition coefficient is reduced (e.g., to 0.1)?

Page 632. Would it be possible to show the time step required for a certain level of accuracy as compared to Karcher instead of Figure 13?

Page 635, line 5-7. This sensitivity to shape is interesting. Are hexagonal columns with the assumed aspect ratios in your model typical of what is observed in the atmosphere?

Page 636, The atmospheric relevance of the explored vertical velocity forcing should be discussed. Is it reasonable to assume that the entire atmospheric column will be lifted uniformly at 10 cm/sec?

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Page 637, Several timescales are mentioned here, but they are not defined or quantified. It would be interesting to take this one step further and investigate the competing timescales to see when transitions between different vertical structure regimes occur.

Page 638, line 1. Please justify why these fluctuations make the model more realistic. Are they based on observations under typical cirrus cloud conditions?

Page 638, Lines 25-30. This is a really interesting result. Can you quantify it?

Page 639, Lines 10-12. Again, can you justify that this is a reasonable amount of shear for the upper troposphere during typical cirrus cloud formation?

Page 641, Kay et al., (2006 - JGR) estimated that vertical velocity fluctuations only affect nucleation when fluctuation timescales are shorter than fallout timescales, but longer than ice crystal growth timescales.

Page 642, Line 5. Clearly, box model simulations cannot treat sedimentation or resolve vertical structure in the same way that a 2D or 1D model can. That being said, a lot of insight can be gained from simple models. For example, the importance of sedimentation timescales in dictating the microphysical and humidity evolution has been demonstrated using simple box models and timescale ratios (see Kay et al., (2006 - JGR)).

Page 643, line 13-15. A comment 8211; We have also found that aerosol depletion is important in our box modeling looking at aerosol sensitivity during homogeneous freezing (Kay and Wood, in review for publication in *Geophysical Research Letters*).

Editorial comments/typos:

I would reword phrases such as *Note that*, *strongly believe*, *it turns out*, or *gave good results*. Discussion using these phrases seems a bit too colloquial or nonspecific for a scientific paper.

Page 603, Lines 5-14. I would recommend streamlining this discussion to motivate the

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work presented here. I would also recommend including citations to previous work such as Hoyle et al. (2005), Karcher and Strom (2003), Jensen et al. (1994) or including an e.g. when citations are providing an example of a sensitivity of cirrus clouds to vertical velocity variability.

Page 612, line 8; Page 617, line 14, etc. What is *ansatz*?

Page 617, lines 10-12. I have not seen part 2. Is it appropriate to cite a paper here that has not already been submitted?

Page 636, *updraught* should be changed to *updraft*

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